Ballot structure and political selection. Evidence from changes in electoral rules☆

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ABSTRACT

This paper studies the effect of different ballot structures in proportional representation systems on the educational attainment of elected politicians as a proxy for their "ability". By exploiting the Italian electoral reform that switched from open to blocked lists of candidates, the difference-in-differences analysis shows that the reform lowered the educational attainment of national politicians compared to the counterfactual (regional politicians). The dynamic impact of the reform is persistent over time, and it is robust to several concerns. The estimates account for the changes in the party system due to the "Clean hands" judicial inquiry against political corruption that began in 1992. The voters' choice of *better* politicians than political parties seems to be the most likely mechanism behind the results.

1. Introduction

Parliamentary democracies rely on the interaction between electors and political parties during elections. Politicians pursue their own particular interests as well as the general interests of the community; those interests often differ and conflict. The public interest can be achieved in two ways: first, by providing the right incentives to politicians; second, by selecting good politicians. This paper focuses on the latter approach: political selection is of the utmost importance because those elected are responsible for instituting and implementing policies in the interest of the society as a whole (Besley et al., 2011).

Democratic elections are the primary instrument for selecting politicians. They shape the recruitment of good politicians by affecting candidates' decisions to run for office (Caselli and Morelli, 2004; Mattozzi and Merlo, 2008) and the parties' selection of candidates (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015). Elections are governed by laws that define the characteristics of the electoral system in terms of *district magnitude* (the number of politicians elected in a district), *electoral formula* (how votes are translated into seats), and *ballot structure* (the voting schemes). The political economy literature largely deals with the impact of the first two of those characteristics on the recruitment of politicians (Besley, 2005; Myerson, 1993) and on policy outcomes (Persson et al., 2016), typically comparing proportional and majoritarian systems (Galasso and Nannicini, 2017; Besley and Preston, 2007). This paper instead contributes to the literature on the effect of different ballot structures within proportional representation (PR) on political selection, comparing "preference" and "non-preference" voting schemes. In the former, voters can cast votes for their preferred candidates (so, the ranking of elected politicians depends on the number of votes each candidate receives from electors).

This is PR with *open* lists of candidates. In the latter, voters by contrast vote only for a party symbol that refers to a *blocked* list of candidates drawn up by the party (the leaders decide which candidates will enter parliament by placing them at the top of the list - PR with blocked lists) (Bowler and Farrell, 1993; Farrell, 2011).

The ballot structure can affect the selection of politicians through the relative power it accords to voters and party machines. With open lists, elected politicians should feel more accountable to the electorate than to the party: they have a stronger incentive to promote policies in the interest of voters in order to win re-election (Persson et al., 2003). On the other hand, this system may also encourage corruption through vote trading or buying (Chang and Golden, 2007) resulting in policies favoring selected interest groups. Instead, PR with blocked lists, while rewarding party loyalty rather than voter consensus, mitigates the fratricidal struggle within political parties reducing electoral campaign expenses of the single candidate (higher electoral campaign costs might favor only the wealthy at the expenses of quality).

On the basis that there is not an unambiguous prior on the direction of the causal effect of the ballot structure on politicians' quality and on the reasonable assumption that there is a positive relationship between educational attainment and a politician's ability (Kotakorpi and Poutvaara, 2011), in this paper we want to shed light on the effect of the Italian Parliamentary electoral reform of 1993 (which switched from open to blocked lists) on the education of politicians. We apply a difference-in-differences (DiD) framework, identifying national parliamentarians as the "treatment" group and regional councilors as the "control" group. We exploit two successive parliamentary electoral reforms (Laws 276-277/1993 and Law 270/2005), which instituted PR with blocked lists in place of PR with open lists (Laws 6/1948 and 29/1948). Over the period 1983–2014, the ballot structure with preferences prescribed by the regional council election laws (Law 108/1968 and Law 43/1995, both proportional with open lists) remained unchanged. We observe the education level of politicians before and after the treatment (the electoral reforms), which is independent of individual ability, and we estimate the causal effect of the different ballot structures in PR on the educational attainment of elected politicians.

While National and regional elections naturally differ, regional councilors nevertheless seem to be an appropriate counterfactual. Indeed, while members of parliament (MPs) have greater responsibility and prestige than regional councilors, many Italian parliamentarians have been members of local governments, which can be thought of as a kind of training ground providing useful opportunities to gaining consensus in local communities. The main identification hypothesis of the DiD methodology, namely the parallel trends in the average education of parliamentarians and regional councilors before the reform, finds robust statistical support in the data through the estimation of a fully flexible DiD model and the conducted test by Mora and Reggio (2019). Moreover, to corroborate the choice of regional councilors as a "good" control vis-à-vis the treated parliamentarians, we also restrict the sample of deputies who in the past were regional presidents or councilors.

We compare the education level between the groups of parliamentarians in different legislatures and regional councilors elected before and after the reform, performing an empirical analysis over the time-span 1983–2014. Given that Laws 276-277/1993 prescribed a mixed electoral system where only 25% of deputies were named under proportional representation with blocked lists and 75% of deputies and all senators were elected under a majoritarian rule, in our main analysis we consider deputies only.

We find that the introduction of the blocked lists resulted in deputies with lower education level than under open lists, vis-à-vis the counterfactual scenario. Specifically, the reform lowered the probability of electing university graduates deputies by between 12 and 18 percentage points. Considering the actual number of years of education, the reform led to a decrease in education by more than one year in the treatment than in the control group. To complement our baseline analysis, we also examine senators, finding a sharper reduction in their education (in this case we compare Laws of 1948 and Law 270/2005 only) than in that of deputies. We also improve the estimation of the effect of the reform on the education of elected politicians by adding several regional control variables.

We explore the plausibility of our identification strategy by estimating a fully flexible dynamic model (the event-study estimates) including the interactions of the groups dummy (treatment/control) with each of the pre- and post-reform time dummies. Following the approach of Mora and Reggio (2019), we find statistical support for the crucial assumption of parallel outcome trends among treatment and control groups prior to the reform. The dynamic impact of the reform further suggests that the lower education of parliamentarians elected with blocked lists is persistent over time.

We also investigate the gender effect of the reform, finding that women politicians were not affected, probably due to the negative stereotypes about women as policy-makers; thus, the effect is driven by the election of low-educated men.

At the time of the first electoral reform (Laws 276-277/1993) the Italian party system itself changed: the old parties disappeared and new ones were born as a consequence of a sweeping judicial investigation into political corruption known as "Clean hands" (Mani Pulite). Citizens' hostility to the misconduct of the old parties led the new parties to engage in electoral competition by fielding more honest candidates (Chang et al., 2010; Nannicini et al., 2013) and, presumably, better educated ones. Or, on the contrary, if corrupt politicians are more educated and voters turned against more corrupt politicians after "Clean hands", this could lead to a decline in the education of parliamentarians after 1992. Thus, the effect of "Clean hands" could confound our estimate of the effect of the Parliamentary reforms on the educational attainment of politicians. Our analysis addresses this concern by estimating a relationship between the educational attainment of deputies and an index of their misconduct (finding a negative link) and introducing dummy variables for the biggest political party in each national and regional election (thus distinguishing between the evolution of the party system as such and the effect of the introduction of blocked lists of candidates).

The reduction in deputies' education is more pronounced when the sample is restricted to deputies elected in regional councils in the past. Moreover, to address the concern that the strengthen in fiscal federalism in Italy might have made subnational level of government more attractive, thus confounding the results, we control for the Constitutional reform of 2001.

The findings are robust to a further measure of politicians' ability, namely a dummy for a skill-intensive occupation, as well as to comparison between "pure" proportional systems, and to the 1995 law reducing the number of preferences at regional elections. This robustness suggests that our results are unlikely to be driven by confounders or reverse causality.

The paper provides new evidence that education of elected deputies diminishes with respect to the control group when electors' ballots change from open to blocked lists. There are two possible mechanisms in voter or party behavior that may be responsible for this effect. Do the findings imply that voters are better able to select "good" politicians than parties? Or are they consistent with an alternative interpretation that voters can observe only a restricted set of candidates' attributes (e.g., gender, age, education), while parties have a larger set of information and use different criteria to select political candidates? To disentangle these competing explanations, information on candidates at national and regional elections are needed. Unfortunately, data on candidates' characteristics are not available and cannot be collected. However, the estimated negative link between the misconduct of politicians and their education level, offers statistical support to the argument that the most likely driver of our results is voters' choice in the ballot box rather than the parties' choice of candidates in the lists. Indeed, after 1992 Italian parties tried to gain electoral consensus by selecting better, more educated, candidates in their lists. Hence, the negative effect of the electoral reform reasonably cannot be ascribed to party choices but to voters choices.

1.1. The literature

Within the general framework of the studies analyzing the mechanisms affecting the quality of politicians, this article contributes to the empirical literature on the effect of the two ballot structures for PR (open vs blocked lists) on political selection, focusing on politicians' education. The crucial difference between the two voting schemes relates to the rationale of voters and political leaders in choosing candidates. The central premise for voters is that they agree that competent politicians are more desirable. Thus, they vote for the candidates they consider capable of implementing policies in the public interest. Political leaders, on the other hand, tend to rank candidates according to criteria that do not always coincide with those important to voters. Leaders whose aim is power may not consider candidates' ideological commitment to the party or how well they represent voters' interests. Instead, they tend to choose candidates who, once elected, are likely to follow the leader's guidelines (Besley, 2005).

Hence, a trade-off emerges in solving the puzzle of the effect of ballot structures on the quality of the politicians elected. If accountability determines who has the power to select politicians (political parties or voters), we expect voters to be more effective in selecting higher quality politicians. Indeed, under blocked lists, elected politicians are likely to feel more accountable to the party leadership than to voters; in fact, their political future depends on the party, not the electorate. Hence, politicians do not always promote policies in the interest of voters. Conversely, where voters can directly select their preferred candidate, politicians are more accountable to the voters who can reward competent politicians with re-election and punish the incompetent by denying their subsequent elections. This can lead to an elected body more inclined to promote effective policies in the interests of the community.[1] In this framework, we expect preference voting schemes to be a better mechanism than blocked lists for selecting higher quality politicians (Persson and Tabellini, 2003; Norris et al., 2004).

However, there are also some disadvantages of PR with open lists that may reflect in lower educated politicians than in closed lists. It may be, for instance, that candidates on an open list act more in the interest of their voters but not in the interests of society or the community as a whole (Carey and Shugart, 1995; Ashworth and Mesquita, 2006; Grimmer et al., 2012). Indeed, open lists encourage consortiums and trading or buying votes, thus leading to corruption (Chang, 2005; Chang and Golden, 2007) and partitocracy, which can lead to policies in the interest of voters but not the country, region, or municipality. Moreover, preference voting can lead to fratricidal struggle within political parties, as candidates must collect more preferences than others in their party (Blumenau et al., 2017), and it can encourage candidates to campaign not only for their party but above all for themselves, driving up electoral campaign costs. These problems with open lists might go at the expenses of "quality" and can be presented as a rationale for blocked lists.

Thus, the ballot structure is an important driver of political selection and there is not an unambiguous prior about its effects on politicians' ability.

The greatest challenge that studies on that topic face is the measure of politicians' quality. Based on the assumption that the education level is a good measure of politicians' ability (widely use in the empirical literature — see Baltrunaite et al., 2014; Daniele and Geys, 2015; De Paola and Scoppa, 2011; Daniele, 2019; Baraldi et al., 2022) and being aware of its weaknesses, this paper contributes to a growing literature on the selection of politicians,[2] with a focus on a little-emphasized aspect of PR and enriching the work of Galasso and Nannicini (2015), which shows that, in a blocked list system, parties optimally allocate low-quality politicians to safe and high-quality politicians to contested seats. This result is corroborated by De Paola and Scoppa (2011), who find that political competition in Italian local elections is correlated with the quality of mayors and municipal councilors. Comparing the behavior of politicians elected in single-member majoritarian districts with that of those elected under PR, Gagliarducci et al. (2011) find that the majoritarian system increases the frequency of geographically targeted bills and reduces the rate of absenteeism. In the same scenario, Galasso and Nannicini (2017) find that when the number of competitive districts increases, the majoritarian system becomes more effective; the reverse holds when safe districts are the majority.

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[1] Carey and Shugart (1995) explain that candidates have strong incentives to distinguish themselves from others in their party when the vote is cast for a candidate rather than a party.

[2] Other studies have documented the effect of open vs blocked list PR on vote and seat shares in an international comparative framework scenario (Bergman et al., 2013; Nemoto and Shugart, 2013).

The way electoral systems affect the quality of elected politicians has also been investigated in the light of political corruption (Chang, 2005; Chang and Golden, 2007; Golden and Picci, 2008). Very recently (Hangartner et al., 2019), studying the unique PR system of Colombia (where local parties can use open or blocked lists in municipal elections), find that with open lists parties can increase their vote share, incentivize all candidates to campaign, and attract more candidates with higher experience. Studies on Italian municipalities have found that politicians' quality is affected by gender policies, such as quotas (Baltrunaite et al., 2014), and by organized crime (Daniele and Geys, 2015; Daniele, 2019). Finally, Folke et al. (2016) shows how political parties may have an incentive to promote preference votes among their politicians; parties use candidates' performance in attracting preference votes in making subsequent decisions on career advancement.

Dealing with the challenge in measuring politicians' quality another relevant issue is the relationship between honesty and skill. Although education and skills are widely employed in the empirical literature, in some cases they may be emphasized at the expense of honesty. That is, higher education in *political careers* is probably not "better" than *career politicians* in terms of motivation, honesty, and commitment to public organization (Mattozzi and Merlo, 2008). Therefore, as regards the *quality* of politicians, one may run into a combination of "adverse selection" (fixed characteristics such as education and skills) and "moral hazard" (the effort politicians make once elected as honesty); both aspects could be affected by the electoral reform. We address this issue in the interpretation of our results following Fedele and Naticchioni (2016), performing our analysis on the most motivated *public-fit* rather than *market-fit* politicians. Our results are confirmed, indicating that the effect of the reduction in politicians' education and skills owing to the introduction of blocked lists can be interpreted, with due caution, as a reduction in their "quality".

The rest of the paper is organized as follows. Section 2 describes the Italian institutional framework, variables, and data. Section 3 presents the empirical strategy. Section 4 shows the results and discusses the possible mechanisms behind results. Section 5 performs some robustness checks and Section 6 concludes. Further results of robustness checks are in Appendix A, while Appendix B presents additional tables and figures.

2. Institutional framework, data and variables

2.1. Institutional framework

Italy has different electoral systems disciplining the election of representatives at various levels of government. Here we are interested in the national and regional electoral laws. The empirical analysis refers to the parliamentary election system governed respectively by: (1) Laws 6/1948 and 29/1948; (2) Laws 276/1993 and 277/1993; and (3) Law 270/2005. Laws 6/1948 and 29/1948 were in force until the 12th Legislature (1994). Under that system, MPs were elected under open list PR in large districts. Voters could express up to four preferences for deputies and one for senators in single-member districts. We call this system "proportional with preferences".

The electoral system with blocked lists was introduced by Laws 276/1993 and 277/1993, the "Mattarella Law", which instituted a mixed electoral system: all the members of the Senate and the 75% members of the Chamber of deputies were elected by majority vote in single-member districts (candidates were elected in local constituencies). The remaining 25% of deputies were elected under proportional representation with blocked lists. For the Chamber voters were given two ballots: one to vote for a candidate in their single-member district, one to vote for a party list in their larger proportional district. The rationale for the majoritarian approach was to reinforce the power of voters, that for the proportional portion was to ensure the representation in Parliament of political minorities and small parties.

Law 270/2005, the "Calderoli Law", implemented a PR system with blocked lists for all members of parliament, while also introducing a majority bonus and a minimum threshold to avoid the dispersion of votes.[3] The main justification for this reform was reduction in political competition to overcome the sharing of votes among candidates caused by the previous proportional electoral rules.

We call the system with blocked lists under Laws 276-277/1993 (only for 25% of the House of Representatives) and Law 270/2005 as "proportional with no preferences".

"Proportional with preferences" and "proportional with no preferences" prescribe different ballot structures: the former allows up to four preferences for candidates; the latter mandates a vote solely for a list of candidates drawn up by the political parties. Table 1 summarizes the main characteristics of the national electoral laws we refer to.

Law 108/1968, a proportional electoral system where voters could express up to three preferences, applied to regional council. It remained in force until 1995, when Law 43/1995, the "Tatarella Law", was introduced, instituting PR for the election of 80% of the councilors with a majority bonus (for the remaining 20%), and allowing only one preference.[4] During the period under analysis,

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[3] The way the bonus was awarded is the main difference between the Chamber and the Senate. In the Chamber, the national territory is divided into 27 constituencies and the allocation of seats is at national level. To be represented, a party must achieve the threshold of 4% (10% for coalitions). Each party wins a number of seats proportional to the number of votes. If none achieves 55% of the seats, the most voted coalition is awarded a majority bonus to give it 340 seats. The majority bonus is allocated between the parties of the coalition according to the number of votes achieved by each list. For the Senate, the allocation of seats is at a regional level. Thus, in each region, the party or coalition with a plurality without achieving 55% of seats was entitled a majority bonus in order to reach that percentage of seats. The thresholds required were 8% for a single party, 20% for a coalition and 3% for each list within a coalition.

[4] According to the mixed system, in order to elect the regional council, the elector casts two votes: the first to elect 80% under PR based on the lists presented at the provincial level (voters can express a preference for a candidate within the list); the second, the remaining 20%, represents the majority bonus, awarded to a "listino bloccato" (blocked lists) at the regional level (in which it is not possible, therefore, to express preferences), linked to the candidate for regional president. Party that obtain less than 3% of the vote gets seats unless they are linked with a presidential candidate who gets at least 5% of the votes. The coalition supporting the winning candidate for president is awarded a bonus of seats, thus ensuring a majority in the regional assembly. The bonus is then redistributed amongst the parties of the winning coalition. Law 43/1995 allows the elector to express only one preference.

Table 1

Electoral systems and ballot structure of national electoral laws.

	Proportional with preferences	Proportional with no preferences	
	(**Laws 6/1948-29/1948**)	(**Laws 376/1993-277/1993** and **Law 270/2005**)	
Electoral system	Proportional with open lists	**Laws 376/1993-277/1993** Proportional with blocked lists for 25% of the Chamber	**Law 270/2005** Proportional with blocked lists
Ballot structure	Chamber: up to 4 preferences Senate: 1 preference	Chamber: no preferences	Chamber: no preferences Senate: no preferences

Table 2

Electoral systems and ballot structure of regional electoral laws.

	Law 108/1968	Law 43/1995
Electoral system	Proportional	Proportional system with a majority bonus for the winning coalition
Ballot structure	Up to 3 preferences	1 preference

the regional electoral law remained unchanged: a PR system with open lists and preferences. Table 2 shows the main characteristics of the regional electoral laws we refer to.

The data are those provided by the Ministry of the Interior on parliamentary and regional politicians. Specifically, to compare the ballot structures under "proportional with preferences" and "proportional with no preferences", the baseline analysis is for deputies only (as noted, under the "Mattarella Law" senators were elected by a majoritarian criterion, so during that three Legislatures we would no have senators within the dataset).[5] Regional elections are not held at the same time as national elections. Table 3 shows the parliamentary and regional elections that we consider.

The empirical analysis compares Legislatures IX and X (for parliamentarians) and the corresponding regional elections from 1984 to 1993 before the national reform of 1993 and Legislatures XII, XIII, XIV, XV, XVI (for parliamentarians) and the regional elections from 1995 to 2014 after the reform. We indicate legislatures/elections with an index t from 1 to 6 where $t = 1, 2$ comprises those prior to the reform and $t = 3, 4, 5, 6$ refers to those after the reform. Given that Legislatures XII (1994) and XIII (1996) are so close, we collapse them into $t = 3$ (see Table 3).

Even though the electoral law of 1948 (*proportional with preferences*) also affected Legislature XI (in April 1992), we exclude it for two reasons: (1) the parliament elected in 1992 produced the electoral reform we refer to, so excluding that Legislature rules out possible problems of endogeneity (see Section 3); (2) it falls in the period of "Clean hands", which almost certainly altered the behavior of parties and voters alike.[6]

In this last regard, the political and legal context during the years of the electoral reforms is crucial in justifying the causal effect of the reforms on the average education of parliamentarians. Laws 276-277/1993 were the response of the traditional Italian party system to the public outrage over the corruption scandals revealed by "Clean hands", Italy's broadest judicial campaign against political and bureaucratic corruption.[7]

One of the main consequences of "Clean hands" was that even major political parties disappeared, including the Christian Democrats and the Italian Socialist Party, and new parties were born. After "Clean hands" public opinion expressed broad support for the judges fighting corrupt politicians and strong disapproval of the parties' conduct. The new party system reacted accordingly, excluding from their lists most of the politicians prosecuted for corruption and, reasonably, choosing politicians with both higher moral and educational standards in order to public gain credibility. In what follows the estimated models account for the evolution of political parties in the period.

2.2. Data and variables

Measuring politicians' quality is challenging because there are no direct measures. Besley et al. (2017) and Dal Bó et al. (2017) get a measure that is likely to reflect personal ability by constructing an earnings score based on education and occupation. Unfortunately, there is no publicly available earnings information for Italian politicians. Thus, following the relevant theoretical (Bó et al., 2006; Besley and Reynal-Querol, 2011; Galasso and Nannicini, 2011; Fortunato and Panizza, 2015; Kotakorpi and Poutvaara, 2011; Glaeser et al., 2004) and empirical literature (De Paola and Scoppa, 2011; Baltrunaite et al., 2014; Daniele and Geys, 2015; Baraldi et al., 2022) that consider education level as a good proxy for politicians' ability, by and large and with due caution, our analysis can be interpreted in terms of politicians' quality.

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[5] We perform estimations on the sub-sample of senators as robustness evidence.

[6] A further reason could be that the Italian referendum of 1991 modified the electoral law, allowing only one preference for the election of deputies rather than the previous four.

[7] The "Clean hands" investigation began in February 1992. Its consequence was the end of the "First Republic" represented in Legislature XI (1992–1994) by the so-called "Parliament of the indicted".

Table 3
Parliamentary and regional elections.

t - Reform	Parliamentary Elections	Law	Regional Elections	Law
t=1 - Before	Leg IX (1983)	6/1948-29/1948	1984, 1985, 1986, 1988	108/1968
t=2 - Before	Leg X (1987)	6/1948-29/1948	1989, 1990, 1991, 1993	108/1968
t=3 - After	Leg XII (1994)-Leg XIII (1996	276/1993-277/1993	1995, 1996, 1998, 1999	43/1995
t=4 - After	Leg XIV (2001)	276/1993-277/1993	2000, 2001*, 2003, 2004	43/1995
t=5 - After	Leg XV (2006)	270/2005	2005**, 2006, 2008, 2009	43/1995
t=6 - After	Leg XVI (2008)	270/2005	2008***, 2010, 2011, 2013, 2014	43/1995

Notes: Basilicata ,Calabria, Campania, Emilia Romagna, Lazio, Liguria, Lombardy, Marche, Piedmont, Puglia, Tuscany, Umbria, Veneto voted in 1985, 1990, 1995, 2000, 2005, 2010; Abruzzo voted in 1985, 1990, 1995, 2000, 2005, 2008; Friuli Venezia Giulia voted in 1988, 1993, 1998, 2003, 2008, 2013; Molise voted in 1985, 1990, 1995, 2000, 2006, 2011; Sardinia voted in 1984, 1989, 1999, 2004, 2009, 2014; Sicily voted in 1986, 1991, 1996, 2001, 2006, 2008; Trentino Alto Adige and Valle D'Aosta voted in 1998, 1993, 1998, 2003, 2008, 2013. *The Molise regional election of 2001 took place on 11 November 2001. The early election in 2000 was invalidated due to irregularities in the vote. **Basilicata did not vote along with the other Italian regions in the 3-4 April 2005 regional elections because of legal issues with the presentation of the list Social Alternative. It voted a couple of weeks later on 17–18 April 2005. ***The Abruzzo regional election of 2008 took place on 14/15 December 2008 due to the early resignation of President Ottaviano Del Turco after his indictment for alleged corruption.

No Italian institution provides comprehensive data on the years of education of deputies and senators, so we collected this information ourselves and built our own database. Our main source of information was the Ministry of Interior. For each parliamentary term, the ministry website shows the list of parliamentarians in office at the time, with their education level and some personal information such as date and place of birth, previous job, date of election, political movement, and so on.[8] Regarding the education of regional politicians, the Ministry of Interior supplied data on elections starting from the year 1984.[9] In particular, the data includes the highest qualification attained by each regional politician.

We translated the qualitative information on educational attainment of national and regional politicians into number of years of education. We upgrade the criterion of Baltrunaite et al. (2014), Daniele and Geys (2015) and De Paola and Scoppa (2011); that is, we match the qualification and the previous occupation of each politician to attribute a more appropriate number of years of education.[10] For example, if a politician reported being a specialized surgeon, we attribute 5 years of education above the 18 needed to hold a university degree. This gave us a range of variation in the number of years of education from 0 to 23.[11] In the baseline analysis, we consider two measures of quality: (1) a dummy variable taking the value of 1 for university degree and 0 otherwise (called *Degree*); (2) number of years of education (*Years of Education*).

Tables 4 and 5 show the descriptive statistics of deputies and regional councilors for *Degree* and *Years of Education*. Overall, the educational level of regional politicians is lower than that of national politicians, probably owing to higher opportunity costs for regional politicians which may discourage the more educated from entering politics.[12] The data shows that the average percentage of deputies with a degree decreased sharply after the electoral reform of 1993, falling from 80% to 70%. The decrease in number of years of education too was significant, corresponding to approximately five months of education. The percentage of regional councilors with a degree shows no significant difference over time, nor does the average number of years of education.

To justify the choice of regional councilors as the control group, note that a substantial share of deputies and senators were former regional and municipal councilors. The data collected by Gagliarducci et al. (2011) shows that of the parliamentarians elected in six elections from 1987 to 2006, 54.25% had served as councilor or president in regional, provincial, or municipal local governments; 16.74% of the deputies in our database having served as regional president or councilor. Hence, we can argue that Italian professional politicians start their career in local government and advance to a higher institutional level (in parliament) with better background knowledge.[13] Moreover, Italian parliamentary elections occur with a breakdown of constituencies at regional (Senate) and sub-regional level (Chamber of Deputies), corresponding to a region, where candidates are called on to represent the interests of their constituency (candidates in regional elections too are expected to represent the interests of people living within the region at regional council). Hence, first, the same group of electors – at roughly the same time – have to choose parliamentarians as well as regional councilors; secondly, given that candidates in national and regional elections both alike aim to represent the interests of people living in the region, we can ignore the fact that voters could theoretically make different choices at national and regional elections. In order to further corroborate the suitability of regional councilors as the control group, in the empirical analysis we restrict the treatment group to deputies who have had past experience as regional councilor or president. In this way, we perform the analysis on two roughly homogeneous groups.

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[8] Data on the politicians elected in 1983 was drawn from Fondazione Rodolfo Debenedetti and from the publication La Navicella (*I Deputati e Senatori del IX Parlamento Repubblicano*, (1983), La Navicella, Editoriale Italiana, Roma). We also collected information on the members of the Italian parliament elected in 1987 from the database used by Gagliarducci et al. (2011).

[9] http://amministratori.interno.it/AmmIndex5.htm No information on regional politicians is provided before 1984.

[10] These authors measure education simply as the minimum number of years necessary to obtain a certain degree, i.e., no education = 0 years; primary education = 5 years; lower secondary = 8 years; upper secondary = 13 years; university or more = 18 years.

[11] Table B.1 in Appendix B shows the approach used in the conversion.

[12] Both the earnings and the power of regional officers are lower than those of Parliamentarians.

[13] In their recent study on how electoral rules – majoritarian vs proportional – affect the representation of women in Italy, Profeta and Woodhouse (2018) use a counterfactual similar to ours, consisting of regional, provincial and municipal councilors.

Table 4
Descriptive statistics of deputies and regional councilors having a degree in the legislatures/elections.

Legislature	Deputies			Elections	Regional Councilors		
	Mean	St. Dev.	Obs.		Mean	St. Dev.	Obs.
IX (1983)	80.46	39.68	471	2nd before	59.25	49.16	1,087
X (1987)	82.24	38.25	518	1st before	60.30	48.95	1,063
XII (1994)	70.78	45.62	154	1st after	58.74	49.25	1,081
XII (1996)	70.51	45.74	156	2nd after	57.30	49.48	1,068
XII (2001)	74.00	44.01	150	3rd after	57.26	49.49	1,095
XV (2006)	70.06	45.83	618	4th after	59.11	49.18	1,081
XVI (2008)	69.30	46.16	619				

Notes. The table reports the descriptive statistics of the variable *Degree* (measuring the share of politicians having a university degree) for deputies and regional councilors. For deputies, the statistics are calculated for each Legislature from the IX to the XVI. For regional councilors, they are calculated on each election. Specifically, the 2nd election before comprises regional elections taking place in 1984, 1985, 1986 and 1988; the 1st election before comprises regional elections taking place in 1989, 1990, 1991 and 1993; the 1st election after comprises regional elections taking place in 1995, 1996, 1998 and 1999; the 2nd election after comprises regional elections taking place in 2000, 2001, 2003 and 2004; the 3rd election after comprises regional elections taking place in 2005, 2006, 2008 and 2009; the 4th election after comprises regional elections taking place in 2008 (only Abruzzo), 2010, 2011, 2013 and 2014.

Table 5
Descriptive statistics of the years of education of deputies and regional councilors in the legislatures/elections.

Legislature	Deputies			Elections	Regional Councilors		
	Mean	St. Dev.	Obs.		Mean	St. Dev.	Obs.
IX (1983)	17.37	3.51	471	2nd before	15.54	3.55	1,087
X (1987)	17.53	2.59	518	1st before	15.79	3.36	1,063
XII (1994)	17.05	3.19	154	1st after	15.74	3.29	1,081
XIII (1996)	16.46	2.64	156	2nd after	15.78	3.08	1,068
XIV (2001)	16.66	2.29	150	3rd after	15.93	3.19	1,095
XV (2006)	16.97	2.94	618	4th after	16.08	3.15	1,081
XVI (2008)	16.91	3.05	619				

Notes. The table reports the descriptive statistics of the variable *Years of Education* for deputies and regional councilors. For deputies, the statistics are calculated for each Legislature from the IX to the XVI. For regional councilors, they are calculated on each elections. Specifically, the 2nd election before comprises regional elections taking place in 1984, 1985, 1986 and 1988; the 1st election before comprises regional elections taking place in 1989, 1990, 1991 and 1993; the 1st election after comprises regional elections taking place in 1995, 1996, 1998 and 1999; the 2nd election after comprises regional elections taking place in 2000, 2001, 2003 and 2004; the 3rd election after comprises regional elections taking place in 2005, 2006, 2008 and 2009; the 4th election after comprises regional elections taking place in 2008 (only Abruzzo), 2010, 2011, 2013 and 2014.

Finally, Table 6 shows descriptive statistics on the average education level of our two groups of politicians. We calculate the averages using data on deputies and regional politicians elected before and after the reform (Legislatures and elections as in Table 3). The statistics confirm that the education level of regional councilors is, on average, lower than that of deputies before and after the reform. The change over time is negative for the treatment group, and the difference is significantly different from zero; specifically, the share of graduate deputies falls by 11.1 percentage points after the reform, while education decreases by 0.571 years (approximately seven months of education). Instead, the mean difference in the share of graduate regional councilors before and after the reform is not significantly different from zero; while that in their years of education indicates the opposite: after the reform, education level appears to increase.

As regards of the inclusion in the sample of the 25% of deputies elected by proportional representation under Laws 276-277/1993, concern may arise over a possible composition effect at party level affecting the allocation of candidates across the two subgroups of deputies (those competing in single-member and those competing in proportional districts). Accordingly, we perform a mean difference test on the average education level between the two sub-groups of deputies in the three elections under the "Mattarella Law" (1994, 1996 and 2001). The results show that there is no significant difference in average education level .[14]

3. Empirical strategy

In order to identify the effect of the change in the ballot structure on politicians' education, we exploit the parliamentary election reform, Laws 276/277 of 1993. This represents an exogenous variation in the ballot scheme, which enables us to isolate the effect of the reform from time-specific effects – general trends in education level – that could drive the results. The regional electoral law remained unchanged in ballot structure, so, our identification strategy resembles a DiD framework in which we observe the education of each politician before and after the treatment (i.e., the national electoral reform), where assignment to the treatment group (i.e., the timing of the reform) is independent of politicians. In fact, the reform was passed by parliamentarians elected in Legislature XI (1992), which we exclude from our analysis, ruling out the possibility of endogenous treatment and strengthening the robustness of our DiD identification strategy.

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[14] Results are shown in Table B.3 in Appendix B.

Table 6
Descriptive analysis of *Degree* and *Years of Education*.

	Before	After	Difference
Deputies vs Regional politicians			
Degree			
Treatment group	0.814	0.702	0.111***
No. Obs	989	1,697	
Control group	0.597	0.581	0.015
No. Obs	2,150	4,355	
Difference	0.216***	0.120***	
Years of Education			
Treatment group:	17.449	16.878	0.571***
No. Obs	989	1,697	
Control group	15.662	15.890	−0.227***
No. Obs	2,150	4,355	
Difference	1.787***	0.988***	

Notes: The table shows the t-test on the average of dependent variables, *Degree* and *Years of Education*, in the treatment and control groups of politicians (respectively, deputies and regional councilors) before the reform (from 1983 to 1993) and after (from 1994 to 2014) (see Table 3). The following symbols indicate different significance levels: *** - significance at 1% - ** significance at 5%- * - significance at 10%.
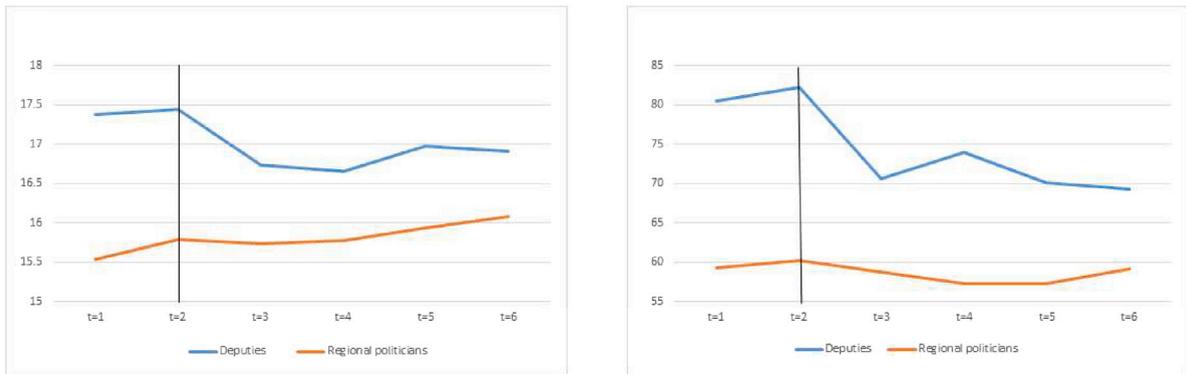
(a) (b)

Fig. 1. Trend in the *Degree* and *Years of Education*.
Notes. In Fig. 1(a) the vertical axis is the percentage of politicians having a degree; in Fig. 1(b), the average number of years of education. On the horizontal axis $t = 1 - 6$ is the index indicating the legislatures/elections considered (as in Table 3). The black vertical line in both graphs divides the pre and post-reform periods.

We therefore identify MPs (deputies) as the treatment group (exposed to the change in the ballot structure) and regional politicians as the control group. We compare the change in education across the two groups in the elections before the reform (Legislatures IX and X) and after it (Legislatures XII–XVI). In this way, we can make inferences on the effect of voter preferences on the education of elected politicians. The exogenous nature of the shock means that the treatment and the control group are unrelated to other unobserved characteristics affecting the dependent variable of our analysis.

Our key identification approach is based on the so-called "parallel trends" assumption: in the absence of treatment, the average outcomes for treated and control groups would have followed parallel paths over time. However, given that the evolution of outcomes for deputies in the counterfactual (untreated) state is undetectable, this assumption cannot be tested. In order to provide initial support for the assumption of a common flat trend in the average education level among the treated and the control group in the years preceding the reform, we statistically test the hypothesis that the difference in the means of both our measures of education (*Degree* and *Years of Education*) in the pre-reform period is not statistically different from zero. We find that we cannot reject the null that average educational attainment did not change for deputies and regional councilors elected before the reform. Figs. 1(a) and 1(b) offer further graphic evidence showing a similar trend in education level in the years preceding the electoral reform for the two groups of politicians.

We focus on two main outcomes: the probability of electing a graduate and number of years of education. We first estimate the mean impact of the electoral reform of 1993 on those outcomes, using the following specification:

$$Y_{irt} = \beta_0 Treat_i + \beta_1 Treat_i * Reform_t + \gamma_r + \delta_t + \epsilon_{it} \tag{1}$$

where Y_{irt} is the education level of politician i elected in region r, in electoral year t. *Treat* is a dummy taking value 1 for parliamentarians and 0 otherwise, allowing us to control for unobserved time-invariant characteristics that may differ across politicians in the two groups. *Reform* is a dummy for elections taking place after the reform. *Treat*Reform* is the interaction term between the two dummies; it measures the treatment effect of interest, namely the difference in the quality of parliamentarians and regional councilors before and after the reform. γ_r stands for region fixed effects that account for the characteristics common to politicians in the same region and is constant over time. δ_t are fixed effects for each election year[15] and control for differences across years that affect both treatment and control groups. Finally, ϵ_{it} is the idiosyncratic error term.

To test the parallel trend assumption formally, we take the most common approach in DiD literature investigating trends in outcomes for both treated and control groups in the periods before the reform through an event study analysis. That is, we study parallel trends and the full dynamic effects of the reform by specifying a general "fully flexible" model (as in Mora and Reggio (2019)), that includes the interactions of the group dummy (treatment/control) with each of the pre- and post-reform time dummies.[16] In order to investigate the effects of the reform on the outcomes of interest over time, we augment the model in Eq. (1) as follows:

$$Y_{irt} = \beta_0 Treat_i + \sum_{t=1}^{6} \nu_t \cdot Treat_i \times B_t + \gamma_r + \delta_t + \epsilon_{it} \tag{2}$$

where t from 1 to 6 is an index for legislatures/elections before the reform ($t = 1, 2$) and after ($t = 3, 4, 5, 6$) (as in Table 3). B_t is the complete set of dummy variables for each legislature/election. Because we are controlling for time-invariant characteristics of the treatment group, not all of the DiD coefficients are identified. Accordingly we impose $\nu_1 = 0$. The dynamic coefficients ν_t allow us to measure the effect of the reform for every legislature/election before and after it. This specification enables us to assess whether changes in the education level of politicians are temporary or durable, distinguishing between short-run, medium-run, and long-run effects.

We recall that we consider only the 80% of the regional councilors elected by the proportional method under the "Tatarella Law" and only the 25% of the deputies elected with blocked PR lists under the "Mattarella Law".

4. Baseline results

4.1. Mean effect

Table 7 presents the estimation of the mean effect of the reform on the main outcomes of interest as specified in Eq. (1). It focuses on the effect of the national electoral reform on the education level of deputies. In the first three columns, the dependent variable is *Degree*; in the last three, *Years of Education*. All columns report the estimates including the (electoral) years' fixed effects; in columns 2, 3, 5 and 6 we add regional FE and in columns 3 and 6, regional controls. The null hypothesis that one of the parameters of the model equals zero is tested using the wild cluster bootstrap methodology proposed by Cameron et al. (2008),[17] where clusters are the Italian regions.[18] In the following tables the bootstrapped p-values of the null hypothesis are shown in parentheses.

The coefficient of the *Treat* dummy is statistically significant and positive: this indicates that deputies are, on average, more educated than regional councilors. The coefficient of the interaction term *Treat*Reform* is statistically significant and negative in all columns, suggesting that the reform led to lower educated deputies. By educational degree, column 1 shows that the reform lowered the probability of having university graduate deputies by 18 percentage points compared to the change among regional councilors; controlling for regional dummies, this percentage moderates to −13.8. By years of education, after the reform the average education of deputies decreased by 1.40 and 1.03 years, or about 17 and 13 months less of education, respectively.

In columns 3 and 6 we include regional-varying controls to counter omitted-variable bias. If the inclusion of these controls does not affect the estimates of the coefficients (i.e., the impact on size and sign is limited), a causal interpretation of the results can be maintained. The control variables used are the log regional population, the regional education level defined as the ratio between university graduates and resident population aged 25 to 64, and the regional per capita GDP. The results are substantially unchanged in sign and significance of the treatment coefficient. The inclusion of these control variables affects the size of the estimated coefficient only slightly, supporting the causal interpretation of our results.[19]

Since our classification extends the possible number of years of education to 23, where the literature caps it at 18, one possible concern is that our results for *Years of Education* may be driven by outliers. We address this concern by showing that the distribution of the years of education for candidates in treated and untreated groups does not show the presence of outliers.[20]

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[15] We include in Eq. (1) electoral year FE for elections that took place in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010.

[16] The same general model has been estimated by Reber (2005), Benzarti and Carloni (2019) and Button (2019).

[17] Bootstrapped p-values of the t statistic have been calculated using the unofficial STATA command *boottest* by Roodman et al. (2019).

[18] Given the limited number of clusters (in our case, 20 regions), the usual cluster-robust standard errors estimates would be downward-biased (Cameron and Miller, 2015).

[19] The estimates of the effect of covariates show that years of education increase as regional education level increases. Instead, there is weak evidence that the wealth of the region has a negative impact on years of education. The population of the region seems to have no effect. None of the covariates affects the probability of electing graduate politicians. Coefficients are not reported.

[20] See Fig. B.1 in Appendix B.

Table 7
Mean impact estimates of the electoral reform.

Dep.Var.	(1) Degree	(2) Degree	(3) Degree	(4) Years of Education	(5) Years of Education	(6) Years of Education
Treat	0.241***	0.228***	0.211***	1.911***	1.916***	1.911***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.180***	−0.138***	−0.122***	−1.401***	−1.033***	−1.016***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	9161	9161	9161	9161	9161	9161
R-squared	0.027	0.049	0.049	0.038	0.066	0.067
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). All estimations include election year FE for elections in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010. Columns 2, 3, 5 and 6 add Regional dummies. Columns 3 and 6 add control variables: regional population (in log), regional education level and regional per-capita GDP; coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

As a first qualification of our results, we verify the baseline results by considering the other branch of the Italian Parliament, the Senate. As Table B.2 in Appendix B shows, before the reform the percentage of graduate senators was 89%, compared with 81% for deputies; years of education reveals the same pattern: senators had, on average, one more year of education. After the reform, instead,[21] both measures of the education level are almost equal for senators and deputies. The DiD estimation results comparing the education level of senators and regional councilors (displayed in Table B.4, Panel A, in Appendix B) show that the reform also affected the education of senators negatively and significantly. Looking at both measures of quality, the estimated effect is larger for senators than for deputies. Indeed, the reform lowers the probability of having graduate senators by 21 to 29 p.p. more than in the counterfactual. The magnitude of the decrease in years of education is in the range 1.06–1.52 (down approximately 12 to 18 months).

An initial observation of the results concerns the difference in the effect of the reform on senators and deputies, which is probably due to the different number of preferences allowed. Indeed, Laws 6-29/1948 prescribe two different preference schemes for parliamentarians: up to 4 preferences for deputies and 1 preference only for senators (see Table 1). In order to check for this aspect, we perform a test on the hypothesis that the difference between the treatment coefficients for senators and deputies is nil.[22] We get a *p*-value equal to 0.36 in the model where the dependent variable is *Degree*. The result is replicated with *Years of Education* as dependent variable: a *p*-value of 0.11. Thus, we can claim that the number of preferences allowed has no effect on the education of the politicians elected.

4.2. Dynamic impact

Table 8 presents the estimation results for the dynamic effect of the reform on the main outcomes of interest according to model in Eq. (2). The effect of the introduction of blocked lists of candidates on the probability of electing a graduate deputy was strong in the first year after the reform (Treat*B_3), equal to −9.32 p.p. (column 1); it then turned immaterial, but subsequently reaching −11 p.p. in the last electoral period (column 3). This pattern characterizes all the specifications in Table 8, columns 1 to 3.

Our findings show a substantial negative impact of the reform on *Years of Education* as well. Here, all the coefficients of the dynamic specification after the reform are negative and significantly different from zero. The decrease was immediate after the reform with a coefficient −0.81 (column 4) meaning a reduction of about ten months of education of deputies by comparison with regional councilors. The decrease is greatest in the last electoral period, at one year. Improving the dynamic specification by adding regional FE and regional control variables, the sign and significance of all the coefficients is preserved, while their magnitude (in absolute value) is slightly reduced.

The dynamic analysis confirms the negative impact of blocked lists on the education of politicians (as in the mean impact estimates) and also it suggests that the reduction in the education of parliamentarians elected under blocked PR is persistent over time.

We perform a Wald test for the null hypothesis of no dynamics in treatment effects after the reform under the model as in Eq. (2). The bootstrapped *p*-value of the test performed on specification as in column 3, Table 8, is 0.37 and that in column 6 is 0.10. Thus, whatever outcome variable is used, we accept the null of equal treatment impact at conventional levels. This result supports the validity of the results of model as in Eq. (1) (Table 7).

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[21] Recall that we have no information about the education level of senators in Legislatures XII, XII, and XIV, 1994, 1996, and 2001 respectively, because under the "Mattarella Law", they were elected in a majoritarian criterion. Thus, we can consider only Legislatures XV (2006) and XVI (2008) after the reform.

[22] We perform the test using the wild cluster bootstrap method.

Table 8
Dynamic impact estimates of the electoral reform.

Dep.Var.	(1)	(2)	(3)	(4)	(5)	(6)
	Degree	Degree	Degree	Years of Education	Years of Education	Years of Education
Treat	0.212***	0.208***	0.198***	1.823***	1.759***	1.672***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*B_2	0.00717	0.00857	0.00684	−0.0821	−0.0706	−0.0878
	(0.76)	(0.73)	(0.79)	(0.72)	(0.75)	(0.71)
Treat*B_3	−0.0932**	−0.0863**	−0.0882**	−0.811***	−0.752**	−0.774**
	(0.011)	(0.02)	(0.015)	(0.01)	(0.016)	(0.016)
Treat*B_4	−0.0452	−0.0399	−0.0318	−0.848***	−0.801***	−0.722***
	(0.25)	(0.30)	(0.43)	(0.00)	(0.00)	(0.00)
Treat*B_5	−0.0842**	−0.0801**	−0.0668*	−0.779***	−0.742***	−0.614**
	(0.03)	(0.04)	(0.10)	(0.00)	(0.00)	(0.013)
Treat*B_6	−0.110***	−0.104***	−0.0872**	−1.000***	−0.948***	−0.784***
	(0.00)	(0.00)	(0.012)	(0.00)	(0.00)	(0.00)
Observations	9161	9161	9161	9161	9161	9161
R-squared	0.026	0.048	0.049	0.036	0.066	0.067
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions showing the dynamic impact of the electoral reform estimated using Eq. (2) All estimations include election year FE for elections that took place in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010. Columns 2, 3, 5 and 6 add Regional dummies. Columns 3 and 6 add control variables: regional population (in log), regional education level and regional per-capita GDP; coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

(a) Degree (1)   (b) Degree (2)   (c) Degree (3)

(d) Years of Education (1)   (e) Years of Education (2)   (f) Years of Education (3)
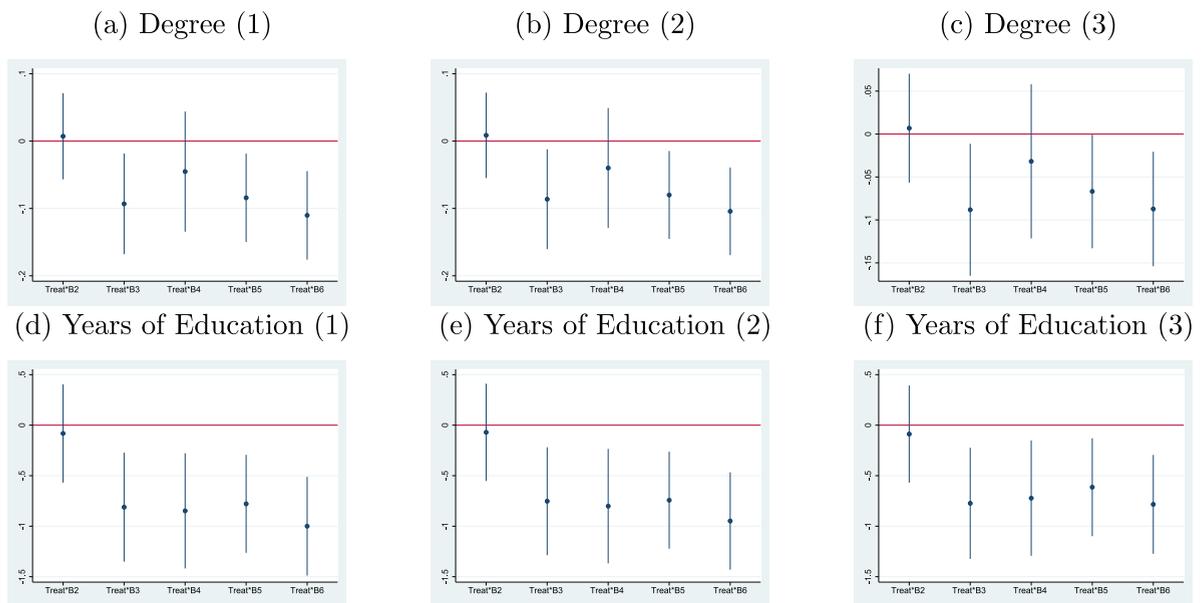
Fig. 2. Event study.
Note. The graphs report coefficients and confidence intervals estimated according to specification as in Eq. (2). Graph 2(a) refers to the estimates in column (1) Table 8; graph 2(b) to column (2), Table 8; graph 2(c), column (3), Table 8; graph 2(d), column (4), Table 8; graph 2(e), column (5), Table 8; graph 2(f), column (6), Table 8. Standard errors clustered at regional level.

Fig. 2 plots the 95% confidence intervals of v_t corresponding to all six columns in Table 8. The pattern shows that there is no evidence of differential pre-trends across the outcome variables, providing support for our identifying assumption that regional politicians are a good counterfactual for Parliamentarians. Specifically, the zero lead coefficients in the period prior to the electoral reform show that the two groups followed the same pattern over time. In addition, conditional on covariates (graphs 2(c) and 2(f)), the education of two groups was also identical in the pre reform period.

Overall, Fig. 2 indicates that the parallel trends assumption is satisfied in our DiD analysis, justifying a causal interpretation of our findings.

Table 9
Estimated effects under *Parallel-1* and *Parallel-2*.

	$t = 3$	$t = 4$	$t = 5$	$t = 6$
Dep. Var: Degree				
Parallel-1	−0.094	−0.048	−0.088	−0.112
s.e.	(0.037)	(0.044)	(0.032)	(0.032)
Parallel-2	−0.103	−0.065	−0.114	−0.147
s.e.	(0.058)	(0.090)	(0.116)	(0.147)
Dep. Var: Years of Education				
Parallel-1	−0.681	−0.730	−0.671	−0.877
s.e.	(0.245)	(0.263)	(0.214)	(0.215)
Parallel-2	−0.611	−0.589	−0.459	−0.595
s.e.	(0.408)	(0.634)	(0.852)	(1.092)

Notes. t indicates the legislatures/elections after the reform (t=3,4,5,6). Robust standard errors, clustered at regional level, in parenthesis.

Similarly, the graphs highlight the evolution of the outcome variables in the post-reform period, where the estimates turn negative and are statistically significant.

4.3. Parallel trends

The dynamic model (Eq. (2)) allows the test on parallel paths proposed by Mora and Reggio (2019). First, note that the coefficient of *Treat*B$_2$* in Table 8 is never significantly different from zero, confirming the parallel trend assumption before the reform. In the case of two pre reform periods, Mora and Reggio (2019) show that the effects of the reform can be estimated either assuming equal average change in outcomes in treatment and control groups (*parallel-1*), or assuming equal average acceleration (*parallel-2*), in the absence of the reform. The authors show that when both *parallel-1* and *parallel-2* hold, the common trend assumption is fulfilled. The results of the test,[23] presented in Table 9, refer to specifications 2 and 5 in Table 8. Table 9 shows the estimates of the treatment effect under *parallel-1* and *parallel-2* assumptions for each of the four post-reform periods ($t = 3, 4, 5, 6$). The estimates are often statistically significant with the expected sign, and they are all quite similar, as confirmed by the Wald test, which supports the presence of common pre treatment dynamics with a high p-value: 0.791 when the outcome is *Degree* and 0.773 for *Years of Education*.

4.4. Gender effect

In recent years a number of policy measures to increase women's participation in politics have been adopted, with the goal of legitimizing democracy and improving the quality of institutions (Epstein et al., 2005). On these grounds, a substantial strand of researchers in the social sciences have begun to analyze the effectiveness of such reforms in increasing women's participation in politics (for Italy see De Paola et al. (2010) and Baltrunaite et al. (2019)) and improving the quality of political institutions (Baltrunaite et al., 2014; Besley et al., 2017).

In this regard, it would be interesting to investigate whether the change in the ballot structure considered here affects the education of male and/or female politicians. First, the data shows a radical change in the gender scenario in the Italian parliament after the reform. The number of women doubled to 17.27% of the seats; their education level was slightly lower than that of their male colleagues.

Looking at regional councilors, the number of women also doubled in the two periods after the national reform with respect to the two periods before it (from 7.41% to 14.81%). We also witness a decrease in the percentage of both male and female graduate councilors before and after the reform, while years of education increased slightly. Splitting the sample of deputies by gender, the coefficient of the treatment variable is always negative but significant only for male deputies (Table 10); that is, the reform reduced the probability of electing a graduate male deputy by 13 p.p. and the deputies' years of education by more than a year with respect to regional councilors. Furthermore, among the treated deputies, the education level of elected women did not change after the reform compared to the control group. Accordingly, our results show that in the Italian political context, with the entry of a higher number of women than men, with no significantly different level of education (among women), the introduction of open lists lowered the average education level of parliamentarians because of the lower level of the men elected.

We advance a possible explanation linked to the presence of negative stereotypes about the ability of women in politics, which is one reason for the under-representation of women in politics, still widespread. Where prejudices exist that suggest women are "less able" policy makers, then in order to increase the involvement of women in politics, women must be "perceived" by voters and by political leaders to be better than their male counterparts. So, women must always enter the political arena with a consistently higher standard (in terms of education level), and this necessity is reflected in the lack of significant variation in women's level of education after the electoral reform.

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[23] We used the STATA unofficial command *didq* (Mora and Reggio, 2015).

Table 10

Mean Impact: Sample divided by gender.

Dep. Var.	(1)	(2)	(3)	(4)
	Degree (F)	*Degree (M)*	*Years of Education (F)*	*Years of Education (M)*
Treat	−0.0355	0.230***	0.582	2.033***
	(0.73)	(0.00)	(0.35)	(0.00)
Treat*Reform	−0.0677	−0.129***	−0.788	−1.045***
	(0.63)	(0.00)	(0.45)	(0.00)
Observations	1080	8079	1080	8079
R-squared	0.030	0.058	0.033	0.076
Regional FE	Yes	Yes	Yes	Yes
Electoral year FE	Yes	Yes	Yes	Yes
Regional Controls	Yes	Yes	Yes	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). "F" for "Female", "M" for "Male". All estimations include election year FE, Regional FE and control variables (regional population (in log), regional education level and regional per capita GDP); coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

4.5. Discussion of possible mechanisms

One interesting topic to deal with would be distinguishing between the two possible mechanisms responsible for the reduction in the education level of parliamentarians after the reform: voters' choice in the ballot box, and parties' choice of the list of candidates. By estimating the effect of the reform at national elections on the education of *candidates* at election, would enable us to determine what combination of voter' choice and nomination by party leaders led to the findings. Unfortunately, no information on candidates at regional and national elections is available, so we can only consider the lowering of the education level due to the change in the ballot structure.

However, the coincidence of the period of the electoral reform with that of "Clean hands" inquiry could help us to shed some light on which of the two mechanisms is the most likely. In order to do that, we exploit the dataset constructed by Miriam Golden (Golden, 2007). Miriam Golden and her co-authors (Chang et al., 2010) built a comprehensive dataset on members of the Italian Chamber of Deputies in the legislatures between 1948 and 1994. They concentrate on cases of misconduct by parliamentarians revealed by judicial requests for permission to proceed. With this data, we estimate the effect of malfeasance charges on the educational attainment of Italian deputies over all the available legislatures (1948–1994) and over the two elections just after the beginning of "Clean hands" (1992 and 1994).

The estimation results are shown in Table 11. As in the previous estimations, the dependent variable is years of education of deputies.[24] The regressor of interest is *Malfeasance charge*, a dummy that takes the value of 1 if the deputy has been charged for malfeasance and 0 otherwise. We control for the year of birth (*Year of birth*), a dummy for women deputies (*Sex*), the number of legislatures served (*Tenure*), the number of preference votes collected by the deputy at each electoral round, in log (*Preference vote*) and dummies for legislatures and for political parties.

In Panel A, Table 11, we show the estimation results over the time span 1948–1994 of five model specifications that differ in the controls included. They all highlight a significant negative coefficient of *Malfeasance charge*, pointing out a strong negative association between education and misconduct. The results in Panel B, for the elections in the "Clean hands" years, show a coefficient of *Malfeasance charge* statistically not different from zero everywhere in Table 11.

Looking at a longer time span, as in Panel A, or just over the two legislatures affected by "Clean hands", as in Panel B, the evidence is against the hypothesis that "Clean hands" caused a decline in educational attainment levels. This evidence suggests that political parties after "Clean hands" (and, at the same time, after the electoral reform) were not inclined to candidate less educated politicians than before, in order to be credible for voters. Then, our diff-in-diff results of a lower education level after the reform can be more likely due to the voters' behavior rather than to choice of parties: through the preference vote (available under PR with open list of candidates), electors are able to select "better" politicians than political parties.

The discussed evidence allows us also to strength the level of education as a proper measure of politicians' ability. Indeed, if we agree that voters prefer competent politicians, the positive and significant coefficient of *Preference votes* in Table 11 strongly suggests that voters find better educated politician to be the deputy's more competent. Moreover, if corrupt politicians are perceived as bad ones, the findings also support the assumption of educational attainment as an indicator of politicians' quality.

5. Robustness checks

In this section we present the results of robustness checks of the main estimates. Firstly, we take into account the change in the Italian political system due to the 1992 judiciary inquiry against political corruption called "Clean hands". Secondly, we restrict the

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[24] In the (Golden, 2007) database the years of education are caped at 20 for politicians with a PhD.

Table 11

Charge of malfeasance and the educational attainment of Parliamentarians.

Dep.Var.	(1) Years of Education	(2) Years of Education	(3) Years of Education	(4) Years of Education	(5) Years of Education
Panel A: Legislatures 1948–1994					
Malfeasance charge	−0.614***	−0.674***	−0.458***	−0.576***	−0.643***
	(0.161)	(0.163)	(0.163)	(0.148)	(0.148)
Year of birth				0.010*	0.013**
				(0.006)	(0.006)
Sex				0.017	0.108
				(0.262)	(0.262)
Tenure				0.232***	0.180***
				(0.027)	(0.030)
Preference votes					0.483***
					(0.096)
Legislature dummies	No	Yes	Yes	Yes	Yes
Political Party dummies	No	No	Yes	Yes	Yes
Observations	6837	6837	6837	6591	6570
R2	0.002	0.005	0.082	0.107	0.111
Panel B: Legislatures 1992–1994					
Malfeasance charge	0.046	−0.066	−0.125	−0.186	−0.248
	(0.299)	(0.310)	(0.316)	(0.274)	(0.279)
Year of birth				−0.001	0.001
				(0.014)	(0.014)
Sex				0.234	0.255
				(0.421)	(0.419)
Tenure				−0.033	−0.069
				(0.078)	(0.081)
Preference votes					0.415**
					(0.185)
Legislature dummies	No	Yes	Yes	Yes	YES
Political Party dummies	No	No	Yes	Yes	Yes
Observations	1277	1277	1277	1235	1235
R2	0.000	0.002	0.037	0.041	0.045

Notes. OLS regressions. Panel A shows the results for the 11 Legislatures from 1948 to 1994, Panel B for the Legislatures of 1992 and 1994. Coefficients for the legislature dummies and political party dummies are not reported. Robust standard errors are shown in parenthesis. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

sample to deputies that in the past were elected in regional councils because they look closer to politicians in the control group. Finally, we treat the issue of education as a satisfactory proxy for the "quality" of politicians. Appendix A shows how the main results of our analysis are not significantly affected by both the constitutional reform of 2001 that empowered regions, and the 1995 reform that reduced to one the preferences in regional ballot schedules. In this Appendix we also present the results of DiD estimates performed on data from two proportional systems in two different periods: before 1993 and after 2005.

5.1. Change in politics due to "Clean hands"

Linked to the previous discussion, one specific concern in the econometric analysis is the following: did the change in the Italian political party system due to "Clean hands" affect the results presented here? This far-reaching campaign against political corruption generated profound changes in political groups, with the disappearance of all the main national parties. If the change in political system resulted in a renewal of the political class, it is probable that the inflow of new politicians contributed in generating changes in the average education of the elected by altering both the selection into the candidacy and the perception of voters. Moreover, if the pool of candidates running for national elections and for regional elections changed differently, it is difficult to attribute the effects on the education level merely to the ballot structure.

Therefore, these aspects of "Clean hands" could confound the estimates of the impact of the electoral reform on deputies' education. In the baseline empirical estimation (Table 7), by excluding the 1992 legislature, we already control for the "Clean

Table 12
Mean Impact: Party Dummies.

Dep.Var.	(1)	(2)	(3)	(4)	(5)	(6)
	Degree	Degree	Degree	Years of Education	Years of Education	Years of Education
Treat	0.249***	0.238***	0.212***	1.992***	2.013***	1.886***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.207***	−0.168***	−0.145***	−1.640***	−1.274***	−1.150***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	9161	9161	9161	9161	9161	9161
R-squared	0.046	0.065	0.065	0.060	0.086	0.087
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes
Party Dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). All estimations include election year FE for elections that took place in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010. All estimations include Party dummies for *DC*, *PCI* and *PSI* in the elections before the reform; party dummies for *PDS*, *Forza Italia* and *Alleanza Nazionale* in the first national and regional elections after the reform; party dummies for *Democratici di Sinistra* (DS), *Forza Italia* and *Alleanza Nazionale* in the second national and regional elections after the reform; party dummies for *L'Ulivo* and *Forza Italia* in the third national and regional elections after the reform; party dummies for *PD* and *Popolo delle libertà* in the fourth national and regional elections after the reform. Columns 2, 3, 5 and 6 add Regional dummies. Columns 3 and 6 add control variables: regional population (in log), regional education level and regional per-capita GDP; coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. Coefficients are not reported. The following symbols indicate significance levels: *** - 1%, ** - 5%, * - 10%.

hands" effect. Here, we tackle this concern over the robustness of our main results by augmenting the estimations of the model as in Eq. (1) with dummy variables to control for the evolution of the main political parties in each national/regional election. Specifically, we construct dummy variables as the product between a dummy for each party winning more than 10% of the seats in the Parliament/regional councils and a dummy for each election year where those parties appear. Accordingly, in Legislatures IX and X, and in the 1st and 2nd regional elections before the reform, the main parties within both the Parliament and the regional councils were the Christian Democrats (DC), Communists (PCI) and Socialists (PSI). In the first elections after the reform the main leading parties were *Forza Italia* and *Alleanza Nazionale*, and under Law 270/2005 they were the Democratic Party (PD) and "Popolo delle Libertà". The results are given in Table 12.

The significant negative sign of the coefficient of *Treat*Reform* in all the specifications in Table 12 is confirmed: controlling for the change in the Italian political party system, the negative impact of the reform on the educational attainment of politicians stands. The absolute value of the coefficient is greater than that estimated without the party dummies, as shown in Table 7 (comparing all the corresponding specifications as numbered at the top of Tables 7 and 12). This confirms our prior that in the wake of "Clean hands", newly formed parties selected more honest as well as more educated politicians.

To provide support for the identifying assumptions, Fig. 3 plots the estimated coefficients and 95% confidence intervals from Eq. (2), the dynamic DiD model, for our two measures of education. Graphs 3(a) and 3(d) refer to estimates with election year FE only; graphs 3(b) and 3(e) refer to estimates adding regional FE; graphs 3(c) and 3(f) refer to estimates adding regional controls. All the regressions that generated these graphs contain party dummies. Small and statistically insignificant coefficient estimates for *Treat*B*$_2$ provide support for our empirical approach. The statistical significance of the dynamic coefficients v_t shows that the education level of deputies responded negatively, year by year, to the reform.

5.2. Parliamentarians with past regional positions

In this context, can we be sure that regional politicians are an adequate counterfactual to national politicians? We have discussed the extent to which regional politicians are a "good" counterfactual for the DiD strategy used to estimate the effect of ballot structure on politicians' education. One potential concern is that the sample of regional councilors may differ in some meaningful dimensions from the treatment group. To address this concern, we restrict the treatment group to deputies with past experience in regional councils (as president and/or councilor) in order to make the two groups as similar as possible (as the DiD approach requires).

We estimate the mean impact of the electoral reform as in Eq. (1); the main results are reported in Table 13. Whatever outcome we consider, the findings remain rather unchanged: the effect of the national election reform is negative and significant in all the columns, and its magnitude is slightly greater than for the full sample of deputies (see Table 7). Specifically, the results suggest that the average education level of these deputies with regional experience is approximately 1.5 years less in the period following the reform, *ceteris paribus*, than it would have been in the absence of the reform. Likewise, the percentage of graduate deputies with regional experience declined by between 25.8 and 21.1 percentage points more than for regional councilors. Therefore, we are reassured that the results for the full sample are not driven by comparison between excessively heterogeneous groups.

For the sample of senators having held regional office, the results (in Panel B Table B.4, Appendix B) show the same pattern.
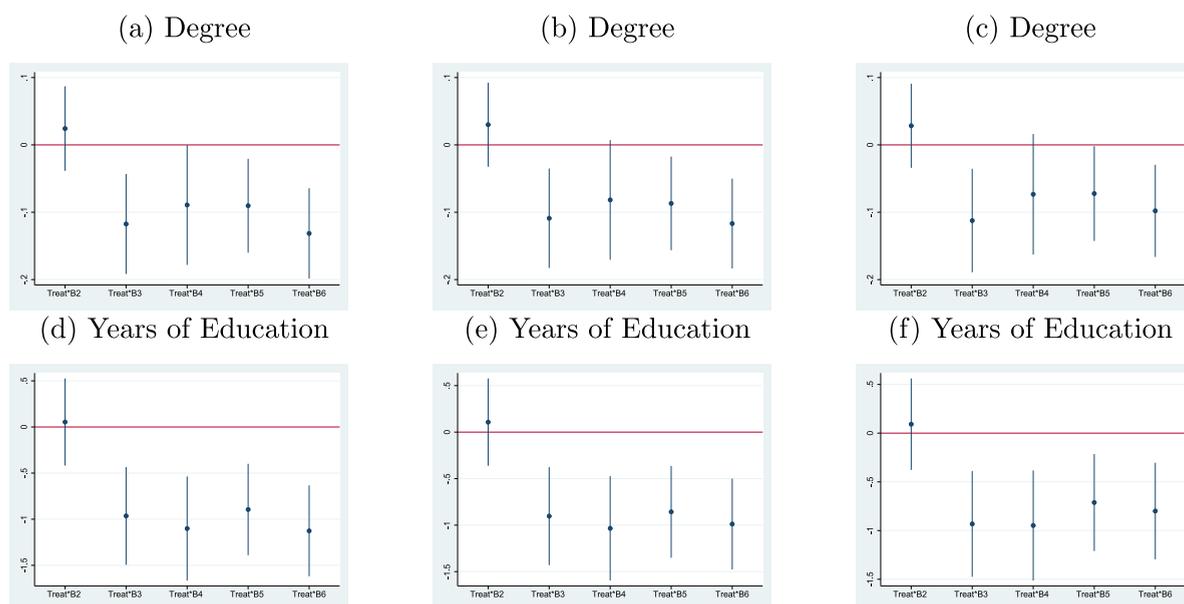
(a) Degree   (b) Degree   (c) Degree

(d) Years of Education   (e) Years of Education   (f) Years of Education

Fig. 3. Dynamic DiD Model: Party Dummies.
Note. The graphs report coefficients and confidence intervals estimated according to specification as in Eq. (2) for the two dependent variables *Degree* and *Years of Education*. Graphs 3(a) and 3(d) refer to estimates with electoral FE only; graphs 3(b) and 3(e), to estimates adding regional FE; graphs 3(c) and 3(f), to estimates adding regional controls. All estimations also contain party dummies. Standard errors clustered at regional level.

Table 13
Mean Impact: Deputies with past regional positions.

Dep.Var.	(1) Degree	(2) Degree	(3) Degree	(4) Years of Education	(5) Years of Education	(6) Years of Education
Treat	0.276***	0.270***	0.264***	1.977***	1.840***	1.923***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.258***	−0.218***	−0.211***	−1.577***	−1.234***	−1.283***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	6941	6941	6941	6941	6941	6941
R-squared	0.009	0.033	0.033	0.012	0.044	0.045
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). All estimations include election year FE for elections that took place in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010. Columns 2, 3, 5 and 6 add Regional dummies. Columns 3 and 6 also add control variables: regional population (in log), regional education level and regional per-capita GDP. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Coefficients are not reported. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

5.3. Education as Quality

An important issue, of course, is whether education is a satisfactory proxy for the "quality" of politicians. Although education and skills are recognized as one of the best measures of politicians' quality and, as noted, are widely employed in the empirical literature, in some cases they could come at the expense of honesty. As Mattozzi and Merlo (2008) point out, in a political career more education may well not be "better" in terms of motivation, honesty, and commitment to the public interest. Fedele and Naticchioni (2016) relax the hypothesis that ability is the sole relevant characteristic of politicians and investigate the role of motivation in shaping politicians' conduct. They find that *public-fit* politicians perform better than *market-fit* politicians because they are more highly motivated.[25] Like these authors, we select *public-fit* among those deputies who had at least one previous political position

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[25] They define a public-fit politician as "an individual that, before entering parliament, had at least one political experience as a town councilor or a mayor, for example, or the president/councilor of a province/region, or has shown party affiliation/appointment at the local and/or national level". By contrast, a market-fit politician is defined as "an individual with no previous political experience".

Table 14
Mean Impact: *public-fit* deputies.

Dep.Var.	(1) Degree	(2) Degree	(3) Degree	(4) Years of Education	(5) Years of Education	(6) Years of Education
Treat	0.223***	0.216***	0.204***	1.549***	1.522***	1.550***
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.216***	−0.162***	−0.150***	−1.352***	−0.893***	−0.899***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Observations	7288	7288	7288	7288	7288	7288
R-squared	0.008	0.032	0.033	0.010	0.042	0.044
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). All estimations include election year FE for elections that took place in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010. Columns 2, 3, 5 and 6 add Regional dummies. Columns 3 and 6 also add control variables: regional population (in log), regional education level and regional per-capita GDP; coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

Table 15
Mean Impact. Dep Var: Skill-intensive occupation.

	All Deputies			Deputies with past regional positions		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.365***	0.341***	0.381***	0.416***	0.406***	0.452***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.293***	−0.223***	−0.257***	−0.322***	−0.275***	−0.311***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	9131	9131	9131	6902	6902	6902
R-squared	0.059	0.076	0.077	0.029	0.048	0.049
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). Columns 1-3 show the results for all deputies, columns 4-6 for those with regional experience. All estimations include Electoral year FE. Columns 2, 3, 5 and 6 add Regional FE. Columns 3 and 6 add control variables: regional resident population (in log), regional education level and regional per-capita GDP; coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

as a town councilor, mayor, or president/councilor of a province or region. In our database, they account for almost 42% of all deputies. We perform the baseline DiD analysis on that sub-sample of deputies (results are presented in Table 14).

Our results are confirmed, bolstering the argument that the reduction in the education and skills of politicians due to the introduction of blocked lists of candidates can be interpreted as a reduction in their "quality".

5.4. Previous occupation

Here we further face the challenge in measuring the politicians' quality by using a different measure, based on politicians' previous occupations. We take a dummy variable taking a value of 1 for deputies and regional politicians who were engaged in skill-intensive occupations before being elected.[26] We assume that skill-intensive occupations allow higher earnings. Accordingly, as in Dal Bó et al. (2017), there is a strong positive correlation between all the ability measures and the pre-office incomes. Moreover, according to Dreher et al. (2009), the professional background of a head of government is important for quality and performance because it may encourage market-liberalizing policies. Thus, the rationale is that politicians whose previous occupation is skill-intensive would be considered good. We report the estimated coefficients in Table 15 for all deputies (columns 1–3) and those with past regional positions (columns 4–6). The effect of the reform is confirmed as negative and significant in both comparisons: it reduces the probability of having a highly-skilled deputy by between 22.3 and 29.3 p.p. more than for the control group. The negative effect is stronger for those with regional experience.

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[26] Table B.5 in Appendix B shows the full list of occupations included in this category.

Table A.1

Chow test: Constitutional reform of 2001.

Dep. Var	(1) Degree	(2) Years of Education
Trend*D2001	0.00327	0.0141
	(0.26)	(0.38)
D2001	−6.554	−28.20
	(0.26)	(0.39)
Observations	6475	6475
R-squared	0.026	0.036
Legislature FE	Yes	Yes
Regional FE	Yes	Yes

Notes. OLS regressions. All estimations include regional FE. Coefficients are not reported. p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

Table A.2

Constitutional reform of 2001.

Dep.Var.	(1) Degree	(2) Degree	(3) Years of Education	(4) Years of Education
Panel A: All Deputies				
Treat	0.235***	0.254**	1.981***	2.132**
	(0.00)	(0.05)	(0.00)	(0.02)
Treat*Reform	−0.130***	−0.0692	−1.549***	−0.956*
	(0.00)	(0.20)	(0.00)	(0.08)
Observations	5499	5499	5499	5499
R-squared	0.035	0.061	0.045	0.076
Electoral year FE	Yes	Yes	Yes	Yes
Regional FE	No	Yes	No	Yes
Panel B: Deputies with past regional positions				
Treat	0.857***	0.312**	2.047***	1.947**
	(0.00)	(0.05)	(0.00)	(0.02)
Treat*Reform	−0.782***	−0.110	−1.679***	−0.865*
	(0.00)	(0.20)	(0.00)	(0.08)
Observations	4237	4237	4237	4237
R-squared	0.478	0.040	0.010	0.046
Electoral year FE	Yes	Yes	Yes	Yes
Regional FE	No	Yes	No	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). Panel A shows the results for the treatment group of all Deputies; Panel B for those with regional experience. Sample restricted to 1983–2011. All estimations include election year FE for elections taking place between 1983 and 2001. Columns 2 and 4 add Regional FE for elections. Coefficients are not reported. Note that the deputies for regional experience account for around 4% of the total. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

6. Conclusions

A growing body of research holds that electoral systems affect the quality of institutions, first by helping to determine which politicians are elected and then the policies enacted. This paper contributes to the literature by investigating the effect of open as against blocked lists of candidates under proportional representation on the education of elected politicians, as a proxy for their ability. We exploit the national electoral reform of 1993 (Laws 276-277/1993) to compare the change in the education level of politicians in the treatment group (Parliamentarians) and a control group (Regional Councilors) following the reform. The evidence indicates that the introduction of the blocked list ballot scheme lowered the education level of national politicians. The results are strong: the reform lowered the probability of electing a university graduate and decreased their average years of education; and the dynamic impact of the reform is persistent over time. It negatively affected deputies as well as senators, and the results were driven by male politicians. The absence of a reform effect for female politicians is explained by the presence of negative stereotypes about the women's ability in politics.

Proportional representation, in various forms, is employed worldwide and the debate about its benefits for socio-political-economic outcomes is ongoing. While the rationale for blocked lists lies in the fight against exchange voting, corruption, and the undesirability of intra-party competition at elections, our findings clearly show that the implications of blocked lists for the

Table A.3

Chow test: "Tatarella Law".

Dep. Var	(1) Degree	(2) Years of Education
Trend*D1995	−0.000338	−0.0160
	(0.96)	(0.55)
D1995	0.649	31.70
	(0.96)	(0.56)
Observations	6475	6475
R-squared	0.026	0.036
Regional FE	Yes	Yes
Electoral year FE	Yes	Yes

Notes. OLS regressions. All estimations include Legislature and regional FE. Coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

Table A.4

Mean Impact: "pure" PR.

Dep.Var.	(1) Degree	(2) Degree	(3) Degree	(4) Years of Education	(5) Years of Education	(6) Years of Education
Panel A: All Deputies						
Treat	0.239***	0.218***	0.225***	1.887***	1.887***	2.057***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.196***	−0.163***	−0.169***	−1.345***	−1.114***	−1.256***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	6552	6552	6552	6552	6552	6552
R-squared	0.032	0.053	0.053	0.042	0.069	0.071
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes
Panel B: Deputies with past regional positions						
Treat	0.274***	0.257***	0.281***	1.954***	1.782***	2.146***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.267***	−0.240***	−0.262***	−1.547***	−1.336***	−1.655***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	4741	4741	4741	4741	4741	4741
R-squared	0.010	0.033	0.034	0.014	0.045	0.047
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions showing the mean impact of the electoral reform estimated using Eq. (1). Panel A shows the results for all deputies, Panel B for those with regional experience. The post-reform period includes national and regional elections that took place in 2005–2014. All estimations include election year FE. Columns 2, 3, 5 and 6 add Regional dummies. Columns 3 and 6 add control variables: regional population (in log), regional education level and regional per-capita GDP; coefficients are not reported. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Bootstrapped standard errors are clustered at regional level. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

recruitment of the political class involve the community as a whole. That is, we have documented that the ballot structure in PR systems has important consequences for the education of the candidates selected. Thus our results, which support previous studies arguing that the direct link between individual performance and election through open lists incentivizes good governance (Persson and Tabellini, 2003), support the re-introduction of preference voting schemes as an effective mechanism to ensure the election of better educated politicians.

Appendix A. Further robustness results

A.1. The constitutional reform of 2001 and suggestive evidence

There could some reasons why the educational attainments of national politicians declined after 1993 whereas they did not for regional politicians that we have not yet considered. The destruction of the post-war party system thanks to the exposure of so
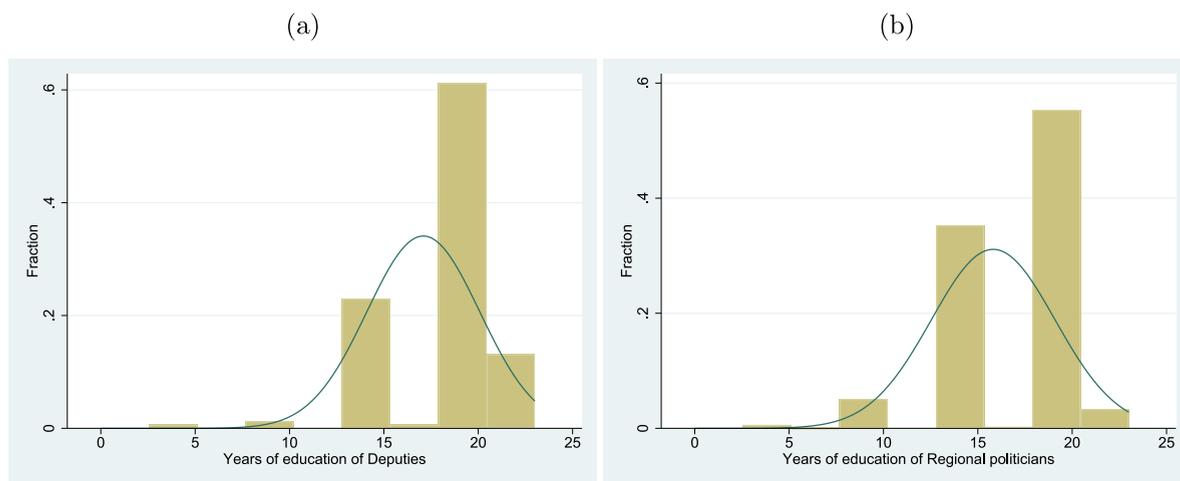
(a)   (b)

Fig. B.1. Distribution of the years of education of Deputies and Regional politicians.

Notes. Panels B.1(a) and B.1(b) compare the distribution of the years of education of deputies (treated politicians) and regional councilors (untreated politicians).

Table B.1

Table of conversion of the level of education into years of education.

Categories	Degrees	Years of education
University	Specialization Schools that enable to practice chosen professions and to use the title of specialist. In this case, admission requires students to obtain a specific degree and to pass a selective examination. Other qualifications officially recognized as equivalent	23
University	University Researcher, Phd, Other post-graduate's degree, Other qualifications officially recognized as equivalent	21
University	Bachelor of Medicine, Bachelor of Surgery. Master Degree, Other qualifications officially recognized as equivalent	19
University	Bachelor Degree, Other qualifications officially recognized as equivalent	18
Upper Secondary	High School Degree, Vocational Diploma, Other qualifications officially recognized as equivalent	13
Lower Secondary	Middle School Degree, Other qualifications officially recognized as equivalent	8
Primary Education	Elementary School Diploma	5
No education	No Diploma/Degree	0

Notes: In case no education level is specified, we use the variables "job" to derive an estimation of years of education according to the minimum level of education that the Italian law prescribes to attain that job. Instead, in cases either to infer directly the years of education from the variable "job", we assign an average value. The latter is equal to 15.5 years of education if the job of the politician can be exercised with both a university degree and a high school degree. Moreover, we assign a value of 10.5 when the job requires either a high school degree or a middle school degree.

Table B.2

Descriptive statistics of the education level of senators in Legislatures IX-XVI.

Legislature	Degree			Years of Education		
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.
IX (1983)	88.44	32.04	225	18.30	3.06	225
X (1987)	89.39	30.86	245	17.96	2.33	245
XII (1994)						
XIII (1996)						
XIV (2001)						
XV (2006)	65.48	47.65	309	17.13	2.60	309
XVI (2008)	69.28	46.20	306	17.39	2.70	306

Notes. The table reports the descriptive statistics of the variables *Degree* and *Years of Education* for Senators under PR with open list of candidates (Legislatures IX and X) and under PR with blocked list of candidates (Legislatures XV and XVI). Legislatures XII, XIII and XIV are empty because here senators were elected under majoritarian criterion.

many national politicians as deeply involved in corruption may have rendered national politics less attractive to the well-educated. National politicians who had been exposed as corrupt may have moved down to the regional level, where they would be less publicly visible. In order to face this concern, we present suggestive evidence looking at deputies that moved down to regional council. In our database they amount to 55, i.e., the 1.8% of the full sample of deputies. This would seem to exclude an incentive to a downward movement of parliamentarians.

Table B.3
Mean difference test on the average education level between the two sub-groups of deputies elected under Laws 276-277/1993.

Deputies	Majoritarian	Proportional	Difference
Dep. Var.: Degree			
XII Legislature	0.68	0.71	−0.028
N. Obs	475	155	
XIII Legislature	0.69	0.70	−0.016
N. Obs	475	156	
XIV Legislature	0.71	10.74	−0.028
N. Obs	475	150	
Dep. Var.: Years of Education			
XII Legislature	16.73	17.05	−0.32
N. Obs	475	154	
XIII Legislature	16.40	16.46	−0.056
N. Obs	475	156	
XIV Legislature	16.59	16.75	−0.160
N. Obs	475	150	

Notes: The table shows the t-test on the average of the two dependent variables, *Degree* and *Years of Education*, in the two bub-groups of deputies elected under Laws 276-277/1993. The "Majoritarian" sub-group comprises the 75% of deputies elected under majoritarian while the "Proportional" sub-group comprises the 25% of deputies elected under PR with blocked lists. The following symbols indicate different significance levels: *** - 1% - ** 5%- * - 10%.

Table B.4
Mean Impact: senators vs Regional councilors.

Dep.Var.	(1) Degree	(2) Degree	(3) Degree	(4) Years of Education	(5) Years of Education	(6) Years of Education
Panel A: All Senators						
Treat	0.313***	0.266***	0.254***	2.421***	1.942***	1.984***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.289***	−0.230***	−0.214***	−1.529***	−1.063***	−1.068***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	7590	7590	7590	7590	7590	7590
R-squared	0.026	0.048	0.049	0.045	0.073	0.074
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes
Panel B: Senators with past regional positions						
Treat	0.328***	0.280***	0.277***	2.175***	1.824***	1.923***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat*Reform	−0.315***	−0.246***	−0.240***	−1.358***	−0.841**	−0.903**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.04)	(0.03)
Observations	6724	6724	6724	6724	6724	6724
R-squared	0.006	0.032	0.032	0.011	0.044	0.045
Electoral year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes

Notes. OLS regressions. Panel A shows the results for all Senators, Panel B for those with past regional positions. All estimations include election year FE for elections that took place in 1983, 1985, 1987, 1990, 1994, 1995, 1996, 2000, 2001, 2005, 2006, 2008, and 2010. Columns 2 and 4 add Regional dummies. Coefficients are not reported. Controls are: regional population (in log), regional education level and regional per-capita GDP. Bootstrapped p-values of the null hypothesis that the parameter equals zero are shown in parenthesis. Coefficients of control variables are not reported. The following symbols indicate different significance levels: *** - 1%, ** - 5%, * - 10%.

Another important concern to deal with is the de facto strengthening of federal institutions in Italy that may have made subnational office more attractive. In 2001 the Italian Constitution was amended substantially, giving much greater power and autonomy to regions; important parts of the health system, energy system, active labor market policies, and local taxation (among other issues) were transferred to the regions. The aim of the reform was to promote federalism. From the perspective of this study, one possible consequence could have been that both parties and politicians revised their views of the most desirable office: after 2001 the attraction of regional elections became much stronger, which, clearly might have affected the incentives for high/low educated politicians to run for regional office.

Table B.5
Skill-intensive occupations.

Code	Description
111	Full and associate professors
112	High school teachers
113	Secondary school teachers
115	Headmasters
121	Writers, reporters, publicists
122	Painters, sculptors
124	Musicians, orchestral players, opera artists, actors
141	Surgeons (general)
142	Surgeons (specialized)
143	Dentists
144	Pharmacists
151	Magistrates
152	Lawyers and solicitors
153	Notaries
162	Veterinarians
163	Biologists, animal scientists, naturalists
171	Physicists, astronomers, geologists
172	Chemists
173	Construction engineers
174	Engineers
175	Architects
181	Mathematicians, statisticians, economists and sociologists
182	Chartered accountants
211	Entrepreneurs and chief executive officers (transport, credit, service and industry sector)
212	Entrepreneurs and chief executive officers (business)
213	Entrepreneurs and chief executive officers (public services)
214	Directors (transport, credit, service and industry sector)
215	Managers
216	Directors (public services)
217	Directors (public administration)

Notes. Data from the Italian Ministry of the Interior, Department for Territorial and Internal Affairs.

We address this concern in two ways. Firstly, we perform a Chow test for the presence of a structural break in the trend in education level of regional politicians before and after the Constitutional reform. We define a dummy variable (*D2001*) taking value 0 from 1983 until 2001 and 1 from 2002 to the end of the period of analysis and interact this dummy with the trend variable (*Trend*D2001*). We run OLS regressions in which the dependent variables denote, respectively, the share of graduates and the years of education of regional councilors. The results are presented in Table A.1. The coefficients of both the interaction term and *D2001* are never significantly different from zero. This would appear to exclude that changes in the incentives for regional politicians affected their quality.

As a second check, we replicate the baseline mean impact analysis restricting the sample to 1983–2001; this limits the analysis to politicians who were not affected by the 2001 reform. The results are presented in Table A.2 for all deputies (Panel A) and those with regional experience (Panel B). The significance of the treatment coefficient shows that the introduction of the electoral reform of 1993 was responsible for the decrease in the probability of electing a graduate deputy by 7 to 13 p.p. and the decrease in the average number of years of education of deputies by between 0.95 and 1.55 years (about 12 to 18 months of education). Our findings are confirmed considering only deputies having held regional office in the past.

A.2. Change in the number of preferences in regional elections

Another possible concern is the reduction in the number of preferences available to voters (from 3 to 1) affected by the regional election reform (Law 43 of 1995). We check whether this reform affected the education level of regional councilors, estimating a Chow test for a structural break in the trend of the education level of regional politicians before and after Law 43/1995. Thus, we define a dummy variable (*D1995*) taking value 0 until 1994 (when Law 108/1968 was in force) and 1 from 1995 to the end of the period of analysis, interacting this dummy with the trend variable (*Trend*D1995*). We run an OLS for both the main dependent variables denoting the education level of regional councilors. The results are presented in Table A.3. The coefficient of the interaction term as well as the dummy *D1995* are not significantly different from zero, regardless of the measure of education. This means that the switch from 3 preferences to 1 did not affect either the trend nor the level in the education of regional politicians, providing support to our identification assumption and corroborating our findings.

A.3. Open vs blocked lists in "pure" PR

A possible concern is that Laws 276-277/1993 disciplined a *mixed electoral system* with only 25% of deputies elected under the blocked lists proportional representation. The whole senate and the remaining 75% of the Chamber of Deputies were elected under

a majoritarian scheme. Even though on election day voters received different ballots for different votes according to both electoral rules, this system might have produced a bias in voting behavior. In order to consider this possible bias, we exclude from the sample the three Legislatures under "Mattarella Law" and ideally attribute the electoral reform to Law 270/2005. In this way we are able to compare two "pure" PR systems for the election of the entire parliament that differ only in their ballot structure: open vs blocked lists of candidates. In this case, the corresponding regional counterfactual are the third and fourth elections after the reform, as shown in Table 3. The estimation results, presented in Table A.4, are substantially unchanged in magnitude and remain significant, confirming, once again, that the reform was definitely not beneficial for the education of the politicians elected.

Appendix B. Additional tables and figures

See Fig. B.1, Tables B.1–B.5.

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