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ENFRANCHISEMENT AND REPRESENTATION: EVIDENCE FROM THE INTRODUCTION OF "QUASI-UNIVERSAL" SUFFRAGE IN ITALY

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Abstract

What are the political consequences of introducing de jure political equality? Does it change patterns of political representation and the identity of elected legislators? This paper uses an important electoral reform passed in 1912 in Italy to provide evidence on these questions. The reform trebled the electorate (from slightly less than three million to 8.650.000) leaving electoral rules and district boundaries unchanged. By exploiting differences in enfranchisement rates across electoral districts we identify the effect of franchise extension on various political outcomes. Enfranchisement increased the vote share of left-wing social reformers but had no impact on their parliamentary representation, no impact on parliamentary representation of aristocracy and traditional elites and no effect on political competition. We show that left-wing parties decreased their vote shares and were systematically defeated in key swing districts. We document elite's effort to minimize the political impact of the reform and, in particular, we show that the Vatican's secret involvement in the post-reform electoral campaign had a substantial impact on voting results, although formerly and newly enfranchised voters were equally affected. We relate our results to economic theories of democratization, which appear to be only partially compatible with our evidence.

Keywords: democratization, voting, electoral competition, inequality, swing districts, political violence, Vatican, socialism.

JEL code: D72

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"Everything must change so that everything can remain the same"

[Giuseppe Tomasi di Lampedusa: The Leopard]

1 Introduction

The arrival of *de jure* political equality in Western European countries during the late 19th and early 20th century was followed by rapid public policy changes. Lindert (1994, 2004), referring to what he defined *"the 1880-1930 laboratory"*, documents the historical proximity between franchise expansion and public provision of education, increased spending in social transfers, labour market reforms and the creation of income tax systems.[1] Correlations between the presence of democratic institutions and the type of policies that governments implement are generally well documented (Acemoglu and Robinson, 2006a). Causal evidence on the consequences of democratization is more difficult to establish. Most empirical studies exploit institutional variation that occurs across countries. In such settings, however, it is difficult to convincingly establish causality.[2] Natural experiments within a country have a better chance to identify causal relations, although both the institutional changes and the potential outcomes are necessarily more limited than in a cross-country setting. Both cross-country and within-country studies also face another challenge: institutional reforms often come in "bundles", not allowing therefore to identify the effect of political equalization in itself.[3]

This paper presents evidence on the political consequences of the introduction of "quasi-universal" male suffrage in Italy in 1912. Many characteristics contribute to make this reform an ideal setting to empirically analyse the political consequences of enfranchisement. First, this is one of the most significant franchise extensions in Western Europe. The reform almost trebled the size of the electorate from slightly less than three million to 8,650,000 and left disenfranchised only about half million adult males.[4] In most other countries enfranchisement was more gradual. In the

[1] See also the discussion in section IV.C of Acemoglu and Robinson (2000).

[2] For a discussion of the limits of cross-country analysis for the study of institutions see Pande and Udry (2006).

[3] The British Second Reform Act of 1867, for example, almost doubled the size of the electorate but, at the same time, it modified the boundaries of a vast majority of electoral constituencies (see Berlinski and Dewan, 2012).

[4] Figure 1 shows the number of registered voters from the annexation of Rome to the Italian Kingdom to the advent of fascism. Various franchise extensions occurred during this period and cross country studies tend to use 1919 as the date of the first election with universal suffrage. However, as shown in figure 1, the 1912 reform was substantially more important from a quantitative point of view. Historians refer to this reform as the introduction of "quasi-univeral" manhood

UK, for example, there were three Reform Acts (1832, 1867, 1884) which gradually extended the franchise before universal manhood suffrage was passed in 1918. In the years preceding 1912, the percentage of enfranchised population aged twenty and above was 38.7 in Germany, 32.5 in Sweden, 28.8 in the UK and 43.4 in France. In Italy it was only 15% and reached 42% with the reform (Flora, 1983).

The second characteristic that make this reform particularly interesting from an empirical viewpoint is that it left the electoral law and the electoral district boundaries unchanged making pre-reform (1909) and post-reform (1913) elections comparable. Third, enfranchisement levels varied substantially across the 508 single-member electoral districts. In the Sicilian district of Regalbuto, for example, registered voters increased from 2,145 to 16,704, an almost eightfold increase which transformed the previously enfranchised voters into a tiny minority. On the other side, the district of Milan II saw an increase from 8,493 to 10,702 and the impact of the newly enfranchised on the outcome must have necessarily been more modest. This heterogeneity can be used to identify the political impact of enfranchisement. The main identification challenge to be addressed is that districts like Regalbuto and Milan were different in other ways that can confound the impact of enfranchisement.

Our analysis is motivated by some influential economic theories of democratization that have been proposed in recent years. A common starting point of these theories is an apparent historical puzzle. A movement towards political equality gives more power to people with policy preferences which are likely to differ from those of previously enfranchised voters.[5] This changes the identity and policy preferences of the pivotal voter, therefore moving public policy away from the preferences of the elite (Meltzer and Richard, 1981). So why did the elite extend the franchise? According to Acemoglu and Robinson (2000, 2006a), the elite was forced to extend the franchise by revolutionary threats.[6] An alternative possibility is that franchise extension was granted as a consequence of an internal conflict within the elite (Lizzeri and Persico, 2004; Oxoby and Llavador 2005).[7] Related to these theories is also the

suffrage.

[5]Here and in the rest of the paper I refer to "preferences" not in the sense of a primitive of an economic model. Different policy preferences can be derived from the same primitive preferences but different endowments, in which case they indicate an economic conflict rather than different intrinsic predispositions.

[6]In such cases redistribution to meet the economic demands of the population may not be sufficient and an extension of the franchise works then as a commitment device to future redistribution. See also Conley and Temimi (2001).

[7]Enlarging the electorate makes pork-barrel politics less attractive for politicians and public good provision a more effective way to gain votes. Hence, by enfranchising larger segments of the population, non-swing elite groups, and particularly the urban and industrial elites, were trying

idea that democratization arrives as a consequence of economic equality and capital mobility (Boix 2003), since both reduce the equilibrium tax rates and therefore the opposition of elites to democracy.[8]

A key feature of all economic theories of democratization is that the newly and formerly enfranchised voters should have, on average, different preferences. Consistently with these theories, our empirical analysis shows that enfranchisement caused an increase in the vote share of social reformers.[9] Franchise extension, however, had a negative effect (or, at best, no effect at all) on the legislative representation of these same social reformers, on their probability to run for office, and on the competitiveness of elections. It also had no impact on the legislative representation of aristocrats and other members of the traditional elites.

One possible interpretation of these results is that there is no mechanical correspondence between *de jure* political equality and *de facto* empowerment of individuals. When elites decide to democratize, they still manage to retain sufficient *de facto* power to minimize the political impact of the newly enfranchised. Acemoglu and Robinson (2006b), for example, discuss how "captured democracies" can emerge because the newly created institutions maintain an advantage for elite groups.[10] In particular, elites' efforts to neutralize democracy should be expected if democratization arrives as a consequence of an intra-elite conflict, since the part of the elite whose interests are threatened by democracy can still use its *de facto* power to reduce the effects of democratization.

This paper will document and analyze elites' efforts to minimize the consequences of the 1912 reform by providing evidence on the effects of a secret pact (the Gentiloni pact) and by documenting how social reformers increased their vote shares where votes were less useful and were instead systematically defeated in key swing districts.

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to move the equilibrium policy in the direction of more public good provision and less patronage. Such elite groups gained the upper hand gradually during the 19th century, which explains the gradual extension of the franchise that occurred during that period.

[8] The literature on the determinants of democratization is vast: here I only discuss the theories that are most closely related to the subsequent empirical investigation and that I call "economic theories of democratization". Another prominent hypothesis, which goes under the label of "modernization theory" (Lipset, 1959), posits that economic development and political development move in parallel since, for various reasons, markets have better chances to prosper under democratic regimes. This theory lacks microfoundations and, as stressed by Rueschemeyer, Stephens and Stephens (1992), it does not specify clear causality links. On empirical grounds, the modernization hypothesis has been criticized by Acemoglu et al. (2009).

[9] One standard deviation in enfranchisement led to an average 2% increase in the vote share of social reformers.

[10] One example is the presence of a non-elected chamber, like in the UK or in Italy, or an extremely malapportioned one like in the USA.

The interpretation of these results rests ultimately on which model of electoral competition we think is best at representing what happened. In a Downsian context, with full commitment to proposed platforms, policy change can be achieved without much political change. It would not be surprising then to find little impact of enfranchisement on political outcomes. In theoretical terms, models that remove the full commitment assumption tend to stress the role of credibility and personal identity and, therefore, the importance of political selection.[11] In empirical terms, a number of recent papers show that parties and the personal identity of elected representatives generally matter for implemented policies.[12] We can presume that the political affiliation and personal characteristics of elected representatives had therefore some policy relevance at the time of the Italian democratization, suggesting that our findings can be related to both the political and policy consequences of enfranchisement.

The rest of the paper is organized as follows. Section 2 outlines the historical and institutional background, presents the main political actors and discusses the process and possible motivations that led to the franchise extension. Section 3 discusses the empirical strategy and the data. Section 4 presents our main empirical results and some robustness checks. Section 5 asks why the reform had so little impact on representation in spite of its effect on vote shares. Section 6 concludes discussing how our results relate to economic theories of democratization and to the findings of previous empirical research on enfranchisement.

2 Historical background

2.1 The political landscape

The years between 1901 and 1914 are politically dominated by the figure of Giovanni Giolitti: historians commonly refer to this period as the "Giolitti era". Moderately progressive and close to the emerging industrial elite, Giolitti rejected the repressive policies that had characterized the last years of the 19th century. Giolitti's years

[11] These include the models of representative democracy (better known as citizen-candidate model) of Osborne and Slivinsky (1994) and Besley and Coate (1995).

[12] Among others, Besley and Case (2003), Lee et al. (2004), Petterson-Libdom (2008) provide evidence of a partisan impact on public policy (Ferreira and Gyourko, 2009, however, find no impact in the case of US municipalities). Pande (2003), Chattopadhyay and Duflo (2004), Clots-Figueras (2010) provide evidence on the policy impact of the personal identity of elected representatives. Jones and Olken (2005) show that the identity of leaders has an impact on economic growth.

were characterized by a substantial increase in real wages, particularly in the industrial sector, possibly as a consequence of the increased bargaining power of unions which followed a more neutral approach of the state in disputes between employers and employees (Zamagni 1984; Gentile, 2003).[13]

2.1.1 The *Estrema*

According to economic theories of democratization, parties with a programme of social and institutional reform should be the main beneficiaries of universal suffrage. In Italy 1912 these parties were the Radical, the Republican and the Socialist, often refereed to as the *"Estrema"*, because located at the extreme left of the ideological spectrum. Although coming from different histories and traditions, these parties advocated policies that, to a certain extent, were similar, sharing demands for both economic and democratic reforms.[14] Candidates of the *Estrema* often formed alliances on a local basis but there was never a formal national alliance between them. They all remained mostly moderate and reformist during the Giolitti era.

2.1.2 The Constitutionals

The dominant "Constitutional" camp included both moderately progressive and conservative members of parliament (MPs). These people, however, had no party, no leader and no electoral manifesto.[15] There were instead factions, groups created around personal networks and the phenomenon of *"trasformismo"*, *"a system of political clientelism based on the formation of ad hoc parliamentary groups that monopolized political office by using patronage and fraudulent elections to ensure electoral success"*.[16] Parliamentary coalitions were, therefore, unstable and lacked a clear political identity.

[13] Giolitti helped to establish a modern system of industrial relations by refusing to use the military and the police to repress organized labour during disputes with employers.

[14] The parties of the *Estrema* shared proposals for important economic reforms (like the abolition of import tariffs on grain and the reduction of military spending), as well as ambitious programmes of social reforms that ranged from the tax system to schooling and labour regulations. Proposals for institutional reforms included universal suffrage, an elected upper chamber (Senators were appointed by the government) and the replacement of Monarchy with Republic.

[15] *"In Italy only the Republicans, the Radicals and the Socialists can be called parties. They have a programme, distinct from the programme of other parties, and they are kept together by the purpose of implementing that programme. The programmes of the various constitutional groups, instead, are not clear (...) More than political parties (...) these can be called factions"* (Duca di Gualtieri 1910: Necessita' di una ricostituzione dei partiti politici, *Rassegna Nazionale*, 31-171, p.133. My translation from Piretti, 1990, p. 107).

[16] Collier (1999), p. 70.

Constitutional MPs were divided in "Ministerial" and "Opposition" on the basis of whether they supported the current government or not. All Constitutionals, however, accepted the current institutional arrangements and recognized the authority of the Monarchy. Whether conservative or moderately progressive, they had a perception of themselves as the ruling elite, the only people that could possibly govern the country.

2.1.3 The Catholics

Italy had been unified half a century before the events described in this paper at the expenses of, among others, the Catholic state. The Vatican had never recognized Italy and still maintained the *non expedit*, the prohibition for Catholics to participate in public life. From the early 20th century, however, local bishops could ask for a dispensation, usually on the ground that the Catholic vote was necessary to prevent the election of "subversive" candidates. The first few dispensations were granted in the region of Lombardy in 1904 and some more were granted in 1909. In 1913 this process of unofficial entry of Catholics in Italian politics led to a secret alliance (known as *"Gentiloni pact"*) between the Catholic Electoral Association and Giolitti: as a consequence the *non expedit* was suspended in 228 electoral districts. By signing the secret pact candidates committed to a number of pro-Catholic policies (on family and moral values, schools, Catholic education etc.).

2.2 The electoral reform

The existing voting law, in place since 1882, granted voting rights on the basis of literacy and census criteria.[17] The 1912 reform granted universal suffrage to all males over 30, while keeping the pre-existing restrictions for males between 21 and 30. The voting right was also granted to any adult male who had served in the army.

[17] According to the 1882 law, only literate males aged at least 21 could be included in the electoral registers. In addition, they needed to satisfy at least another criterion from a given list. The most important criteria in the list were: (a) to have a minimum of formal education (a two-year certificate); (b) to pay at least 19.80 liras of income tax; (c) other criteria mainly consisting of owning or renting an accommodation of a minimum size (the exact number of square meters depended on the town population). An income tax payment of 19.80 liras was easily reached by most workers in urban areas. According to estimates by Zamagni (1984), the average industrial salary in 1911 was 2.67 liras per day. The income tax rate was 8%. Hence, it was quite easy for an average industrial worker regularly paying taxes to satisfy the census criterion. The literacy criterion could be satisfied in two ways: either with a title of second year primary school (which was then sufficient to obtain the electorate) or by writing an application in front of a public official.

When Giolitti announced his proposal, in June 1911, maybe surprisingly, the *Estrema* did not display much enthusiasm for the reform, defined by socialist Gaetano Salvemini a "*lunch at 8am*". "L'Avanti!", the official newspaper of the Socialist Party, commented*: "democratic progress is not only and always obtained by extending political rights. The bourgeoisie easily concedes freedom and voting rights, but they know other ways to keep intact their economic tyranny, while they concede more economic reforms in favour of the masses when they have a firm grip on the monopoly of political power".*[18]

The Socialists were also quite absent from parliamentary debates, to the point that their leader Filippo Turati, explicitly felt the need to defend their lack of participation on the ground that *"the new law has all the signs (...) of a benefit which has not been conquered, but imposed and to which our part could not impress any of our characteristics".*[19] This could have been just a tactic to avoid conceding any merit to Giolitti for the reform. More likely, however, it reflected a real dilemma and an ongoing debate inside the party between advocates of universal suffrage[20] and the moderate leadership, which only paid lip service to the cause of enfranchisement.[21] This debate also reflected the fact that the moderate leadership was concentrated in urban areas in the North, where blue collar workers were often already enfranchised, and was generally suspicious about the real attitudes of disenfranchised peasants.

In the final secret vote on May 25, 1912, the 346 present MPs (out of 508) were mostly favorable (284 voted in favour, 62 against).[22] As these numbers show, attendance and voting during parliamentary debate was generally low.

[18] L'Avanti!, May 9, 1912. My translation.

[19] "Il suffragio colla museruola", *Critica Sociale*, XXII, n. 10-11, pp. 145-146, May 1912. My translation from Ballini (2007), p. 176.

[20] Gaetano Salvemini, one of the most passionate proposers of universal suffrage, provided already in 1905 a rather "Downsian" view of how it could change implemented policies: *"it opens the field to the competition of all interests and of all parties. Disenfranchising a part of the population means that political parties will not normally be interested in the needs of the excluded; and that a big cause of political education is suppressed, since the many excluded from the voting rights will not find anybody interested in mobilizing them".* Salvemini (1905), p. 371. My translation.

[21] For the dominant reformist faction *"the franchise in itself is an instrument, and without a force that knows how to use it, it can damage precisely those that demand it"* (Bonomi,1905, p. 341. My translation).

[22] On June 29 the Senate, whose life-time members were appointed by the government, approved the law with 131 votes in favour and 40 against.

2.3 Why did Giolitti extend the franchise?

Like in the rest of Europe, universal suffrage arrived in Italy as a concession from the elite and, as for other similar instances, historians have speculated for decades about the motivations behind the reform. In this section I will make an attempt to link the hypotheses made by historians on this specific event to more general ideas about democratization.

One of the most influential theories on democratization is that it emerges from the struggle between elites and non-elites, when the last are in a position to make a credible revolutionary threat (Acemoglu and Robinson, 2006a). In the case of the Italian 1912 reform, a number of factors seem to indicate that revolutionary pressure was low, and certainly lower than in previous years. All the parties of the *Estrema* were controlled by relatively moderate leaders and one party, the Radical, had three ministerial positions in the Giolitti government. Social conflict was low if compared with previous years.[23] From an economic standpoint, Italy's estimated average annual GDP growth rate between 1899 and 1913 was about 2.7%. Average annual growth rate of salaries between 1901 and 1911 was 2.5%, in a context of rapid industrialization and good order in the public finances (Toniolo, 1988). In brief, it appears unlikely that the reform was triggered either by an economic crisis or by the threat of a revolution.[24]

Some historians believe that Giolitti was conscious of the risks associated with a massive suffrage extension, but was convinced that it was inevitable. Hence, it was better for the constitutionals to guide the process rather than being forced to concede it.[25] This might have been a preemptive move against the Socialists, whom sooner or later were expected to launch a campaign for universal suffrage. Also, by controlling the process of franchise extension, Giolitti could make sure that it was implemented in a way which was advantageous for the constitutionals.[26] This inter-

[23] When Giolitti proposed the reform, in June 1911, number of strikes and number of participants in strikes was relatively low. See Figure 2.

[24] Giolitti himself appears to avail this conclusion by declaring that *"the big reforms must be proposed when the time is ripe, when the Country is calm"* (my translation from Ballini, 2007).

[25] Gentile (2003).

[26] There were no revisions in the district boundaries and no concession in the direction of a more proportional representation. Both would have given to the Estrema a tangible benefit, since rural (and conservative) electoral districts were heavily overrepresented. Electoral boundaries had remained unchanged since 1892 (and based on 1881 Census data). At a time of rapid urban development, this had led to a situation where some districts could be several times larger than others. Typically, urban areas and rapidly industrializing areas were underrepresented and these were precisely the areas were the Socialists were stronger and growing faster. The Socialists also felt that proportional representation would have moved attention from individuals to programmes

pretation is compatible with the *party-competition* hypothesis, according to which democratization was essentially driven by short term political considerations.[27] It is also compatible with the idea that, when conceding voting rights, elites try to retain or introduce institutional features that minimize their loss of political influence.[28]

With respect to the theory of Lizzeri and Persico (2004), some historians stress that the reform could have emerged from Giolitti's desire to stabilize his majority by enlarging it to the left. It was difficult for Giolitti to fully implement a moderately progressive agenda in a predominantly conservative parliament. He had made repeated attempts to absorb parts of the *Estrema* into the government. He succeeded with some Radicals but not with the Socialists, even the most moderate. Expanding the electorate could have, therefore, represented an attempt to stabilize his majority to the left, in a context in which the *Estrema* was sufficiently moderate.[29]

Recent theoretical developments also link democratization to the presence of war and the need for mass-mobilization.[30] This hypothesis fits well with immediate speculations made at the time about a possible link between the electoral reform and the war for the colonization of Libya.[31] *"With that concession, Giolitti wanted to secure the support of the reformist Socialists to the conquest of Libya"*,[32] or at least to appease the anti-militarists in the *Estrema* (while the war in Libya could be regarded as a concession to the nationalists and the Catholics).[33] This interpretation of the 1912 reform, however, appears to have lost credit among historians.[34]

Finally, according to the so-called *enlightenment hypothesis*, democratization was driven essentially by the fact that the values of the elite were changing.[35] Historical

and that they could benefit from a more party-centered politics.

[27] See the discussion of this approach contained in Acemoglu and Robinson (2000).

[28] See Acemoglu and Robinson (2006b).

[29] This amounted, in Giolitti's view, to a strategic alliance between the most progressive components of the elite and the emerging organized working classes in order to modernize the country (Montaldo, 2001).

[30] Scheve and Stasavage (2010), Vindigni and Ticchi (2008).

[31] See for example Carocci (1961). The Libyan war was declared in September 1911, a few months after Giolitti's electoral reform proposal and, although Libya's annexation to the Italian Kingdom was declared in November 1911, the war was officially concluded only in October 1912. Hence, when the proposal was debated and voted in parliament, Italy was still at war with Turkey over Libya. This provided a new argument to pass the law: in the words of MP Sidney Sonnino "*they have conquered"* their right to vote "*in the Tripoli battlefields; no-one asked then Southern peasants whether they were illiterate or not"* (my translation from Ballini, 2007).

[32] Salvemini (1955). My translation. As a matter of fact, some reformists and, for different reasons, even some revolutionaries in the Socialist Party supported the war.

[33] The Vatican had important economic interests in Libya that felt were not adequately protected by the Turkish government.

[34] It is likely that, at the time the electoral reform was proposed, Giolitti had not planned yet to invade Libya. See for example Montaldo (2001).

[35] See the discussion of this hypothesis in Acemoglu and Robinson (2000).

evidence shows that Giolitti genuinely believed in a stronger and more representative parliament; he had passed other reforms that had reinforced the *Deputy Chamber*[36] and this could have been another step in a process of institutional modernization that Giolitti was confident to keep under his control.[37] Whether this was the consequence of changing values or, rather, of strategic considerations remains a moot point and hard to establish. It is useful perhaps to remember that Giolitti's opinion change on universal suffrage was quite sudden, as proven by parliamentary records[38], a fact which makes the enlightenment hypothesis less plausible.

To sum up, the motives that induced Giolitti to massively extend the franchise remain unclear and still debated today. Without pretending to provide definite answers, this section has highlighted the main links between a consolidated historical research and some influential hypotheses on democratization. Perhaps historians and theorists face the same difficulties, which ultimately lie in insufficient evidence to discriminate between different hypotheses. In the conclusions we will reconsider the issue on the basis of the evidence provided in this paper.

3 Research design and data

3.1 Empirical strategy

Our identification strategy is based on comparing the first post-reform election (1913) with the last pre-reform (1909). This tries to approximate an experiment in which we compare the actual outcomes of the 1913 election with the outcomes that would have occurred without the reform. If we indicate with S_i^{13} the *Estrema* share of vote (or any other outcome of interest) in district i in the 1913 election, we can write

$$S_i^{13} = \alpha^{13} + \beta_P \frac{E_i^P}{E_i^{13}} + \beta_N \frac{E_i^{13} - E_i^P}{E_i^{13}} + e_i^{13} \tag{1}$$

where E_i^P is the number of citizens in district i that would have been enfranchised in 1913 under the old electoral rule, while E_i^{13} is the actual number of enfranchised

[36] He had increased the discretion of parliament in regulating its internal organization and had instituted the explicit vote of confidence at the beginning of a new government. Until then, there was presumption of confidence unless a confidence vote was called and lost by the executive.

[37] See Ullrich (1979) and De Felice (1980).

[38] He had publicly opposed universal suffrage only two years earlier by declaring *"I believe that we need to have universal suffrage but by a different mean: by teaching to everybody how to write and read"* (my translation from Piretti 2001).

citizens in 1913. β_P and β_N represent the average propensity to vote *Estrema* among, respectively, the formerly and newly enfranchised citizens. α^{13} is an effect which is common to all electoral districts in 1913 and e_i^{13} is a district-specific error. E_i^P is unobservable but we can approximate it with E_i^{09}, the actual number of registered voters in 1909, under the assumption that exit (voters that died or moved elsewhere) and entry (new voters that met the capacity condition or moved in the district) in E_i^P balance each other.

If we assume β_P and β_N to be constant (after taking into account the time-specific effect α^t), at least in the short time span we consider, then we can write a similar equation for 1909:[39]

$$S_i^{09} = \alpha^{09} + \beta_P + e_i^{09} \tag{2}$$

By subtracting (2) from (1) we can write our estimable equation:

$$S_i^{13} - S_i^{09} = (\alpha^{13} - \alpha^{09}) + (\beta_N - \beta_P)\frac{E_i^{13} - E_i^{09}}{E_i^{13}} + (e_i^{13} - e_i^{09}) \tag{3}$$

This specification allows us to recover the difference in the propensity to vote *Estrema* among the two groups of voters. This is a differences-in-differences specification with a continuous treatment variable, hence we need to worry about changing rather than fixed characteristics of the electoral districts. To address these concerns we will use control variables, province specific shocks and previous changes in dependent variables. Regressions using placebo treatments will help us to understand what is the impact of pre-existing trends on our results.

Assuming that $E_i^{09} = E_i^P$ is a reasonable approximation since the time span considered is short. The variable E_i^P, however, is measured with an error that, even if randomly distributed, could bias our results downwards. Also, we cannot rule out the existence of a correlation between the measurement error $(E_i^{09} - E_i^P)$ and $(e_i^{13} - e_i^{09})$. For example, labour organizations might have been stronger in districts with higher immigration leading therefore to larger increases in *Estrema* vote. One of the control variables employed, population change between 1901 and 1911, should at least partially deal with this potential source of bias. Controlling for changes in

[39] This assumption ignores the possibility of strategic voting and, more generally, possible reactions of the formerly enfranchised to the new political situation. We are also ignoring possible differences in turnout rates across the two groups of voters: β_P and β_N bypass that stage and represent the overall reduced-form propensity to vote *Estrema* (where the alternatives are both voting for other parties and not voting).

male literacy rates also helps us to better approximate what would have happened under the previous law that restricted the franchise mainly on literacy grounds.

3.2 Data description

Between 1892 and 1913 Italy had 508 single-member electoral districts with a two-round majority system. Registration data and electoral results for the elections occurred between 1900 and 1913 were collected from the Parliamentary Archive in Rome (*Archivio Storico della Camera dei Deputati*).[40] One of our key dependent variables will be the vote percentage of *Estrema* candidates in the first election round.

Biographical information on members of parliament has been collected from Malatesta (1940).[41] We use information regarding the family and social background of MPs: whether the MP is an aristocrat, a big landowner, a high-ranked military or a diplomat. These groups were generally very close to the Monarchy and represented the traditional (and often most conservative) elites. To this we also add information on whether the MP is a member of a political dynasty, which signals belonging to an established influential family.[42]

Data on the socioeconomic characteristics of electoral districts have been reconstructed using the 1901 and 1911 Censuses. Regression analysis uses both 1901-1911 changes and 1911 levels of the following variables: total population in the districts and the percentages (over the total population) of employees in industrial sectors, of landless agricultural workers, of agricultural workers cultivating their own land, of real estate owners, of illiterate males (over total male population aged six and above). For 1911 only it has been possible to also reconstruct the percentage of urban population. Further details on these variables are provided in the Appendix. Information on other variables is provided in the Sections where the variables are used.

[40] Since candidacy was individual and there was no official affiliation with political parties, the Archive only contains the number of votes obtained by each candidate but does not provide information on political affiliations. The matching between names and political parties has been possible thanks to currently still unpublished information collected by Maria Serena Piretti from newspaper articles of the period. This information has allowed me to reconstruct the vote share by party and by electoral district in the 1900-1913 elections. Table A1 reports information on the number of candidates, votes (in the first round election) and seats for the three parties of the *Estrema* between 1900 and 1913.

[41] This is a three-volumes collection of biographies of Italian MPs from 1861 to 1924.

[42] Table A2 in the Online Appendix reports aggregate numbers of aristocrat and traditional elite members for the period 1904-1913.

3.3 Correlates of enfranchisement

Our main explanatory variable, $\frac{E_i^{13}-E_i^{09}}{E_i^{13}}$ (from now on ΔE) varies substantially across electoral districts.[43] One obvious reason for this variation is the heterogeneity in illiteracy, which was the most important obstacle to registration before the reform.[44]

An OLS regression of ΔE over male illiteracy rates (column 1 in Table 1) shows that 55% of the variation in enfranchisement can be explained by literacy alone. Column 2 in Table 3 introduces other covariates: ΔE is smaller in urban districts and where the percentage of industrial workers is higher but also, controlling for other covariates, in areas with a higher share of agricultural workers that do not own their own land. Columns 3 and 4 use ΔE_{t-1} as dependent variable, showing that pre-reform changes in enfranchisement are positively correlated with the size of a district (overall population) and with population changes (this is the case also for ΔE) but not with any of the other district characteristics. Not surprisingly, enfranchisement between 1904 and 1909 grew faster in districts with lower illiteracy rates. Table 1 suggests that the reform of 1912 created a substantial discontinuity with respect to existing trends in enfranchisement: post-1912 enfranchised population across the electoral districts was substantially different from what would have been under the previous law.

An important question is whether ΔE is correlated with the political orientations of the districts. The columns from 5 to 8 of table 3 show that ΔE was higher in districts with historically weaker *Estrema*.[45] Although not surprising (the *Estrema* was stronger where a larger share of the poor were already enfranchised), these results suggest that simple OLS regressions would deliver biased coefficients. A differences-in-differences approach removes fixed characteristics of electoral districts (including previous *Estrema* electoral strength). Our results could be biased if districts with different pre-existing *Estrema* strength were trending differently, a concern that will be addressed in various ways during the empirical analysis.

[43] Some graphical inspections of the data are reported in the Appendix. Figure A1 shows the distribution of ΔE. Figure A2 reports the distributions of registered voters by electoral district in 1909 and 1913. Figure A3 plots ΔE against changes in Estrema vote percentages (1909-1913) and indicates whether the district was from the North-West (NW), North-East (NE), Center (C) or South (S). See Table A6 for a definition of these variables.

[44] Figure A4 plots ΔE over male illiteracy rate in 1911 showing, not surprisingly, a strong positive correlation. The correlation coefficient is 0.74. It is also clear that illiteracy was strongly correlated with latitude.

[45] Similar results, with slightly smaller magnitudes, are obtained if we control for male illiteracy rate in 1911.

4 The political impact of the 1912 reform

4.1 A simple graphical inspection of the data

The most important empirical concern associated with our identification strategy is that unobservable changing characteristics of the electoral districts can't be controlled for, and these characteristics may in turn be correlated with changes in electoral outcomes. Figure 3 provides a simple graphical inspection of the performance of Estrema candidates between 1900 and 1913. Panels (a) and (b) divide the districts into ΔE tertiles. In 1913 we observe an increase in Estrema vote share in districts with higher ΔE. Compared with all other elections, the distance between low ΔE districts and the others is now much narrower which is consistent with the idea that the 1912 reform, making the electorate relatively more similar across districts, should have reduced the differences in *Estrema* vote.[46]

The pattern for share of elected MPs is different (panel b). In 1913 the number of elected MPs from the *Estrema* increases in all districts but particularly in those with low ΔE. The reason might be that, although the vote change is smaller (see panel a), it may be sufficient to cause an Estrema victory in those districts. On the other side, in districts with high ΔE the *Estrema* might have experienced higher gains, but most of the times not sufficient to win the seat. This hypothesis, however, is contradicted by panels (c) and (d), which divide the districts by Estrema vote share in 1909. It appears that where the Estrema was already strong it gained nothing, on average, after the franchise extension, both in seats and in votes. Gains tend instead to be concentrated in districts with an intermediate or a weak Estrema. Comparing (a) and (b) with (c) and (d) it is evident that there is only partial overlap between ΔE and *Estrema* pre-reform vote share. Vote gains appear to be concentrated in districts with relatively high enfranchisement and weak Estrema, seat gains in districts with low enfranchisement but also not with (pre-reform) strong Estrema.

4.2 Baseline estimates

Estrema vote share. We start our regression analysis reporting results when the dependent variable of equation (3) is the share of votes received by candidates of

[46] Blue collar workers were sometimes already enfranchised in parts of the country because of higher literacy rates and higher incomes. Hence, before the reform the poorer segments of society were partly enfranchised in some districts and not enfranchised in others. In this sense the reform must have made the electoral districts more similar with each other.

the *Estrema*. Starting with a simple regression in Table 2 we progressively include control variables and province specific shocks.[47] To account for possible pre-existing trends we then introduce the percentage change in Estrema vote between 1904 and 1909 (alone in column 5 and interacted with ΔE in column 6).[48] Point estimates appear to be positive, statistically significant and rather stable. They range between a minimum of 0.167 and a maximum of 0.294. Controlling for a lagged dependent variable gives us a coefficient of about 0.25. Statistical significance of at least 10% is always achieved. The coefficients are easy to interpret, since both the dependent and independent variable are expressed as percentages. Taking column (6) as a benchmark, a 1% increase in ΔE caused a 0.25% increase in the votes of *Estrema*. The smallest estimate (column 3) is such that one standard deviation in enfranchisement (almost 12%) corresponds to a 2% increase in *Estrema* votes.[49] This implies that the difference between the district of Regalbuto ($\Delta E = 87$) and that of Milan II ($\Delta E = 21$) generates a difference in votes for *Estrema* of about 11% due to enfranchisement only.

Estrema MPs. Table 3 shows OLS estimates of the impact of enfranchisement on the net seat gains of *Estrema* candidates. Our dependent variable is now equal to 1 if the seat was gained, -1 if it was lost and 0 otherwise.[50] Following our discussion of Figure 3, we include a non-linear control for pre-existing Estrema vote share, since the impact of marginal votes on probability of victory is different depending on pre-existing vote shares. The coefficient of ΔE remains negative across all specifications and becomes statistically significant when we introduce control variables. In spite of the gains in votes, Estrema candidates on average appear to have been disadvantaged by the reform in terms of their chances of victory. In columns (6) and (7) we distinguish between the probability of gaining a seat (in districts where the incumbent MP was not from the Estrema) from the probability of losing a seat for incumbent Estrema MPs. Enfranchisement had no impact on the victory chances of Estrema challengers but it adversely affected incumbents. Ceteris

[47] Given that the dependent variable is expressed in differences, province fixed effects represent 1913 province-specific shocks compared to 1909 levels.

[48] From column (5) it is clear that the lagged dependent variable has a significant effect on the 1909-1913 vote change, signalling that the performance of *Estrema* candidates was trending differently across districts.

[49] A similar magnitude is implied by column (4), considering that within-province standard deviation is equal to 6.4.

[50] Using maximum likelihood ordered probit confirms the findings of table 3. Ordered probit models, however, cannot include fixed effects. We prefer to report OLS estimates because these can include province specific shocks.

paribus, one standard deviation increase in ΔE makes an incumbent MP from the Estrema 8% more likely to be defeated.

Aristocrats and traditional elites. In 1909 and 1913, aristocrats represented almost one fifth of elected MPs. In 1913 there were 31 transitions of an electoral district from an aristocrat to a non-aristocrat and 28 on the other direction.[51] Let us call ΔA a variable equal to 1 if a district transits from a non-aristocrat to an aristocrat, -1 if the transit happens in the other direction and 0 otherwise. The first 3 columns of Table 4 report OLS coefficients where ΔA is used as dependent variable. The coefficient of enfranchisement never achieves 10% significance level[52] and, perhaps more importantly, is never negative, indicating that a higher enfranchisement rate is more likely to have caused an aristocrat to gain a seat rather than loosing it.

This analysis has been replicated by using a less narrow definition of traditional elites. $\Delta elite$ includes aristocrats and non-aristocratic landowners, military, diplomats and members of political dynasties.[53] The variable *elite* includes 134 MPs in 1913 and 127 in 1909 with 45 negative and 37 positive transitions in the 1909-1913 period. The results are very similar to those we found for aristocrats alone, with slightly larger coefficients but far from any acceptable statistical significance.

Candidacy. There were 156 districts with no *Estrema* candidate in 1909, 95 in 1913.[54] Observing a candidate of the *Estrema* in 1913 but not in 1909 (and viceversa) could signal that expectations about the performance of *Estrema* candidates in that districts have changed. Even not winning a seat, a good performance could set the stage for future progress and send a signal to voters that *Estrema* candidates were viable.[55] In Table 5 the dependent variable ΔC is coded as 1 if there is an Estrema

[51] See Table A2 for details.

[52] But with no change in 449 out of 508 cases, the variation in the dependent variable is small.

[53] High ranked militaries had to sworn their loyalty to the Crown and were usually recruited among aristocrats or other influential families trusted by the King. People in charge of foreign policy were usually very close to the Crown and were recruited among the most traditional and influential families. For what concerns dynasties, an MP has been classified as being a member of a political dynasty when it has been possible to establish a family link with at least one other MP from the same or previous Italian parliaments (including the non-elected Senate). There is a substantial overlap between these groups (for example, most high ranked militaries were aristocrats).

[54] Many districts, especially in the South, were contested by more than one constitutional candidate but not by a candidate of the *Estrema*.

[55] In 1904, for example, both the Radicals and the Socialists had candidates in a large number of districts that received a single-digit number of votes. The number of candidates in 1909 was reduced (see Table A1 in the Appendix) in order to concentrate resources and to avoid sending negative signals. The result was an overall clear improvement in the seat per vote ratio. We witness again an increase in the number of *Estrema* candidates in 1913 but we show that it can't be linked to franchise extension.

candidate in 1913 in a district with no Estrema candidate in 1909, -1 if the reverse occurs and 0 otherwise. The estimated coefficients show that larger enfranchisement was associated on average to a small positive ΔC but this effect becomes statistically insignificant when controls and province specific shocks are included.

Electoral district competitiveness. Regulated competition for power is a key characteristic of democracy. Did enfranchisement increase the overall level of electoral competition? This question has been addressed by using the Herfindahl-Hirshman index (HHI) of competition among candidates (i.e. ignoring the candidates' party affiliation). Indicating with s_i the vote share of candidate i, the HHI index is calculated as $H = \sum_i s_i^2$. The results (reported in table 5, columns 4-6) show that enfranchisement did not cause any change in district competitiveness.

Turnout. The 1913 election saw a generalized decline in electoral participation, with overall turnout rate decreasing to 59% from 65.4% in 1909. Table 5 (columns 7-9) shows that this decline was caused by the increase in the number of registered voters, since the newly enfranchised had a lower propensity to participate compared to pre-reform voters. Across all specifications we find a negative effect of ΔE on turnout. Using column 8 as benchmark, we have that a 1% increase in the share of newly enfranchised voters decreased turnout by 0.24%. Hence, the political impact of the reform was mitigated by the lower participation rates of the newly enfranchised.

Summary. Our baseline results suggest that the 1912 enfranchisement caused, on average, an increase in the *Estrema* vote share but had a negative effect, or at best no effect, on *Estrema* parliamentary representation. Enfranchisement also had no significant impact on the parliamentary representation of aristocrats and other traditional elites. Seat competitiveness and the entry of *Estrema* candidates also did not improve. In the Appendix we report further estimates that contribute to our "minimal effect" picture.

These results are puzzling. Why were increased vote shares not translated into an increased parliamentary representation? And why were traditional elites not affected? These questions strike at the core of current debates about democratization. If universal suffrage and a massive input of new voters into the electoral body has no substantial implications for the distribution of legislative power then it is legitimate to ask how can democratization serve as a commitment device to future redistribution. At the same time, however, our results document that new voters

did support *Estrema* candidates more than the previously enfranchised: voting patterns reflected, on average, what would emerge from economic conflict á la Meltzer & Richard.

We will return on these questions in Section 5, where we will explore some of the hurdles that might have prevented the *Estrema* from taking advantage of the reform.

4.3 Robustness checks

The main identification concern for our empirical strategy is that the results could be driven by pre-existing trends. For this reason for each of the outcomes regressions have been re-run using the corresponding 1904-1909 change as dependent variable.[56] Results are reported in table 6. The vote share change of *Estrema* candidates in 1904-1909 appears to be negatively related to subsequent enfranchisement and is never significant at conventional levels. This makes it unlikely that the change in the *Estrema* vote share between 1909 and 1913 is due to pre-existing voting trends. For other outcomes too there appears to be no relationship between ΔE and their 1904-1909 variations, with the notable exceptions of ΔA, $\Delta elite$ and $\Delta turnout$ (when province-specific shocks are included). The chances to be elected of aristocrats and *elites* were declining faster before the reform in districts with high ΔE. Districts that experienced one standard deviation above the mean in ΔE saw a decrease in $\Delta elite$ between 1904 and 1909 of at least 5%. For an aristocrat the decrease is around 4.5%. Since self-selection into treatment was not an option, we cannot rule out that enfranchisement stopped, at least temporarily, the decline in representation of aristocrat and elite MPs.[57]

Table 7 reports further robustness checks for regressions with Estrema vote share as dependent variable. One concern in this case is that the results could be biased by the presence of an upper bound to the dependent variable. We restrict the sample by removing districts with a high percentage of Estrema votes in 1909. Columns

[56] In the interest of space, for each outcome I only report the results from two specifications: one which includes all control variables (both the 1911 levels and 1901-1911 changes) and one which also includes province-specific shocks.

[57] These results are consistent with the presence of an intra-elite conflict of the following form: suppose that an emerging enfranchised bourgeoisie was increasingly displacing aristocrats and the traditional establishment from parliamentary seats; then, the massive franchise extension of 1912 might have helped some elite members to keep their seats. Whether effects of this sort were anticipated or not makes a big difference for our interpretation of the results but remains unfortunately moot in the absence of further evidence.

(1) and (2) report coefficients when we remove districts in, respectively, the top decile and top quintile of Estrema vote in 1909. In columns (3) and (4) we also exclude districts with an Estrema vote percentage of 100% in 1913 (i.e. when the upper bound is binding). These restrictions (and others, not reported) cause only small changes to the estimated coefficient of enfranchisement, both in magnitude and statistical significance.

The last column of Table 7 introduce male illiteracy rate among the control variables. We have chosen to exclude this variable from our regressions because, since franchise was restricted on literacy grounds, illiteracy rates would absorb part of the causal effect that we are trying to estimate. The 1901-1911 difference in illiteracy rate has instead always been included since this helps identifying a more appropriate counterfactual: franchise would have naturally expanded with literacy even without the reform. In any event, when we introduce illiteracy rate as control, the estimated impact of enfranchisement differs only marginally from our previous estimates. This suggests that it did not matter whether franchise expansion was due to the removal of the literacy barrier or to the removal of other obstacles: literate and illiterate newly enfranchised voters did not behave differently on average.[58]

5 Why so little effect on representation?

5.1 Swing districts

It is puzzling to find that enfranchisement had an average positive effect on *Estrema* vote shares but a negative effect (or at best no effect) on *Estrema* net seat gains. This suggests that votes were gained where not needed and possibly lost where they mattered. That many votes end up making little or no difference is typical of majoritarian single-member districts.

To further investigate this possibility I construct a dummy variable to separate swing from non-swing districts. The swing districts are defined as those satisfying at least one of the following conditions: 1) the elected MP changed from *Estrema* to non-*Estrema* or viceversa in the 1909 election; 2) there was a run-off between an *Estrema* and a non-*Estrema* candidate in 1909; 3) the vote share of parties of the

[58] I also run regressions that include an interaction term between illiteracy and ΔE. There is nothing relevant to report about those regressions (probably also because ΔE and illiteracy rates are highly correlated). Regressions controlling for male illiteracy rate for other outcomes are reported in the Online Appendix.

Estrema in 1909 was between 40% and 60%. The three criteria identify 170 districts that are defined as "swing".[59]

The regressions reported in table 8A show that on average the *Estrema* lost votes in swing districts (columns 1 and 2). An interaction between swing and ΔE has a negative sign (columns 3 and 4): in swing districts ΔE has no impact or a much reduced positive impact on Estrema votes. In terms of seats, although on average the Estrema did not do worse in swing districts than in the others (columns 5 and 6), the negative and statistically significant interaction term (columns 7 and 8) shows that enfranchisement adversely affected the Estrema in swing districts.

5.2 The "Gentiloni pact"

Several candidates in the 1913 election signed a pact with the Catholic Electoral Association led by Conte Ottorino Gentiloni. While the Association was not allowed by the Vatican to have its own candidates, it could provide support to specific candidates committed to Catholic values and policies. Local bishops could also demand from the Vatican a suspension of the *non-expedit* which, if obtained, would allow them to openly support certain candidates.

A detailed reconstruction of the events and a list of the signatories, based on research conducted in the Vatican archives, can be found in Piretti (1994). By using Piretti's list of signatories it is possible to construct a dummy variable "Gentiloni" equal to one in districts where one candidate signed the pact.[60] The impact of enfranchisement might have been different in districts that saw an explicit participation of Catholics and a suspension of the *non-expedit*.

From Table 8B, it is clear that in Gentiloni districts the Estrema performed much worse than in other districts, both in term of votes (columns 1 and 2) and seats (columns 5 and 6). In term of votes, the negative interaction term between the Gentiloni dummy and ΔE signals that the positive impact of enfranchisement on Estrema vote shares was smaller in Gentiloni districts. Since the *non expedit* was suspended both for the former and the newly enfranchised, the negative interaction must be due to a higher concentration of Catholics among the new voters. At the same time the interaction is not statistically significant, which could be interpreted as evidence that newly and formerly enfranchised did not react too differently to the suspension of *non expedit*.

[59] Estimates have been repeated using different ranges for criterion 3. Results are quite robust.
[60] There was never more than one signatory per district.

The same interaction term is positive and far from acceptable statistical significance in the net seat gain regressions (columns 7 and 8). This leads us to conclude that, in practical terms, although the Gentiloni pact had probably a large impact on the election outcome, this impact is the same that it would have obtained without the franchise extension.

In Table 8C we use the swing and Gentiloni dummies to divide the districts into four groups.[61] In swing and Gentiloni districts results for Estrema candidates were catastrophic: vote change was between 15% (with all controls included) and 20% (with province-specific shocks) lower than in non-swing and non-Gentiloni districts. Not surprisingly this resulted in substantial seat losses. Interactions with ΔE show that enfranchisement was beneficial for Estrema candidates only in non-swing and non-Gentiloni districts (columns 3-4) where seat gains failed to materialize (columns 7-8). In other districts the impact of enfranchisement appears instead to have been neutral in terms of Estrema votes and negative in terms of seats.

5.3 Turnout

Most historians consider the Gentiloni pact a reaction of traditional elites to a rapidly expanding *Estrema*.[62] It consisted in the mobilization of self-excluded segments of the population: our conclusion is that a substantial share of newly enfranchised voters were conservative. Results for Estrema candidates were negative, however, also in swing non-Gentiloni districts, which points to the existence of at least another force that worked against the *Estrema*. One factor which is recurrent in newspapers of the time is political violence and intimidation.[63] The vote was, in principle, secret and Estrema supporters could not be forced to vote for other candidates. Anecdotal evidence suggests that they were instead "persuaded" to stay at home on the day of the election. Hence, the Gentiloni pact and political violence should have opposite effects on participation: the first should increase turnout by

[61] It is useful to note that the Gentiloni and the swing dummies are not positively correlated as one could expect. The correlation coefficient is -0.10. The proportion of swing districts in which the Gentiloni pact was signed is 37%. This proportion actually increases to 48% in non-swing districts.

[62] Gentiloni himself regarded the pact as a great success and, ultimately, as the device that preserved the established order.

[63] Articles from a reputable and moderate source like *Corriere della Sera* report numerous instances in which labour organizations were attacked and poor voters were confronted by violent groups that operated under the protection of local police forces. See for example the article "Ricordi di una domenica di passione" by Ugo Ojetti, appeared on *Corriere delle Sera* (November 6, 1913), providing a very detailed recostruction of violence and intimidation in the southern district of Molfetta.

allowing Catholics to go to the polls, the second should decrease it by preventing *Estrema* supporters to do so. In table 9 the dependent variable is the difference between turnout rates in 1913 and 1909. Our results show that turnout decreased more than average in swing districts. Depending on whether we include province-specific shocks or not (and always including all the other controls) being in a swing district decreased turnout by a percentage that ranges between 3.3 and 3.7.

The Gentiloni dummy is also correlated with a relative decline in turnout. The magnitude is smaller compared with the swing dummy and less precisely estimated. Columns (5) and (6) show that decline in turnout was particularly strong in districts that were both swing and Gentiloni and slightly less strong in districts which were swing but not Gentiloni. In any event, compared with non-swing non-Gentiloni districts, all other districts experience larger declines in turnout.

Our analysis seems to indicate that unobserved events that happened in key districts have reduced both turnout and *Estrema* vote shares. In districts where a candidate signed the Gentiloni pact the *Estrema* performed worse than average but the Catholic vote was not sufficient to boost turnout rates. This makes it likely that most Catholic voters were not actually mobilized by local priests, in the sense that they voted (if already enfranchised) or would have voted (if newly enfranchised) anyway. It is more likely that they were instead persuaded to vote for conservative candidates while they would have voted for another candidate in the absence of the Gentiloni pact.

6 Final remarks

The 1912 Italian franchise extension constitutes an ideal setting to study the relationship between democratization and political change. A laggard until then, Italy passed in 1912 a reform that made it suddenly one the countries with the most generous franchise regulations, trebling the electorate and leaving electoral rules and district boundaries unchanged. The reform was passed at a time in which labour unions and democratic and socialist parties were well established political actors, pushing in the direction of radical economic and institutional reforms.

Our empirical study suggests that the political changes associated with the reform were minimal. Although the extreme left saw an increase in vote shares, patterns of legislative representation remained broadly unaffected. Enfranchisement did not increase the number of seats won by the left and did not cause a displacement

of traditional and aristocratic elites from their parliamentary seats.

These findings can hardly be fully reconciled with some of the most influential economic theories of democratization. These theories are based on a one-dimensional representation of societal conflict which corresponds to the economic interests of different groups. A substantial body of evidence is compatible with this view. Some of this evidence is based on historical cross-country analysis, like Lindert (1994, 1996, 2004), Boix (2001) and Aidt and Jensen (2009). Husted and Kenny (1997) present evidence of a positive impact on welfare spending of removing literacy tests and poll taxes in the US states during the period 1950-1988. More generally, an emerging body of empirical literature provides evidence of instances in which democracy is good for the poor (Avelino *et al.* 2004; Stasavage, 2005; Kudamatsu, 2011): this implies that democratization can be used by elites as a commitment device to future pro-poor outcomes.[64]

Our case does not appear to entirely fit this view. On one hand we document an impact of enfranchisement on the vote share of parties with a programme of social reforms. If we assume that different parties stand for different policies then our findings are compatible with the view that the new voters, mostly poor, would disproportionately support the left.[65] However, we also document that parliamentary representation and other political outcomes remained essentially unaffected by universal suffrage. Other findings also do not conform to the idea that economic conflict mechanically translates into political representation when *de jure* political equality is granted.

Our results indicate that there is no mechanical link between democratization and political change, conforming to the claim of Acemoglu and Robinson (2006b) that *"when elites who monopolize de jure political power lose this privilege, they may still exert disproportionate influence in politics by increasing the intensity of their collective action"*. Following this intuition, we provide an analysis of the potential reasons that led the parties of the *Estrema* to benefit from universal suffrage in terms of votes but not in terms of representation. The Gentiloni pact is a prime example of the efforts made by elites to minimize the impact of enfranchisement. In fact, it is even possible that, rather than to generate a progressive policy change, democratization might have been used to please some conservative groups (the clericals):

[64] Some studies, however, do not find relevant differences between democracies and non-democracies (Mulligan et al. 2004; Ross, 2006)

[65] In this respect, the 1912 Italian reform is different from the UK Second Reform Act, which generated little increased support for the Liberal party (Berlinsky and Dewan, 2011).

this would again suggest that economic conflict did not easily translate into political cleavages. From a policy perspective, MPs who had signed the Gentiloni pact committed themselves to a pro-Catholic agenda, which suggests that our findings concern both the political and the policy dimensions.

We show, however, that forces other than the Gentiloni pact must have played an important role in key swing districts, where the decrease in turnout was substantially higher than average. Further investigation is necessary to assess more precisely what happened in these districts, but the patterns we uncover are at least compatible with the idea that elite's anti-*Estrema* efforts (of whatever sort) were particularly strong in key districts and that the ultimate consequences of an institutional reform depended on *de facto* as well as on *de jure* political power.[66]

The list of intriguing questions surrounding the reform that remain to be addressed is too long to be discussed here. For what concerns specifically the present study, at least three issues deserve better investigation. First, we ignored possible behavioral changes that enfranchisement may have induced among the previously enfranchised. In one extreme case, these voters may have changed entirely their behavior, for example because the fear of new voters may have induced more conservative choices. This is not necessarily a problem for our conclusions: the counterfactual to actual 1913 election outcomes is what would have happened in 1913 without universal suffrage. If a change of any sort in the voting behavior of the formerly enfranchised was induced by universal suffrage, then the voting returns of 1909 remain a valid counterfactual. Nevertheless our estimates would not capture anymore the different propensities to vote Estrema among newly and former enfranchised.

A second dimension which has been only partially analyzed is turnout. Our coefficients establish a direct link between registration and outcomes, bypassing the turnout stage. Turnout, however, was different for the formerly and newly

[66] Two other papers provide micro-level quantitative evidence on the consequences of enfranchisement in Western Europe. They both focus on comparatively smaller reforms implemented in the UK. Aidt et. al (2010) study the expansion of the voting franchise in English and Welsh municipalities between 1868 and 1886 and conclude that franchise extension had a retrenchment effect, since demand for local public goods came from urban elite and not from the middle classes. Berlinsky and Dewan (2011) study the UK Second Reform Act and find that franchise extension had no impact on electoral support for the Liberal party. Both papers focus on British reforms that enfranchised only a fraction of the male population. After the Second Reform Act, which enfranchised mainly the urban working classes, only about one third of adult males had the right to vote in Britain. In this sense, while the Lizzeri and Persico franchise extension hypothesis is better reflected in the electoral reforms of 19th century Britain, the Acemoglu and Robinson hypothesis faces a more appropriate test with the 1912 Italian reform.

enfranchised, with the latter less likely to vote. While the political implications of our findings remain unaffected by this consideration, a better understanding of the role of mobilization for effective democratization remains of very practical and theoretical relevance.

A third issue concerns the long term consequences of the reform. Although the impact of *de jure* political equalization on representation could be small in the short run, it may nevertheless trigger a change that manifests its effects only after some time, and in particular when the newly enfranchised voters are sufficiently mobilized and informed. We provide an analysis of the 1919 election in the Appendix. The context is unfortunately not favorable to the study of long run consequences, first because the electoral system changed and then because Italy became a dictatorship only ten years after the reform we study. Other contexts could be more favorable to explore this question with quantitative methods. This remains a very important issue to be addressed by future research.

Appendix 1. Data description

The data used in this paper has been mostly described in Section 3.2. This section provides further details, summary statistics (tables A1-A3) and further descriptive graphs (A1-A4).

All data has been analysed at the electoral district level. With the exception of electoral and biographical data, however, information has not been originally collected at the electoral district level. The Census provides population data by gender at town in both 1901 and 1911. Town-level data can be aggregated into electoral district data in a precise way by using the list of towns belonging to each district (available in the *Archivio Storico della Camera dei Deputati*). Literacy is also available at town level by gender in 1911 and we can therefore recostruct male literacy rates by electoral districts. For all other variables the most detailed territorial level for which they are available is the *circondario*. The Italian territory was divided into 206 *circondario* for Census purposes (this was not an administrative unit). Of 508 electoral districts, 318 were entirely contained within a single *circondario* and the *circondario* variables have been used for those cases. In the remaining 190 cases I have estimated the electoral district variable by using weighted *circondario* data, with weights given by town-level population data. This approximation is plausible since between *circondario* variation is almost certainly larger than within *circondario*

variation. For illiteracy percentage, for example, which is available at town level, the within *circondario* standard deviation is 7.9 while the corresponding between *circondario* measure is 17.9. Using contiguous circondario variables to reconstruct electoral district variables is therefore reasonable, although not immune from measurement error. This is, in any event, the only route to recover a number of social and economic indicators at the electoral district level. To my knowledge this is the first dataset (of any country) that provides detailed socio-economic variables by electoral districts for that period.

Appendix 2. Further results

2.1 The geography of the effect

Italian regions in 1912 differed in a number of important characteristics. The North-West was the most industrialized and richer part of the country. It also had a higher share of agricultural workers who cultivated their own land, while large estates prevailed in the South. The North and some regions of the Centre, both in industrial and agricultural areas, had a better organized labour force, stronger unions and political organizations. Hence, an important step in uncovering heterogeneous effects is to run our regressions with an interaction term between ΔE and area dummies, corresponding to districts in the North-West, North-East, Centre and South. Results are reported in Table A4, which focusses on vote shares of the *Estrema* and on net seat gains of, respectively, *Estrema*, aristocrats and elite. We now include area dummies instead of provinces: columns 1, 3, 5, 7 report the results and show that, although some differences occour, the sign and approximate magnitude of the coefficient of ΔE are not substantively affected by replacing the province-specific shocks with area-specific shocks. Notably, Estrema candidates performed substantially better in the South (the omitted dummy) than in all other areas, particularly the North-East and Centre. This is true both for vore percentages and net seat gains and can be due to the low starting point of Estrema in Southern districts.

In columns 2, 4, 6, 8 we introduce interaction terms between area dummies and ΔE. Results provide yet another unexpected picture of the what happened in the 1913 election. In term of votes, although no interaction term is statistically significant, the magnitudes indicate that enfranchisement benefited the *Estrema* mainly in the South, with a smaller positive effect in the North West and negative effects in the North East and Centre. In terms of net seat gains, the effect was negative everywhere and it is 5% statistically significant in the Centre, where we also

have a positive and significant effect on the net seat gains of aristocrats. Aristocracy and traditional elites appear to have been damaged by enfranchisement only in the South (although the effect is smaller and statistically insignificant in the case of traditional elites). It was instead in the North West that the elite benefited the most and the effect has similar size and direction, although with larger standard errors, in the North East and the Centre. In conclusion, and differently from what most politicians of the time expected, there is nothing suggesting that newly enfranchised Southerners voted more conservatively than in other parts of the country, while the opposite appears more likely.[67]

2.2 Inequality

Inequality is a key variable for theories of democratization. Larger inequality should amplify the consequences of enfranchisement by increasing the redistributive demands of the pivotal voter.

Measuring wealth or income inequality in the electoral districts of 1909-1913 is difficult, since data on income and wealth distribution is not available. There is, however, information that can be used to imperfectly approximate inequality. By using data from 1911 Census we construct the following indicator:

$$Inequality = \frac{[\%\text{ agricultural workers who do not own land} + \%\text{ blue collar industrial workers}]}{\%\text{ owners of real estate property}}$$

The numerator represents the percentage of employees not owning their means of production, while the denominator approximates the diffusion of property. As the percentage of real estate owners increases we assume property is more diffused and inequality goes down. When instead larger shares of the population are employed in unskilled jobs and do not own their means of production we assume inequality goes up. Both assumptions could clearly be wrong for many reasons: for example because there is no upper bound to how much the richest could earn or own and our index contains no information about that. Although this indicator would be inappropriate to capture inequality in a developed society, where property is diffused and employees' salaries absorb a consistent share of the output, it is probably less so for Italy 1911, when only about 10% of the population owned real estate and salaries where not far from subsistence levels.

[67] There appears to have been no significant difference between urban and rural areas. An interaction between ΔE and the proportion of population living in urban areas turns out to be always far from any acceptable statistical significance. Results are not reported in the interest of space but are available from the author.

Table A5 reports regression coefficients where our inequality indicator, normalized to be between 0 and 1, is included both as a direct effect and interacted with ΔE. Results show that the parties of the *Estrema* gained votes in more unequal districts. The interaction between inequality and enfranchisement, however, is negative, both for Estrema votes and seats. Although the standard errors are such that we cannot rule out the possibility of no effect, the sign of the interaction term is opposite to what the Meltzer and Richard model would predict. These finding are instead more compatible with the view that inequality may have facilitated elite's "capture" of poor voters. In regressions using ΔA and $\Delta elite$ as dependent variables, inequality is statistically insignificant and does not interact with enfranchisement.

2.3 The election of 1919

To study the long run consequences of the 1912 reform is difficult. Between the 1913 and the subsequent (1919) election, World War I brings dramatic social and political change. In the early twenties the advent of fascism makes elections irrelevant and political parties (except the fascist) are eventually outlawed. For the purpose of our exercise an important obstacle to long run comparisons is a new reform, passed in 1919, that introduces proportional representation and re-draws district boundaries reducing them from 508 to 54. The 1919 reform also introduces full univeral manhood suffrage, extending the franchise to those adults aged 21-30 and still subject to literacy and census restrictions. This reform makes the 1919 election not directly comparable with previous ones.

In this section, with all the necessary caveats, we use the 1919 electoral districts as observation units and compare 1919 results with the results obtained in 1909 and 1913 within the 1919 electoral districts boundaries. This task is facilitated by the fact that 1919 electoral districts follow province boundaries[68] and each pre-1919 electoral district is also entirely contained within a province. The comparison is therefore based on real and not notional data, although the process that generates the data is now different.

Results are presented in table A6. In the first two columns the dependent variable is the 1909-1919 difference in *Estrema* vote share and the main explanatory variable is ΔE_+ calculated as $\frac{E_i^{19}-E_i^{09}}{E_i^{19}}$. In other terms, we study the overall effect of the 1913 and the 1919 reforms. All regressions include the same controls used previously,

[68] The 69 provinces were aggregated into 54 districts by including more than one province in some districts, but never by cutting province boundaries. Data on electoral results of the 1919 elections are taken from Istituto Nazionale di Statistica (1946) and Caramani (1999).

this time calculated using the 1911 and 1921 Censuses. Results show an overall anti-*Estrema* effect of enfranchisement.

Columns (3) and (4) separate the effect of the 1912 reform from that of the 1919 enfranchisement. This is done by using two explanatory variables, $\Delta E_1 = \frac{E_i^{13} - E_i^{09}}{E_i^{19}}$ and $\Delta E_2 = \frac{E_i^{19} - E_i^{13}}{E_i^{19}}$. Given that these indicators span a 10-year period, the assumptions for their reliability are now more likely to be violated. They should nevertheless provide a rough indication of the share of 1919 voters who were enfranchised, respectively, in 1912 and 1919. Both specifications (without and with area dummies) display negative coefficients, with the effect being particularly strong (and statistically significant) in the case of ΔE_1.

Columns 5-8 repeat the same exercise using the net seat gains of *Estrema* candidates in the 1919 electoral districts as dependent variable. In this case enfranchisement effects are never statistically significant, showing that the impressive gains in seats of *Estrema* candidates in the 1919 election (the Socialist party increased its MPs from 78 to 156) have no direct link with the two franchise extensions.

Although, for the many reasons discussed above, these estimates should be taken with caution, they appear to suggest that, in spite of the prevailing account given in most history books, enfranchisement did not benefit the *Estrema*. A slightly longer perspective suggests that the overall extension of the franchise, through the 1912 and the 1919 reforms, had at best no implication in terms of legislative representation. The switch to a proportional system, as well as a general upward trend independent of enfranchisement, are likely to be responsible for the large gains of Estrema in the 1919 parliament.

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Table 1: Correlates of enfranchisement

Dep. variable	ΔE	ΔE	$\Delta E_{(t-1)}$	$\Delta E_{(t-1)}$	ΔE	ΔE	ΔE	ΔE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
male illiteracy 1911 (%)	0.4696***	0.4927***	-0.0341**	-0.0119				
	(0.0211)	(0.0273)	(0.0172)	(0.0262)				
vote percentage Estrema 1909					-0.0041***	-0.0024***		
					(0.0003)	(0.0003)		
vote percentage Estrema 1904							-0.0964***	-0.0502***
							(0.0162)	(0.0121)
industrial workers (% population) 1911		-0.3406***		0.0669	-0.7027***	-0.2999**	-0.9842***	-0.4323**
		(0.1125)		(0.0880)	(0.1086)	(0.1472)	(0.1299)	(0.1852)
urbanized (% population) 1911		-0.0563***		0.0123	0.0257	-0.0750***	0.0250	-0.0813***
		(0.0201)		(0.0199)	(0.0234)	(0.0237)	(0.0271)	(0.0259)
agricultural workers own land (% pop) 1911		-0.0973		-0.0048	-0.9981***	-0.3661	-1.2384***	-0.3196
		(0.1229)		(0.1111)	(0.1120)	(0.2700)	(0.1355)	(0.3125)
agr. workers not own land (% pop) 1911		-0.3138***		0.0824	0.1305*	0.2918**	0.0098	0.2300*
		(0.0629)		(0.0636)	(0.0705)	(0.1130)	(0.0851)	(0.1243)
property of real estate (% population) 1911		-0.0934		0.0518	0.4928***	-0.1929	0.5383***	-0.2831*
		(0.0933)		(0.0897)	(0.1086)	(0.1612)	(0.1227)	(0.1708)
logarithm population 1911		4.5583*		3.0853	10.2107***	12.6074***	5.5723**	9.9833***
		(2.3802)		(2.0072)	(2.4891)	(2.3591)	(2.4879)	(2.2768)
(log pop 1911 - log pop 1901)		22.4079***		17.4587***	9.4654	4.2078	13.3898*	3.7828
		(7.5874)		(5.9459)	(7.2184)	(8.4696)	(7.8097)	(8.7122)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls (1901-1911 differences)	No	No	No	No	Yes	Yes	Yes	Yes
Province Fixed Effects	No	No	No	No	No	Yes	No	Yes
Observations	508	508	508	508	508	508	508	508
R-squared	0.5472	0.6207	0.0076	0.0719	0.5788	0.8128	0.4823	0.7900

Note: see text and Appendix for data description. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses

Table 2: The effect of enfranchisement on the vote percentage of Estrema candidates

	Dep. variable: vote percentage change (1909-1913) of Estrema candidates					
	(1)	(2)	(3)	(4)	(5)	(6)
Enfranchisement (1909-1913)	0.2509***	0.1771**	0.1672*	0.2943**	0.2515*	0.2533*
	(0.0756)	(0.0818)	(0.1004)	(0.1478)	(0.1437)	(0.1423)
Vote percentage change of Estrema candidates (1904-1909)					-0.2126***	-0.5456
					(0.0547)	(0.3381)
Vote change (1904-1909) x Enfranchisement (1909-1913)						0.0048
						(0.0050)
Constant	-8.9633*	-4.8679	-44.5906	-66.5627	-76.3220	-74.8758
	(4.7169)	(6.1427)	(62.5930)	(66.4891)	(67.1687)	(67.1198)
Controls (differences 1901-1911)	no	yes	yes	yes	yes	yes
Controls (1911 levels)	no	no	yes	yes	yes	yes
Province fixed effects	no	no	No	yes	yes	yes
Observations	508	508	508	508	508	508
R-squared	0.0167	0.0327	0.0483	0.2555	0.2891	0.2907

Notes. Control variables in 1901-1911 differences include: natural logarithm of population, percentage of male population above six which is illiterate, percentage population employed in industry, percentage of agricultaral workers (owning land), percentage of agricultural workers (not owning land), percentage of the population that owns real estate. Controls introduced as 1911 levels include all the above controls exept illiteracy rate and adds the percentage of the population living in urban areas. Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%

Table 3. The effect of enfranchisement on the Estrema net gain of seats

Dependent variable	Estrema net gain of seats					gained	lost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enfranchisement (1909-1913)	-0.0022	-0.0029	-0.0044*	-0.0100***	-0.0080**	0.0007	0.0070*
	(0.0018)	(0.0020)	(0.0025)	(0.0036)	(0.0035)	(0.0028)	(0.0036)
Estrema % in 1909		0.0016	0.0025	0.0004	0.0024	0.0002	-0.0044*
		(0.0020)	(0.0021)	(0.0024)	(0.0024)	(0.0040)	(0.0253)
Estrema % in 1909 (squared)		-0.0001**	-0.0001***	-0.0000	-0.0000	0.0001	-0.0002
		(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0002)
Controls	no	no	yes	yes	yes	yes	yes
Province fixed effects	no	no	no	yes	yes	no	no
Lagged d.v. and interaction	no	no	no	no	yes	yes	yes
Observations	508	508	508	508	508	393	115
R-squared	0.0033	0.0351	0.0726	0.2444	0.3222	0.1793	0.2835

Notes. Control variables in 1901-1911 differences include: natural logarithm of population, percentage of male population above six which is illiterate, percentage population employed in industry, percentage of agricultaral workers (owning land), percentage of agricultural workers (not owning land), percentage of the population that owns real estate. Controls introduced as 1911 levels include all the above controls exept illiteracy rate and adds the percentage of the population living in urban areas. Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%

Table 4. Did enfranchisement cause the displacement of traditional elites from parliament?

Dependent variable	Aristocratic elite only			All traditional elites		
	(1)	(2)	(3)	(4)	(5)	(6)
Enfranchisement (1909-1913)	0.0020	0.0027	0.0016	0.0027	0.0044	0.0022
	(0.0017)	(0.0042)	(0.0039)	(0.0019)	(0.0044)	(0.0042)
Elite net seat gain (1904-1909)			-0.4084			-0.4214
			(0.3319)			(0.3039)
Elite net seat gain (1904-1909) x Enfranchisement (1909-1913)			0.0043			0.0037
			(0.0047)			(0.0043)
Constant	-0.1274	-0.2149	-0.0626	-0.2074	-0.6906	-0.4335
	(0.1234)	(1.2294)	(1.2195)	(0.1360)	(1.3793)	(1.3495)
Controls (differences 1901-1911)	yes	yes	yes	yes	yes	yes
Controls (1911 levels)	no	yes	yes	no	yes	yes
Province fixed effects	no	yes	yes	no	yes	yes
Observations	508	508	508	508	508	508
R-squared	0.0065	0.1289	0.1433	0.0083	0.1227	0.1469

Notes. Control variables in 1901-1911 differences include: natural logarithm of population, percentage of male population above six which is illiterate, percentage population employed in industry, percentage of agricultaral workers (owning land), percentage of agricultural workers (not owning land), percentage of the population that owns real estate. Controls introduced as 1911 levels include all the above controls exept illiteracy rate and adds the percentage of the population living in urban areas. Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: The effect of enfranchisement on Estrema candidacies, on electoral competition and on turnout

Dependent variable	Estrema candidacy			Herfindhal-Hirshman index			Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Enfranchisement (1909-1913)	0.0057***	0.0026	0.0029	-0.0012	-0.0009	-0.0012	-0.3066***	-0.2438***	-0.1416*
	(0.0015)	(0.0028)	(0.0023)	(0.0008)	(0.0014)	(0.0012)	(0.046)	(0.088)	(0.075)
Controls (differences 1901-1911)	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls (1911 levels)	no	yes	yes	no	yes	yes	no	yes	yes
Province fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
1904-1909 change in the dep. var.and its interaction with enfranchisement (1909-1913)	no	no	yes	no	no	yes	no	no	yes
Observations	508	508	508	508	508	508	508	508	508
R-squared	0.0389	0.2306	0.4475	0.0304	0.2173	0.4352	0.1369	0.3160	0.5268

Notes. All regressions include a constant term. Control variables in 1901-1911 differences include: natural logarithm of population, percentage of male population above six which is illiterate, percentage population employed in industry, percentage of agricultaral workers (owning land), percentage of agricultural workers (not owning land), percentage of the population that owns real estate. Controls introduced as 1911 levels include all the above controls exept illiteracy rate and adds the percentage of the population living in urban areas. Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. Placebo treatment on 1904-1909 changes

Dep. Variable	Votes	Votes	Seats	Seats	Arist.	Arist	Elite	Elite	Cand.	Cand.	HHI	HHI	Turnout	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
ΔE	-0.0002	-0.2014	0.0003	0.0019	-0.0037**	-0.0078**	-0.0042**	-0.0114***	-0.0017	-0.0011	-0.0003	-0.0006	0.0456	0.1885**
	(0.0963)	(0.1645)	(0.0020)	(0.0032)	(0.0017)	(0.0034)	(0.0019)	(0.0034)	(0.0017)	(0.0030)	(0.0009)	(0.0014)	(0.0601)	(0.0815)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Province FE	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	508	508	508	508	508	508	508	508	508	508	508	508	508	508
R-squared	0.0385	0.1727	0.0480	0.2083	0.0342	0.1652	0.0325	0.2009	0.0563	0.2077	0.0398	0.1756	0.0827	0.2724

Note. The dependent variables in column 1-6 refers to the Estrema and it is, respectively, the Estrema 1904-1909 difference in percentage of votes (columns 1-2). the Estrema 1904-1909 net seat gain (columns 3-4), the Estrema 1904-1909 candidacy. The dependent variable in columns 7-8 is the Herfindhal-Hirshman index of electoral competition. Columns 9 and 10 refer to the net seat gain (1904-1909) of Aristocrats and columns 9-10 to the elite (1904-1909) net seat gain. Control variables include both 1911 levels and 1901-1911 differences as described in the Notes to Table 2. All regressions include a constant. Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%

Table 7. Effect of enfranchisement on vote share: further robustness checks

Dep. Variable	Δestrema (1913)	Δestrema (1913)	Δestrema (1913)	Δestrema (1913)	Δestrema (1913)
	(1)	(2)	(3)	(4)	(5)
Enfranchisement (1913)	0.2643*	0.2898**	0.2473*	0.2496*	0.2038
	(0.1449)	(0.1432)	(0.1290)	(0.1230)	(0.1415)
Illiteracy rate 1911					0.4877
					(0.3072)
Controls and province specific shocks	yes	yes	yes	yes	yes
Sample	excluding top decile of Estrema09	excluding top quintile of Estrema09	Estrema13<100 and excluding top decile of Estrema09	Estrema13<100 and excluding top quintile of Estrema09	All
Observations	457	406	439	395	508
R-squared	0.2756	0.3192	0.3060	0.3422	0.2940

Notes. All regressions include a constant and control variables, both 1911 levels and 1901-1911 differences as described in the Note to Table 2. Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%

Table 8. The effect of enfranchisement in swing and Gentiloni districts

A

Dependent variable	Estrema vote percentage change				Estrema net gain of seats			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
swing	-5.4429**	-4.7824*	18.3736*	12.582	0.0035	-0.0169	0.6307**	0.5194*
	(2.413)	(2.601)	(10.621)	(11.127)	(0.054)	(0.058)	(0.284)	(0.305)
Enfranchisement			0.2846**	0.3713**			0.0009	-0.0053
			(0.128)	(0.172)			(0.002)	(0.004)
Enfranchisement x Swing			-0.3610**	-0.2614			-0.0099**	-0.0086*
			(0.179)	(0.187)			(0.004)	(0.005)
Province fixed effects	no	yes	no	yes	no	yes	no	yes
Observations	508	508	508	508	508	508	508	508
R-squared	0.0553	0.2573	0.0646	0.264	0.0367	0.2163	0.0561	0.2383

B

Dependent variable	Estrema vote percentage change				Estrema net gain of seats			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gentiloni	-10.0659**	-8.756***	-3.8520	4.5597	-0.3573***	-0.3564***	-0.4339**	-0.4847**
	(2.0428)	(2.2577)	(8.7963)	(9.8152)	(0.0371)	(0.0421)	(0.2142)	(0.2359)
Enfranchisement			0.2369*	0.4014**			-0.0029	-0.0085**
			(0.1208)	(0.1642)			(0.0027)	(0.0035)
Enfranchisement x Gentiloni			-0.0976	-0.2051			0.0012	0.0020
			(0.1437)	(0.1586)			(0.0032)	(0.0035)
Province fixed effects	no	yes	no	yes	no	yes	no	yes
Observations	508	508	508	508	508	508	508	508
R-squared	0.0874	0.2769	0.0939	0.2851	0.1796	0.3306	0.1822	0.3402

C

Dependent variable	Estrema vote percentage change				Estrema net gain of seats			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gentiloni & Swing	-19.6186**	-15.4644**	25.1587*	18.7283	-0.4907***	-0.4609***	0.4207	0.1303
	(3.299)	(3.446)	(14.384)	(15.020)	(0.072)	(0.073)	(0.443)	(0.441)
Gentiloni & Not Swing	-7.8237***	-7.4826***	2.7967	21.5370	-0.2198***	-0.2508***	-0.0832	-0.0385
	(2.434)	(2.690)	(11.969)	(13.687)	(0.035)	(0.044)	(0.200)	(0.235)
Not Gentiloni & Swing	-2.9334	-3.2433	18.9947	24.7907*	0.1462**	0.1074	0.7487**	0.7563**
	(3.206)	(3.379)	(13.271)	(14.461)	(0.064)	(0.068)	(0.321)	(0.316)
Enfr. x Gent & Swing			-0.3162	0.0555			-0.0118*	-0.0124*
			(0.199)	(0.239)			(0.006)	(0.006)
Enfr. x Gent & Not Swing			0.2189	0.1424			0.0006	-0.0061*
			(0.150)	(0.193)			(0.002)	(0.003)
Enfr. x Not Gent & Swing			0.0533	0.1572			-0.0068*	-0.0133***
			(0.173)	(0.201)			(0.004)	(0.004)
Enfr. x Not Gent & Not Swing			0.3782**	0.5740***			0.0026	-0.0031
			(0.156)	(0.213)			(0.003)	(0.004)
Province fixed effects	no	yes	no	yes	no	yes	no	yes
Observations	508	508	508	508	508	508	508	508
R-squared	0.1108	0.2867	0.1250	0.2999	0.2188	0.3572	0.2391	0.3780

Notes. Swing is a dummy variable defined in the text (Section 5.1). Gentiloni is a dummy variable equal to 1 in electoral districts where one of the candidates signed the Gentiloni pact. All regressions include a constant and all control variables (both 1901-1911 differences and 1901 levels). Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%. Control variables include both 1911 levels and 1901-1911 differences as described in the notes in table 4. Swing is defined in section 5.1

Table 9: Turnout in swing and Gentiloni electoral districts

Dep. variable	Change in turnout percentage (1909-1913)					
	(1)	(2)	(3)	(4)	(5)	(6)
swing	-3.2667***	-3.7230***				
	(1.109)	(1.250)				
gentiloni			-1.2143	-2.5504**		
			(1.1346)	(1.1672)		
swing and gentiloni					-5.6498***	-7.3711***
					(1.6931)	(1.7110)
swing and not gentiloni					-4.2071***	-4.0713***
					(1.4995)	(1.4650)
not swing and gentiloni					-1.6433	-2.4650*
					(1.4329)	(1.4670)
Constant	-19.4387	15.5300	-5.4009	31.4087	-21.6995	17.9607
	(35.143)	(38.620)	(35.685)	(40.460)	(35.1365)	(38.6602)
Province fixed effects	No	Yes	No	Yes	No	No
Observations	508	508	508	508	508	508
R-squared	0.1198	0.3190	0.1068	0.3119	0.1285	0.3318

Notes: All regressions include all control variables (both 1911 levels and 1901-1911 differences as described in the notes to table 4). Swing is defined in Section 5.1. Gentiloni is a dummy variable equal to 1 in electoral districts where one of the candidates signed the Gentiloni pact. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A1: The parties of the Estrema between 1900 and 1913

Party	Year	number of districts with at least one candidate	average vote per district (%)	total national vote (%)	seats
Socialists	1900	161	9.95	13.01	32
	1904	377	17.01	20.85	27
	1909	234	14.17	18.59	40
	1913	351	20.91	23.02	78
Republicans	1900	68	6.69	6	29
	1904	77	4.34	4.26	21
	1909	50	4.43	4.35	23
	1913	67	3.5	3.52	17
Radicals	1900	76	6.77	6.81	36
	1904	116	9.32	9.08	32
	1909	130	10.98	11.57	53
	1913	150	12.78	12.35	73

Table A2. Aristocrats and elite in the *Camera dei Deputati*

	1904	1909	1913
Aristocrat	97	91	88
Landowners	27	23	27
Military	22	19	18
Diplomatic	10	6	8
Dynasty	54	44	36
Total traditional elites	146	134	127

Notes: data collected from the biographies contained in Malatesta (1940). Some MPs belong to more than one group, hence the total number of MPs of elite background does not correspond to the sum of members in each group.

Table A3: Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Enfranchisement (1909-13)	66.134	11.929	19.634	87.159
Enfranchisement (1904-09)	12.99133	7.334	-29.490	43.991
vote percentage change of Estrema candidates (1909-1913)	7.632	23.142	-67.907	100.000
vote percentage change of Estrema candidates (1904-1909)	-1.101	21.922	-99.458	90.258
Δseats Estrema (1909-1913)	0.104	0.452	-1.000	1.000
Δseats Estrema (1904-1909)	0.051	0.394	-1.000	1.000
Δcandidacy Estrema (1904-1909)	0.120	0.465	-1.000	1.000
Δcandidacy Estrema (1904-1909)	-0.148	0.457	-1.000	1.000
ΔHHI (1909-1913)	-0.062	0.229	-0.755	0.630
ΔHHI (1904-1909)	0.021	0.208	-0.517	0.641
Δaristocrat (1909-1913)	-0.006	0.341	-1.000	1.000
Δaristocrat (1904-1909)	-0.012	0.349	-1.000	1.000
Δelite (1909-1913)	-0.016	0.402	-1.000	1.000
Δelite (1904-1909)	-0.020	0.407	-1.000	1.000
gentiloni	0.441	0.497	0.000	1.000
inequality	0.199	0.166	0.042	1.000
male illiteracy rate 1911	33.613	18.791	4.000	68.753
swing district	0.445	0.497	0.000	1.000
North-West	0.293	0.456	0.000	1.000
North-East	0.098	0.298	0.000	1.000
Centre	0.236	0.425	0.000	1.000
South	0.372	0.484	0.000	1.000

Notes: The number of observations is 508 for all variables. North-West includes Sardegna, Piemonte, Lombardia, Liguria. North-East includes Veneto (which also incuded current Friuli-Venezia Giulia); Centre includes all the remaining regions with the exception of the former Kingdom of Naples, which constitutes the South. All other variables are defined in the main text.

Table A4. The geographic distribution of the effects of enfranchisement

Dependent variable	Estrema vote percentage change	Estrema vote percentage change	Estrema net seat gain	Estrema net seat gain	aristocrat net seat gain	aristocrat net seat gain	elite net seat gain	elite net seat gain
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔE	0.0203		-0.0067**		0.0035		0.0047*	
	(0.103)		(0.003)		(0.002)		(0.003)	
NW	-10.6636**	8.8746	-0.1964**	-0.3042	0.0387	-1.0005**	0.0767	-0.9040*
	(4.388)	(28.771)	(0.083)	(0.522)	(0.059)	(0.404)	(0.070)	(0.529)
NE	-15.5802***	27.3665	-0.2291**	0.0896	0.0869	-1.0229**	0.1268	-0.7802
	(5.419)	(36.696)	(0.097)	(0.687)	(0.077)	(0.505)	(0.086)	(0.582)
C	-14.7010***	28.3717	-0.1990**	-0.0395	0.0303	-1.1920***	0.0367	-0.9417*
	(4.510)	(32.224)	(0.081)	(0.565)	(0.060)	(0.416)	(0.066)	(0.556)
ΔE x NW		0.0928		-0.0053		0.0042		0.0060*
		(0.118)		(0.003)		(0.003)		(0.003)
ΔE x NE		-0.2891		-0.0122		0.0053		0.0048
		(0.359)		(0.008)		(0.006)		(0.006)
ΔE x C		-0.2725		-0.0094**		0.0069*		0.0059
		(0.260)		(0.005)		(0.004)		(0.004)
ΔE x S		0.3454		-0.0070		-0.0101**		-0.0076
		(0.387)		(0.007)		(0.005)		(0.007)
Constant	-45.7972	-72.8334	0.8935	0.8718	-0.2176	0.8005	-0.3346	0.5422
	(55.604)	(62.864)	(1.227)	(1.354)	(0.987)	(1.009)	(1.148)	(1.269)
Observations	508	508	508	508	508	508	508	508
R-squared	0.0786	0.0838	0.0878	0.0897	0.0168	0.0311	0.0168	0.0249

*** p<0.01, ** p<0.05, * p<0.1. Robust standar errors in parentheses. See Table A.6 for a definition of geographic areas. Control variables include all the level and differences controls as described in the note to Table 2. Columns (3) and (4) also include Estrema vote percentage in 1909 and its square.

Table A5. The effect of enfranchisement at different levels of inequality

Dependent variable	Estrema vote percentage change	Estrema vote percentage change	Estrema net seats gain	Estrema net seats gain	aristocrat net seat gain	aristocrat net seat gain	elite net seat gain	elite net seat gain
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔE	0.2542*	0.4475*	-0.0010	-0.0059	0.0022	-0.0001	0.0018	0.0007
	(0.1342)	(0.2360)	(0.0033)	(0.0052)	(0.0026)	(0.0044)	(0.0029)	(0.0049)
inequality	23.4068*	25.0922	1.0449**	0.6152	-0.1795	-0.3932	-0.1912	-0.2841
	(13.2690)	(21.4721)	(0.4567)	(0.6068)	(0.7493)	(0.8811)	(0.7571)	(0.8974)
ΔE x inequality	-0.3119	-0.3395	-0.0117*	-0.0093	0.0018	0.0065	0.0060	0.0104
	(0.2435)	(0.2953)	(0.0070)	(0.0084)	(0.0111)	(0.0126)	(0.0114)	(0.0133)
Constant	-57.5301	-72.4084	0.0886	-1.2805	-0.0259	-0.0810	-0.1446	-0.3882
	(63.8313)	(67.8704)	(1.2341)	(1.3878)	(0.9659)	(1.2199)	(1.1392)	(1.3843)
Control variables	yes	yes	yes	yes	yes	yes	yes	yes
Province fixed effects	no	yes	no	yes	no	yes	no	yes
Observations	508	508	508	508	508	508	508	508
R-squared	0.0504	0.2569	0.0855	0.2468	0.0150	0.1309	0.0153	0.1275

Robust standard errors in parentheses. Significant at 10%; ** significant at 5%; *** significant at 1%. Control variables include both 1911 levels and 1901-1911 differences as described in the notes to Table 2. Inequality is defined in Section 1.2 of the Online Appendix. Columns (3) and (4) also include Estrema vote percentage in 1909 and its square.

Table A6. Enfranchisement and the 1919 election

Dependent variable	vote percentage change (1909-1919) of Estrema candidates				Estrema net seat gain (1909-1919)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enfranchisement (1909-1919)	-0.5282	-0.8375*			-0.0725	-0.0603		
	(0.402)	(0.428)			(0.060)	(0.073)		
Enfranchisement (1909-1913)			-0.7828*	-0.9932**			-0.0937	-0.0755
			(0.394)	(0.459)			(0.059)	(0.070)
Enfranchisement (1913-1919)			0.1777	-0.2255			-0.0140	-0.0006
			(0.810)	(0.917)			(0.102)	(0.132)
Constant	42.9038	75.6047	44.2961	75.0065	7.4609	6.5540	7.5763	6.4956
	(44.259)	(48.293)	(42.880)	(48.847)	(7.125)	(7.831)	(7.043)	(7.879)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	54	54	54	54	54	54	54	54
R-squared	0.1822	0.2492	0.2061	0.2623	0.4779	0.4891	0.4844	0.4941

Notes: The definition of the three enfranchisement variables are given in the text in Section 4.5. Area dummies are defined in the Notes to Table 13. Control variables are the same included in other regressions, both in 1921-1911 differences and in 1911 levels. In this case, instead of the percentage of urban population for 1911 we have the population density both in differences and in its 1911 level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

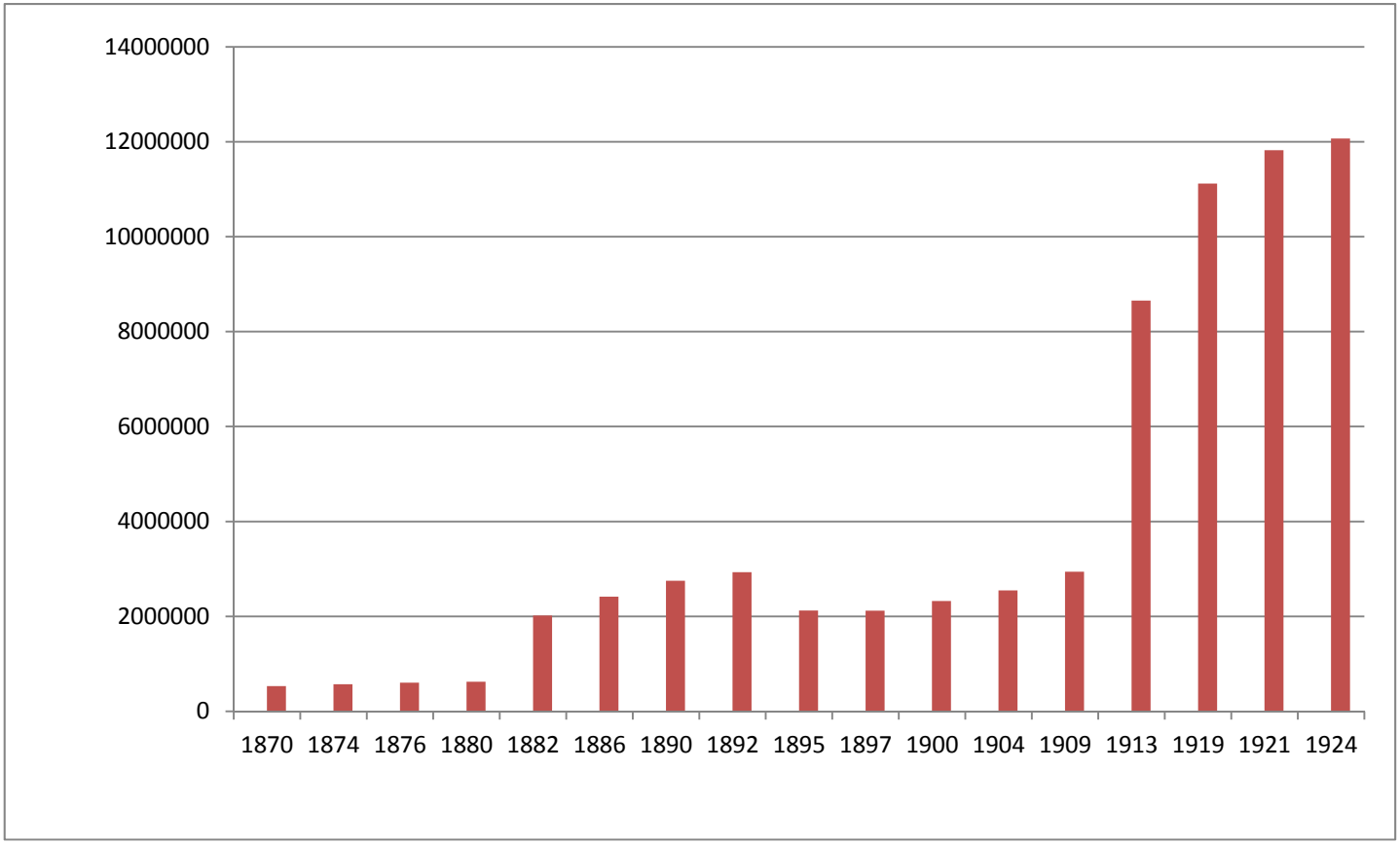


Figure 1: number of registered voters in Italy (1870-1924)

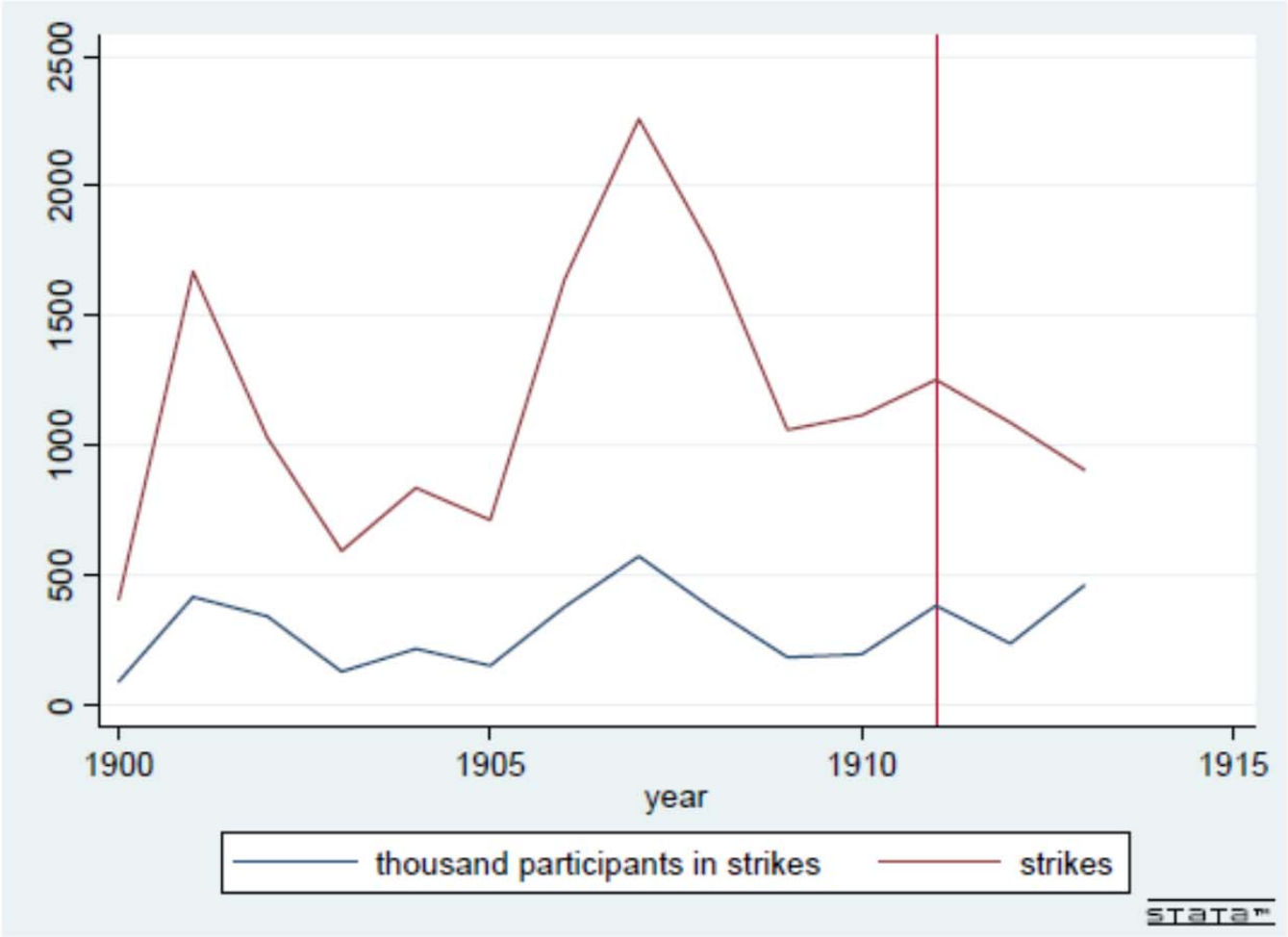
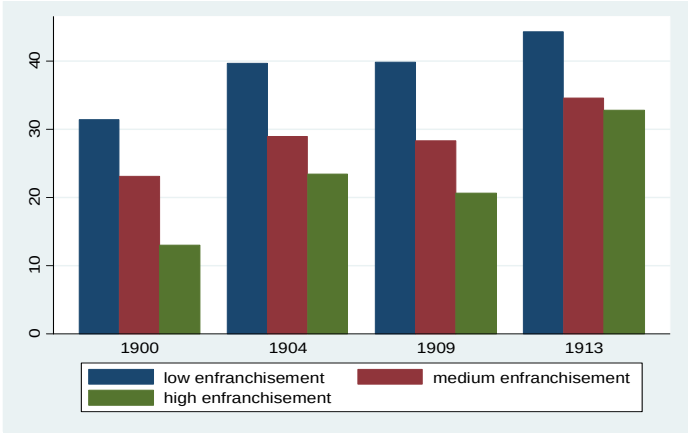
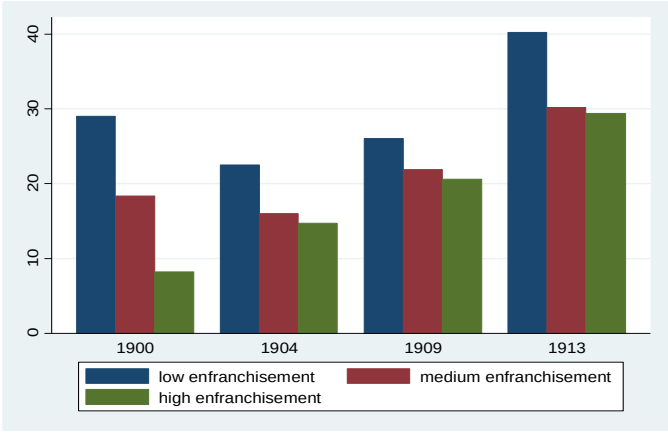


Figure 2. Number of strikes and participants in strikes (1900-1913) (the red line indicates 1911, when the electoral reform was proposed)

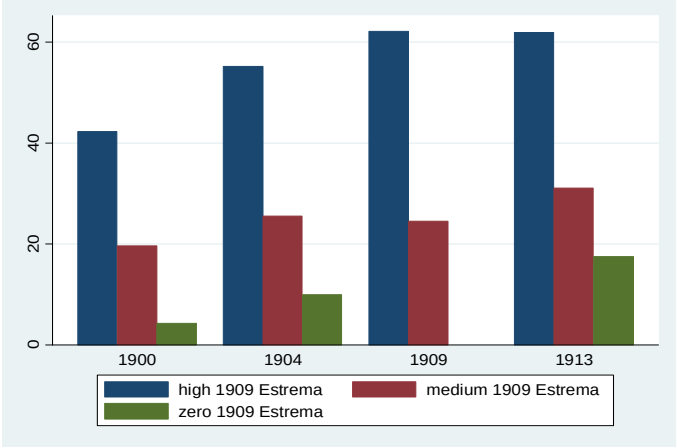
Source: Ministero dell'agricoltura, industria e commercio. Direzione generale della statistica: *Statistica degli scioperi avvenuti nell'industria e nell'agricoltura* (various years).



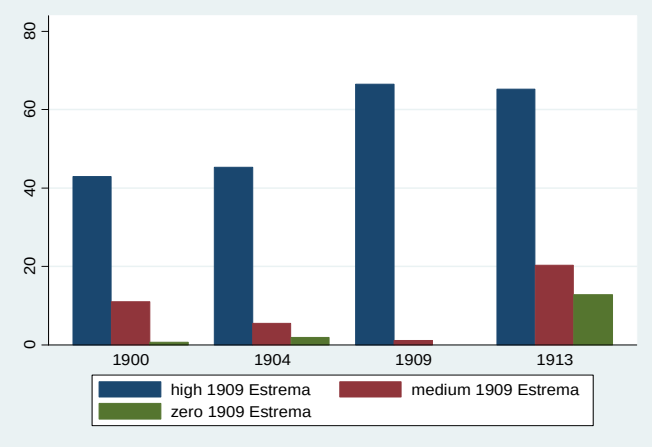
(a) Vote share of estrema for different tertiles of ΔE



(b) Seat share of Estrema for different tertiles of ΔE



(c) Vote share of Estrema by Estrema strength in 1909



(d) Seat share of Estrema by Estrema strength in 1909

Figure 3: Estrema vote and seat share at different tertiles of enfranchisement and Estrema strength

In figures (a) and (b) the districts are divided in three groups of equal size: low enfranchisement refers to the tertile with lowest ΔE, medium enfranchisement to the second tertile, high enfranchisement to the third tertile. In figures (c) and (d) the districts are divided according to their vote share in 1909: the bottom group ("zero 1909 Estrema") is given by 156 districts (almost 1/3 of districts) in which the Estrema had 0 votes in 1909, "high 1909 Estrema" refers to the top tertile of Estrema vote share in 1909, "medium 1909 Estrema" to all remaining districts.

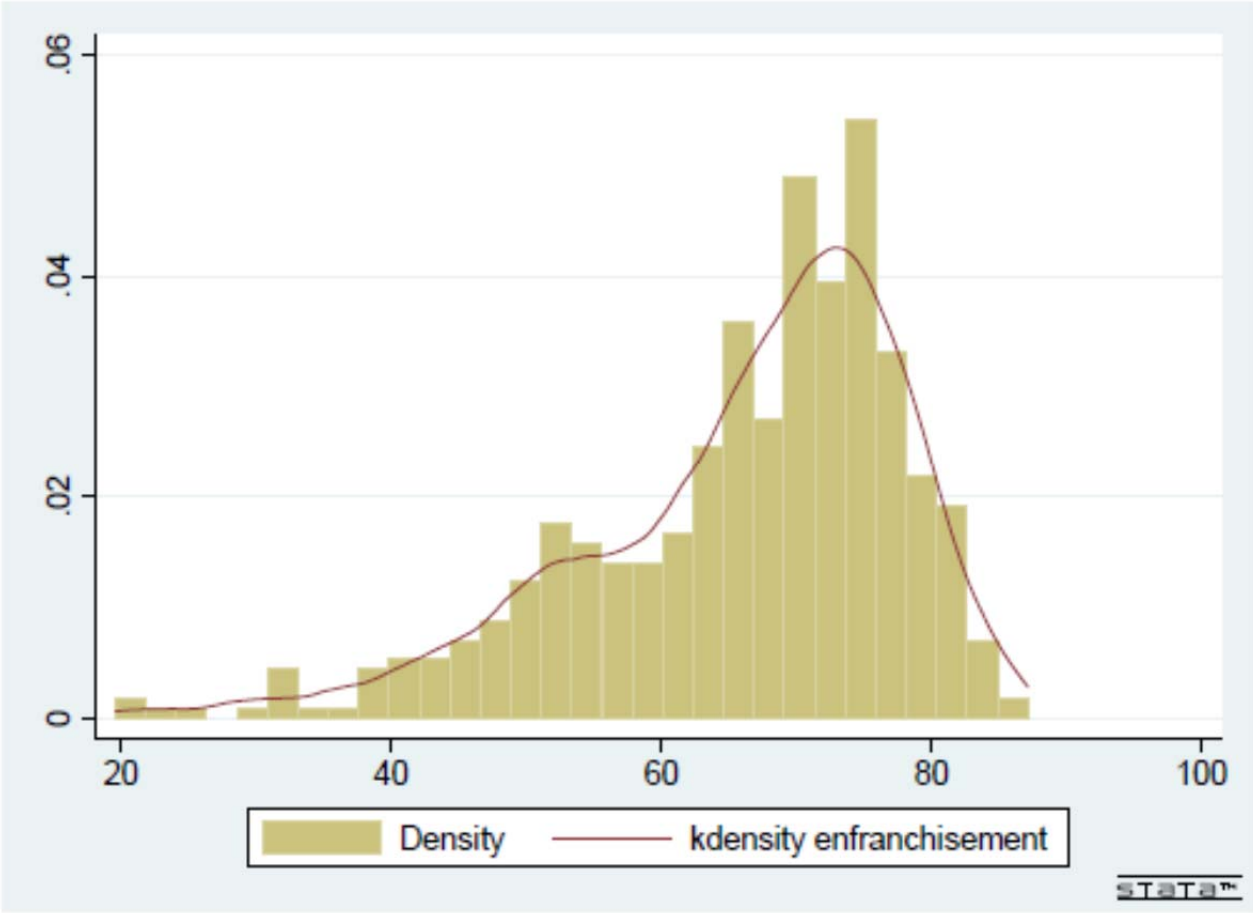


Figure A1. The distribution of ΔE across electoral districts

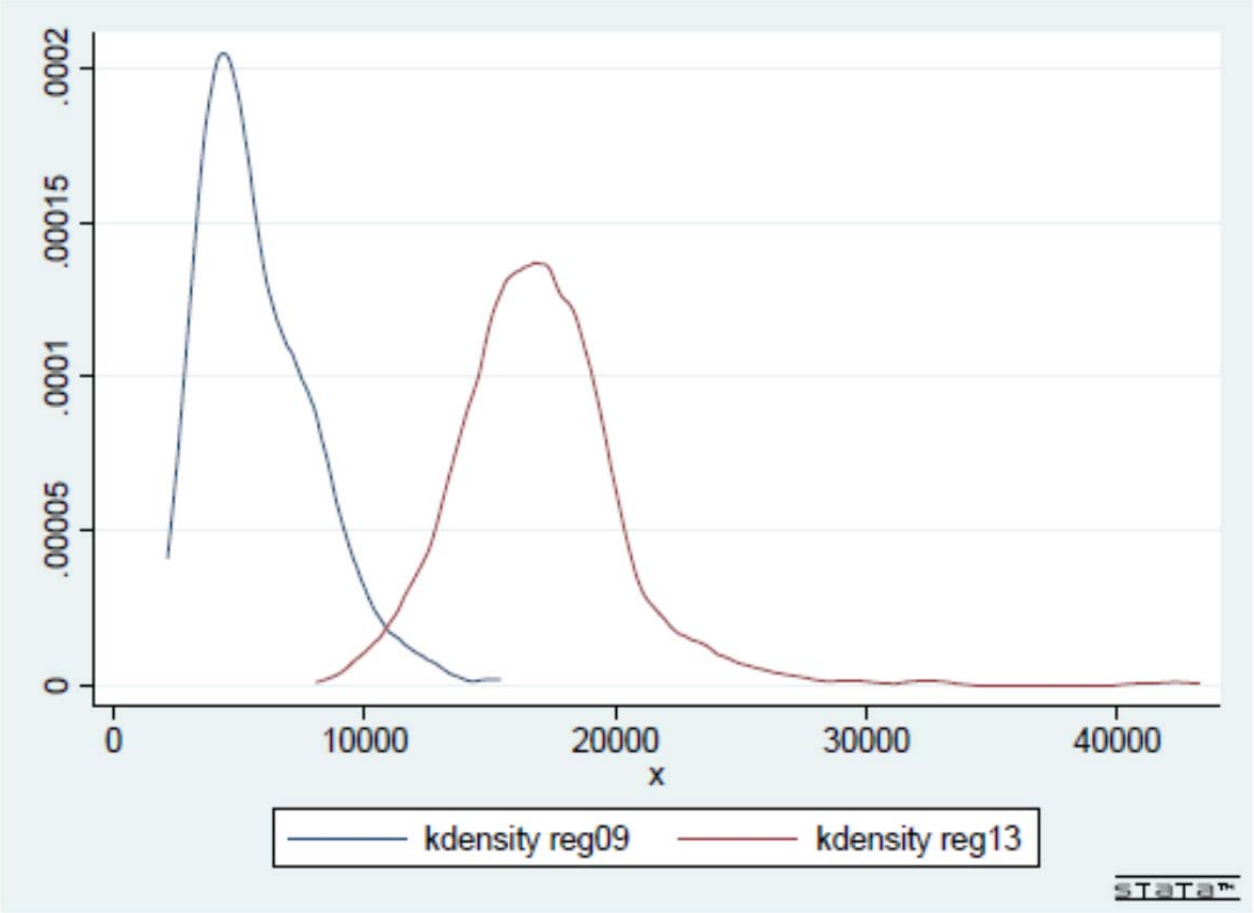


Figure A2. Registered voters by electoral district in 1909 and 1913

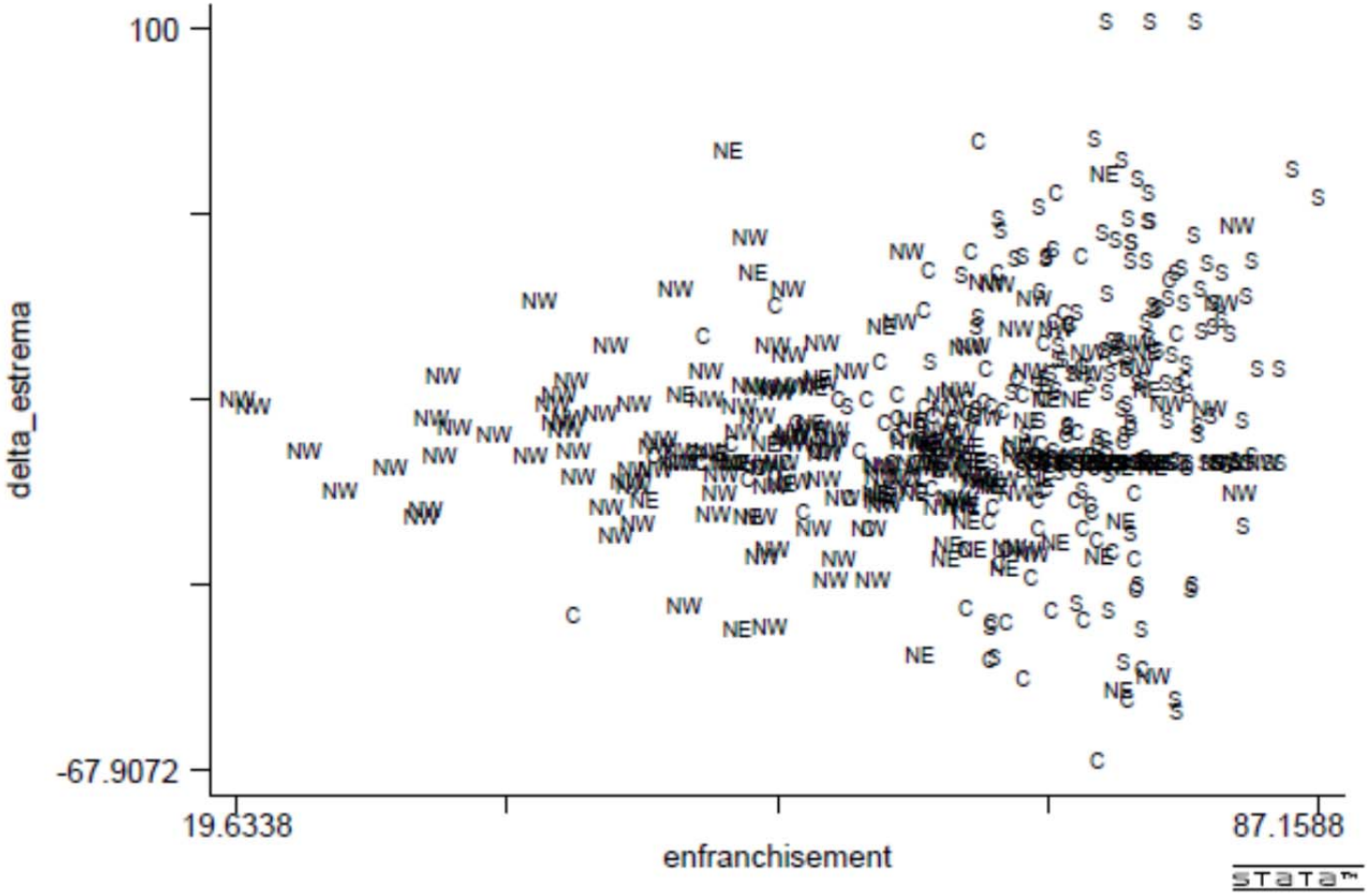


Figure A3. Enfranchisement and change in votes for candidates of the Estrema

(NW stands for North-West, NE for North-East, C for Centre and S for South)

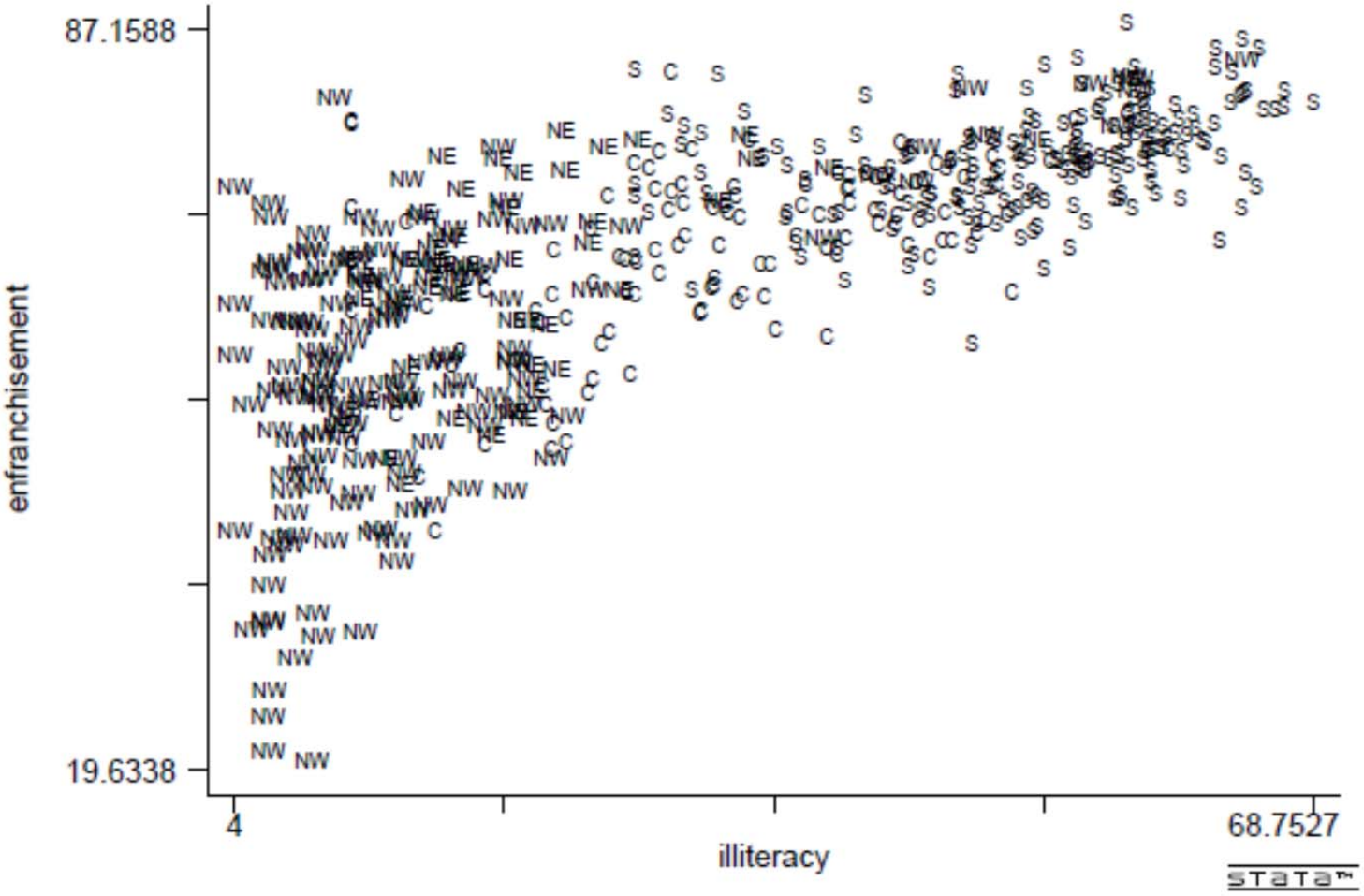


Figure A4. Enfranchisement and illiteracy rates across electoral districts