

Redistribution, Voting and Clientelism: Evidence from the Italian Land Reform *

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Abstract

Many democracies around the world feature pervasive clientelist practices. How these systems emerge and persist is a central question in political economy. Redistribution policies can reduce poverty and inequality, thus undermining important determinants of clientelism. However, by inducing voter reciprocity, they can also initiate the political exchange typical of clientelist systems. Therefore, the relation between redistribution and clientelism is *a priori* ambiguous. We study how voting and clientelism respond to a major redistribution policy, the 1950 Italian land reform. Using a panel spatial regression discontinuity and data for half a century, we show that the large-scale redistribution led to the emergence of a long-lasting clientelist system characterized by political brokers, patronage and targeted benefits. Within this system, the Christian Democratic party, which promoted the reform, experienced persistent electoral gains.

Keywords: redistribution, voting, clientelism, land reform, Italy.

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1 Introduction

Most democracies experience episodes of clientelism. Sometimes, clientelism becomes systemic and dominates politics for long periods of time, as in 1800 New York (Tammany), post-1945 Italy (Christian Democracy) or West Bengal (Left Front). How these systems emerge and persist is a central question in political economy. In this paper, we study how voting and clientelism respond to a major redistribution policy, the 1950 Italian land reform. The effect of redistribution policies on clientelism is *a priori* ambiguous. On the one hand, redistribution policies may reduce poverty and inequality, which are important determinants of clientelism (Robinson and Verdier, 2013). On the other hand, recent research highlights the role of reciprocity in clientelist exchanges (Finan and Schechter, 2012): by inducing voters' gratitude, redistribution policies may initiate the political exchange typical of clientelism.

We examine the effects of redistribution on clientelism by looking at the consequences of a major redistribution policy: the 1950 Italian land reform. Post-World War II Italy is an ideal setting to study this question because the Christian Democratic party (*DC*) was the majority government party in Italy between 1946 and 1992 and throughout this period it engaged in widespread clientelism (Allum, 1974). As in other clientelist systems around the world, DC relied on networks of political brokers to mobilize voters (Lanza, 1991) and exploited public sector employment to maintain consensus through patronage (Chubb, 1982). For all these reasons, DC rule is often taken as archetypical of clientelist systems (see e.g. Hicken, 2011; Piattoni, 2001).

The Italian land reform expropriated large landowners and redistributed their land to poor rural workers. DC government implemented the land reform and only targeted well-defined reform areas identified by agricultural experts. This feature allows us to adopt a panel spatial regression discontinuity design and study differential changes in outcomes between treated and control towns close to the reform areas' borders (Dell, 2010). Land redistribution generated electoral gains for DC, which lasted for four decades after the reform. In the decades following the reform, several clientelist practices became prevalent in treated towns, including political brokers, patronage and targeted benefits. These practices arguably allowed the initial electoral gains to persist. Overall, results from our quasi-experimental design provide evidence that a major redistribution policy contributed to the emergence of a pervasive clientelist system.

We start by showing two preliminary results in support of our research design.¹ First,

¹We focus on target reform areas in the Center-North, as historical and statistical evidence indicates that the required assumption for the panel RDD analysis (i.e. parallel trends at the border) is more likely to hold there. Section 2 elaborates.

voting patterns and clientelist practices were on similar pre-trends in treatment and control towns around the reform border.² Second, contrary to other failed land reforms (e.g. Bazzi et al., 2020), the Italian reform did lead to substantial land redistribution. Ten years after the reform, treated towns experience a 10-15 percentage points increase in the share of small, owner-operated farms, from a control mean of 73%. Thus, the setting allows credible identification of the effects of large-scale redistribution.

The panel spatial RDD shows that DC experiences immediate and long-lasting electoral gains from the reform. In the first elections after the reform, treated towns increase support for DC by two to four percentage points (control mean: 35%). These electoral gains persist over the following four decades (1953-1992). Treated towns appear to support the DC policy agenda, as well as its candidates: in 1974, treated towns were more likely to follow DC directions in a highly divisive referendum on divorce. Overall, these results provide evidence that the 1950 land redistribution transformed reform towns into DC strongholds for four decades.

Next, we study one source of persistence of the electoral effects: clientelism. We hypothesize that the land reform represented the initial ingredient of a long-lasting system of clientelist exchange between DC and voters. Gratitude for the initial land transfer was the first step for this process which subsequently led to a stronger DC clientelistic system in reform areas. We test formally for the emergence of two clientelist practices: political brokers and patronage.

First, treated towns saw an increased presence of DC political brokers (cf. Stokes et al., 2013; Larreguy et al., 2016 and Allum, 1974 for Italy). We focus on *Coldiretti*, an association of small farmers generally considered a key broker for DC in rural areas (Crainz, 1982; Lanza, 1991). Using two newly digitized datasets, we find that in treated towns *Coldiretti* had stronger support among farmers. Thanks to a national healthcare reform passed after the land redistribution, in reform towns *Coldiretti* also managed greater healthcare budgets. The land reform thus not only increased the potential member base of *Coldiretti* (i.e. the number of small landowners), but it also deepened its penetration among small landowners and strengthened its power. A back-of-the-envelope calculation suggests that farmers who received land through the reform were 71% more likely to support *Coldiretti* than pre-existing farm owners.

Second, the panel RDD shows that after the reform, treated towns had a greater share of workers employed in the public sector, a common form of patronage in Italy (cf. Alesina et al.,

²In addition, while not necessary for our panel RDD design, it is reassuring that political, economic and geographic covariates were mostly balanced in levels at baseline.

2001). These effects are particularly large in the 1980s, when DC clientelist system became rampant (Allum, 1997). Thus, as treated towns reliably supported DC in each election, DC continued to dispense benefits to these municipalities, well after land redistribution was concluded. This provides additional support to the existence of long-run political exchange between reform towns and DC governments.³ In sum, our evidence indicates that after redistribution (and not before it) reform towns had greater incidence of distinctive practices commonly associated to the DC clientelist system (Chubb, 1982; Ferrera, 1996).⁴

The land reform thus represents the original ingredient that initiated a system of clientelist exchange between the main ruling party and voters. While the land reform was irreversible, subsequent benefits such as patronage and health care were easier to withdraw and reversible. The combination of the initial large-scale redistribution and of the consequent exchange of reversible benefits in reform towns can explain the sustained electoral support for DC. Voters' gratitude for the initial redistribution makes the subsequent exchanges possible, in line with the insights of Finan and Schechter (2012). In our main contribution, we thus show that a large-scale redistribution policy can foster long-lasting clientelist exchange.

Our paper also speaks to three other strands of literature. Scholars of clientelism have long studied the effect of land ownership on clientelist systems (Baland and Robinson, 2008). This literature has emphasized how landed elites exploit access to land to influence rural workers and to perpetuate clientelist exchanges (see also Anderson et al., 2015 for India). Relatedly, in Mexico, Larreguy et al. (2018) finds that assigning land property rights decreases the vote share of incumbent parties in local elections, arguably because it reduces their control over clients who occupy land illegally. In contrast, in our setting, the land reform strengthened clientelism, arguably because it facilitated new forms of control over beneficiaries. In particular, new landowners started to rely on DC brokers for farm support, a dependence that may have perpetuated their vulnerability (Shefter, 1977; Bobonis et al., 2017).⁵ Thus, our results indicate that when politicians control important inputs of production other than land, transferring land ownership to rural workers may not eliminate clientelism.

Credibly identifying the electoral impact of redistribution policies is challenging and there

³We also find suggestive evidence that treated towns received more pork spending, another ingredient of DC political exchange (cf. Golden and Picci, 2008; Golden and Min, 2013), though these results are not always robust across specifications.

⁴The results are also robust to extensive checks, including an instrumental variable strategy (based on the original plan of reform zones drafted by agrarian experts), analysis of spillovers on voting in control towns, and alternative bandwidths, specifications, sample restrictions, and inference approaches.

⁵Important studies of clientelism include Brusco et al. (2004); Stokes (2005); Kitschelt et al. (2007); Cruz et al. (2017) and Piattoni (2001). See Hicken (2011) and Bardhan and Mookherjee (2016) for reviews. See Galli (1993); Marzano (1996) and Giovagnoli (1996) for studies of DC and Buonanno et al. (2017); Fontana et al. (2018); Carillo (2018) and Durante et al. (2019) for recent political economy studies of Italy.

is little work over their *long-run* electoral effects. Existing empirical studies mostly looks at electoral impact of various fiscal transfer programs over few election cycles (e.g., Bechtel and Hainmueller, 2011, Manacorda et al., 2011, and Zucco Jr, 2013). In the context of land redistribution, few papers have looked at short-term electoral outcomes, exploiting the gradual rollout of the reforms in a difference-in-differences framework (see, e.g., De Janvry et al., 2014, Larreguy et al., 2018).⁶ Since in these cases redistribution eventually expands to initial control areas, it is harder to study the long-term effects of the reforms with these methods. Instead, our design enables us to identify the impact of the reform over several decades.

Finally, we contribute to the literature on the mechanisms of persistence by documenting the role of clientelism in the long-lasting impact of a reform. We also illustrate at which point the electoral persistence disappears (i.e., no differential support for other Christian Democrat and center-right parties after 1992), a rare feature in this literature (Voth, 2021). Additional analysis provides little support to alternative channels of persistence, such as migration, economic development, and voters' beliefs, including intergenerational transmission of beliefs.

2 Background

2.1 The 1950 Land Reform

The Road to the Reform. At the end of World War II, Italian agriculture was backward and poor. In 1948, out of almost 8 million agricultural workers, around 2.5 million were landless rural workers and an additional 1.7 million owned estates too small to support one family (Medici, 1948, cited by Gullo, 1950). This situation created expectations for a reform of land tenure. Proposals ranged from forced leasing of uncultivated plots, to new regulation of sharecropping, to outright land redistribution. During the 1948 political elections it was still unclear which (if any) of these proposals would be enacted. Only at the end of 1949 it became clear that the new DC government would redistribute some land, a move designed to prevent a Communist coalition between rural and urban workers (Segni, 1952).⁷

The Reform Legislation. In the spring of 1950, the DC government presented to the Parliament the *legge di riforma agraria* (P.L.977/1950), an ambitious plan which regulated

⁶An exception is Dell (2012), who shows for the Mexican revolution, that insurgency and subsequent land reform had long-lasting negative effects on development. Several authors look at the political economy determinants of land reforms (e.g., Bardhan and Mookherjee, 2010; Albertus, 2015 and Bhattacharya et al., 2019). González (2013) studies the short-term effect of the 1960s Chilean land reform.

⁷Cf. Acemoglu and Robinson (2000) for a model where *élites* extend the franchise to avoid a revolution.

land ownership in the entire country. The proposed reform partitioned Italy into three zones (A, B, C) and specified different measures for each of these zones. Agricultural productivity, land distribution, and the need for agricultural investments determined the borders of these zones, which the reform specified precisely (tables 2 and 3 in P.L.977/1950: see Appendix Figure A.1-Panel A). These borders were drawn with the assistance of agricultural technical experts, who advised on the type of investments necessary in different parts of the country. Detailed reports used to prepare this map confirm that the areas were identified based on agronomic considerations (Negri, 1949; Canevari, 1949; Servizio Statistica UNSE, 1949).

The initial *legge di riforma agraria* was never approved. Instead, the DC government presented to the Parliament a second proposal, the *legge stralcio*, eventually enacted in October 1950.⁸ The *legge stralcio* was less ambitious than the original proposal and it targeted only the highest-priority regions: roughly “Zone B” of the *legge di riforma agraria* (Salomone, 1950). The final version of the *legge stralcio*, however, was also less precise than the original maps of the *legge di riforma agraria*, and effectively delegated the exact definition of the reform borders to the executive, making sure that the law would not tie Government’s hands too tightly (Gasparotto et al., 1950; Germani, 1950). The final areas of reform were made public in February 1951 with a series of executive orders. These executive orders redistributed land in nine large reform areas (Figure 1-Panel A), comprising around 29% of the country.⁹

Some of the reform borders differed significantly from the original *legge di riforma agraria* (Ruini, 1951; Ministry of Agriculture’s Spokesman, 1951)¹⁰. Changes were concentrated in the South: out of 1.6 million hectares of the original reform proposal, only 60% were eventually included in the reform. Towns with landlords close to DC successfully lobbied to exclude their town from the reform (Calasso, 1952). However, there were also additions: in Campania, an additional group of towns not contiguous to the rest of the reform area entered the final plan: they accounted for 9% of the reform in the South. In various southern regions, towns that had experienced land occupations before the reform were more likely

⁸The *legge stralcio* was enacted as law 249 of 28th October 1950. A third bill regulated land redistribution on a small area covering the Sila plateau in the Southern region of Calabria. The *legge Sila* was enacted on 12th May 1950, and it was later superseded by the *legge stralcio*.

⁹The areas were: Delta Padano (North East), Maremma (Center-West), Fùcino (Center), two separate areas in Campania (Center-South, both managed by the *Opera Nazionale dei Combattenti*), a broad area that straddled across Molise, Puglia and Lucania (South-East), Sila (South-West) and the whole territory of Sicily. The entire island of Sardinia was also affected by the land reform, but two separate agencies managed expropriations around Cagliari (in the *Comprensorio di Flumendosa*) and in the rest of the island. Our empirical strategy does not allow us to study the electoral effect of the reform in Sicily and Sardinia, because these two islands were entirely included in the reform.

¹⁰The Government did not record the debate that led to the inclusion of towns in the reform areas. The omission seems deliberate since the minutes of the Government meeting that defined the reform areas contain detailed information of the debates held before and after the discussion of the land reform (Andreotti, 1951).

to be included in the reform (Rivera, 1950; Percoco, 2019). The political motives of land redistribution in the South transpire in the letters sent to the then Ministry of Agriculture (e.g. Caglioti, 1950).

By contrast, in Delta Padano and Maremma the final borders of the reform followed closely the definition spelled out by the agronomy experts in the original plan: out of the proposed 1.4 million hectares, only 8% were eventually excluded from the reform. Additions accounted for 7% of the final area: they concentrated at the edges of Delta Padano, and made sure that land reclamation projects in the area would be possible. This suggests that political manipulations were less important in the North. Private, confidential records of the then Ministry of Agriculture, reveal frustration of DC politicians for the *absence* of political manipulation in the North.¹¹

Taken together, these archival sources provide suggestive evidence that DC manipulated reform borders in the South. In the North, however, the same records suggest that manipulation may not be a concern for our empirical strategy. While it may be surprising that DC adopted different approaches for the North and the South, scholars have noted that large Southern landowners were influential inside DC (Lanza, 1991; Piazza, 1974). While drafting the reform, it may have been hard for the Ministry of Agriculture to resist pressures when they came from this powerful lobby inside his party.

The formal tests in Section 4 support this preliminary conclusion. In addition, to mitigate concerns of manipulation of the reform borders due to land occupations or other reasons, Section 5.4 also presents an IV strategy where we instrument the actual reform with the original reform in the *legge di riforma agraria*.

The Land Reform Implementation. Nine separate public agencies (*Enti di Riforma*: “Reform Boards”) implemented the reform, each in charge of land redistribution in one of the nine areas of the reform. The *legge stralcio* imposed expropriations only of large and inefficient farms, as specified precisely in a table contained in the bill (Appendix Figure A.3). Landowners received compensation for the property lost in the form of 25-year fixed-rate

¹¹These documents were recently made publicly accessible (Mura, 2017). Appendix Figure A.2 shows three such documents, in which DC politicians complained with the Ministry for the exclusion or inclusion (Panels A, B and C) of reform towns in the North. Appendix Figure A.2 Panel D and E show the replies of the Ministry, who argued that lack of funds prevented him to include more towns in Maremma, and he had to include towns at the edges of Delta Padano to make land reclamation projects possible. The private and confidential nature of these exchanges suggests that these were honest answers. Possibly as a result of this limited interest in the North, opponents of the reform complained about the inclusion of towns where farms were efficient and land distributed evenly (Giornale dell’Emilia, 1951) as well as the exclusion of areas where redistribution was necessary (De Caro, 1951; Toldo, 1957).

government bonds yielding 5% a year.¹² After enacting the law, the government implemented the reform quickly: it expropriated all land before April 1953 and redistributed it shortly thereafter (cf. Segni, 1952; Russo, 1955).

The reform expropriated around 18% of agricultural land in reform areas in the North and around 13% in the South (Marciani, 1966). Rural workers who wanted a plot of land had to apply through one of the Reform Boards, and they purchased the estate with the help of thirty-year public loans at generous rates (3.5%). They could not re-sell the plot before repaying the debt, and could not clear the debt in advance. For their part, expropriated landlords were not allowed to purchase land for 6 years. Almost everywhere, eligible applicants vastly exceeded available land. Excess demand varied across the country: in the North, the beneficiary to request ratio was 60-70% (Bal docchi, 1978); in the South significantly lower at about 25% (Prinzi, 1956; Capobianco, 1992). In the average reform town, 7 owners lost their land to about 200 beneficiaries. Beneficiaries were 47% farmworkers, 37% tenants and 9% small landowners (Marciani, 1966). The vast majority of beneficiaries were resident of the town where the land was located (Dickinson, 1954, Rossi-Doria, 1958, Marciani, 1966).

DC maintained firm control of the whole land redistribution process. Responsibility for land allocation fell on the Reform Boards, whose directors were appointed by the Ministry of Agriculture. During the crucial years of land redistribution, every president of these agencies was a prominent DC personality (e.g. Rossi in Delta and Medici in Maremma). These agencies were relatively free to select beneficiaries: although the law specified eligibility criteria to receive the land, applications exceeded available land and agencies had to decide among many qualified applicants (Bal docchi, 1978; Prinzi, 1956; Capobianco, 1992; Marciani, 1966). Politics influenced decisions: DC officials trying to maximize the impact of the reform would not assign land to Communist voters unlikely to be persuaded. Consistent with this, inspection of original applications reveals that applicants known to be radical Communists were singled out and denied land (Appendix Figure A.4). Journalists at the time observed how the boards favored applicants who moved closer to DC (Russo, 1955). By selecting beneficiaries most likely to reciprocate, strategic land allocation may have created the pre-conditions to start the clientelist exchange. For instance, one of the land beneficiaries leaders said during an assembly in 1955: “My fellow farmers, [...] at the polling stations we have to show our reciprocity, our gratitude and our loyalty to [DC]” (Ufficio DC, 1956).

As is often the case in land reforms around the world (see, e.g., King, 2019; Wolfe, 2017), also in Italy land redistribution came with land improvement projects. These projects included land reclamation, irrigation, roads and housing for the new landowners. Reform

¹²Landowners opposed form and amount of compensation (Capua, 1950; Pecoraro, 1950).

Boards managed these projects. The intervention represented a redistribution in favor of poor farmers and our evaluation studies the electoral effects of the entire bundle. We do not attempt to disentangle the specific impact of land improvement projects.

2.2 Fifty years of DC rule

DC Governments. Following World War II, DC ruled Italy uninterruptedly between 1946 and 1994. While several reasons explain the electoral success of DC, many scholars have noted the importance of clientelism (e.g. Chubb, 1982; Allum, 1997; Golden and Chang, 2001; Golden, 2003). We investigate several aspects of DC clientelist system.

Political Brokers. Grassroot associations acting as political brokers were crucial for DC. Most relevant to our context were the associations of small landowners. While several such associations existed, the largest one, *Coldiretti*, was tightly connected to DC: it routinely campaigned for the party (Appendix Figure A.5) and its leaders served as MPs with DC. *Coldiretti* coordinated a network of farmer cooperatives (Primavera, 2018) and offered farmers many services, including input purchases, output sale, credit, fiscal assistance, and healthcare.

The land reform increased not only *Coldiretti*'s potential member base (i.e. the number of small landowners) but also its actual penetration among small landowners. Back-of-the-envelope calculations discussed in Section 5.3 indicate that farmers who received land through the reform were 71 percent more likely to support *Coldiretti* than pre-existing small landowners in the same areas. The role of DC and *Coldiretti* in managing the reform, specifically in the land allocation process, is likely to have contributed to this pattern. For instance, *Coldiretti* leaders had often manager roles inside the Reform Boards and encouraged beneficiaries to join the organization (Fano, 1981; Lanza, 1991).

One of the most important tools used by *Coldiretti* to influence farmers were the *Casse Mutue*, a system of more than eight thousand local healthcare centers serving farmers. *Casse Mutue* were introduced by a DC healthcare reform four years after the land reform.¹³ These local health centers collected health insurance contributions and provided state-subsidized health services to farmers. Nominally public, *Casse Mutue* were dominated from the beginning by *Coldiretti*, whose leaders sat on more than 90 percent of *Casse Mutue* board seats and effectively controlled over 95 percent of them. Thus, to access healthcare the farmers had to interact with local *Coldiretti* leaders, who used these targeted benefits to mobilize voters during electoral campaigns (Luzzi, 2004; Lanza, 1991). In Section 5.3 we examine

¹³Farmers without access to *Cassa Mutue* had to pay doctors out of pocket or, when too poor, had basic medical expenses covered by the municipality.

Casse Mutue's elections and budgets to gauge the local strength of *Coldiretti*.

Patronage. Over the five decades of DC rule, patronage became widespread (Graziano, 1973; Ferrera, 1996; Shefter, 1977). Robinson and Verdier (2013) cite Chubb (1982) saying that in Italy “when all is said and done, a job signifies a vote and vice versa.” This type of clientelist exchange became arguably more common during the 1980s (Allum, 1997). In Section 5.3 we use public sector employment as an indicator for local incidence of patronage.

Distributive Politics. Anecdotal evidence indicates that DC politicians engaged also in distributive politics (pork barrel: Marzotto and Schachter, 1983; Bracalini, 2016), an intuition confirmed by systematic analysis (Alesina et al., 1995; Golden and Picci, 2008). While clientelism does not always lead to distributive politics, DC politicians used pork to reward aligned constituency. For this reason, in Section 5.3 we investigate this practice by studying discretionary transfers from the central government to municipalities.

The End of DC Rule. In 1993 a major corruption scandal (*Mani Pulite*) led to the resignation of the DC government and the break-up of the party. Following the dissolution of the party, former-DC politicians created several new Christian Democratic parties. None of these parties gained a majority and none was able to form a government after 1994. At the election of 1994, Berlusconi's newly formed *Forza Italia* ran on an economically conservative platform and became the largest right-wing party in Italy. We explore the consequences of these events in reform towns in Section 5.2.

3 Data Sources

We combine several town-level datasets, including many newly digitized data sources: see Appendix B for data construction and variables description.

Reform Towns. We start from a map of Italian towns in 1951 on which we classify every town included in the 1951 land reform. We find the lists of proposed reform towns in the *legge di riforma agraria* and the actual reform towns in the land reform executive orders. We create the reform borders by conflating all contiguous towns inside the reform area. We then use these borders to calculate the distance from the centroid of every town to the closest reform border.

Electoral Outcomes. We source electoral outcomes from Istituto Cattaneo, which publishes town-level results of every election of the lower chamber of the Italian Parliament between 1861 and 2008, along with the results of the 1946 election to the Constitutional Assembly (Corbetta and Piretti, 2009). Our main results focus on elections between 1946

and 1992, but we also use data from earlier (1919-24) and later (1994-2001) rounds. We define time-consistent geographic units based on the list of towns in 1951.

We integrate these data with two newly digitized databases. First, we collect town-level results of the 1974 “referendum on divorce” (Ministry of Interior, 1977). Second, we construct a new database of mayors’ affiliation at the time of the reform with information from the archives of newspapers close to DC (*L’Avvenire*) and PCI (*L’Unità*).

Agricultural Variables. Data on land distribution before the reform come from Medici (1948), who collected farm-level information on the value and size of land in 1948. The publication was commissioned by the Italian Parliament and served as the basis of the discussion of the land reform. From the original publication, we digitized Table II, which reports for each town the number and the value of estates broken down by 11 separate categories of taxable income. We follow the land reform rules and consider estates that could be expropriated as those in one of the top 4 categories. All estates in these categories were worth at least £20’000, and the reform bill prescribed expropriation for estates worth £30’000 or more (Appendix Figure A.3). The 1961 agricultural census includes tables with the number of farms by type of management (e.g. owner-operated, tenant farming, share-cropping). We digitize these tables from the original volumes of the census. We find the same variable for a subset of our towns in the 1929 land registry, whom we digitize (ISTAT, 1936). Finally, we reconstruct land invasions after the reform with newly digitized documents from the archive of the Ministry of the Interior. We read all local police reports on land invasions in 1951-52 and geolocate these events (Ministry of Interior, 1952).

Coldiretti and Casse Mutue. We digitize new data on board elections and budgets of *Casse Mutue*: farmers’ local healthcare providers. Elections of *Casse Mutue*’s board of directors were generally held every three years and always saw *Coldiretti* candidates running. We digitize all available elections results (1955-1970) from the fonds of the All. Naz. dei Contadini (1970). We digitize the 1965 budgets of all *Casse Mutue* from Fed. Naz. Casse Mutue (1966). We measure the relative size of *Casse Mutue* with total revenues and total expenditure per capita (from 1961 population census).

Town Budgets. We measure central government transfers to Italian towns with records from municipal budgets. At the end of every fiscal year, Italian towns must provide the Ministry of Interior with detailed budget records. We found publications summarizing central government transfers at the town level for the years 1952, 1955 and 1959 (ISTAT, 1962b), which we digitize.¹⁴ We normalize transfers by 1951 census population.

¹⁴Budget information for other years is either lost or recorded only by province × town size category.

Contemporary Policies. We measure the intensity of several public policies enacted at the time of the land reform. We infer the intensity of the 1947 malaria eradication program with a map classifying Italian towns where malaria was endemic before the program (Missiroli, 1934); data on the Marshall Plan is from Bianchi and Giorcelli (2018), which we thank; we digitize new data for the 1949 *Piano Casa* from publications of the Ministry of Labor; we create a dummy for inclusion in the 1950 *Cassa del Mezzogiorno* using the original law. Information on the importance of sectors affected by the GATT and ECSC agreement is from the newly digitized 1951 population and economic censuses.

Economic, Demographic and Geographic Controls. Most town-level economic and demographic variables between 1936 and 1991 are from decadal population and economic censuses, including the newly digitized population census of 1936 and population and economic censuses of 1951 and 1961. Income information was not collected in the early decadal censuses. We digitize town-level income in 1981 from Marbach and Ciapparelli (1983) which is the earliest collection of town-level income. We calculate the distance to the coast and to Rome based on the 1951 map of towns. Potential yield of wheat and maize is from FAO (2015) and elevation and slope from USGS (2005).

4 Empirical Strategy

This section describes our empirical strategy. First, we illustrate our approach, which combines spatial RDD and diff-in-diff. Second, we test our identification assumptions.

4.1 Panel Spatial Regression Discontinuity

Evaluating the impact of redistribution policies, including their electoral impact, typically faces major identification challenges. The design of the Italian land redistribution offers an opportunity to overcome challenges. Because the reform targeted towns in well-defined reform areas, we can estimate the impact of the policy by comparing changes in outcomes in treatment and control towns located in proximity of the reform borders. Formally, we combine a spatial RDD with difference-in-differences, exploiting the longitudinal nature of our data (see Grembi et al., 2016 for a similar approach applied to the study of fiscal rules and Dell et al., 2018 for a recent study featuring spatial RDD).

Our empirical strategy is based on the following panel RDD equation:

$$y_{irt} = \sum_t \beta_t T_i + \sum_t f_t(\text{geographic location}_i) + \eta_i + \eta_{rt} + u_{irt}, \quad (1)$$

where y_{irt} is an outcome in town i , reform area r , election year t .

Our parameters of interest are the β_t year-specific treatment coefficients. The model also includes town fixed effects, η_i , and reform area \times year fixed effects: η_{rt} . Throughout the paper, we report two main specifications for the function f_t : a linear function in distance from the reform area border, which we allow to vary on the two sides of the border, and a quadratic polynomial in latitude and longitude, estimated separately for the two reform areas included in the sample (Maremma and Delta Padano). In both cases, we estimate these functions for each election year.¹⁵ The unidimensional model identifies the discontinuity in the most parsimonious way, while the multidimensional model allows geography to have non-linear effects while avoiding overfitting (Gelman and Imbens, 2019). In Section 5.4 we also report results from specifications between these two extremes. We cluster standard errors by town and show robustness to spatial autocorrelation.

In our preferred specification, we restrict the sample to towns that are located within 25 km from the reform border.¹⁶ In addition, we report results for alternative bandwidths between 10 km and 50 km. Figure 1-Panel B shows 25 km buffers inside (dark red) and outside (orange) the reform areas we consider in the analysis.¹⁷ These buffers include 490 towns.

Our empirical strategy identifies the causal effect of the land reform under the assumption of parallel trends *at the reform border*. In Section 4.2, we provide evidence in support of these assumptions by looking at pre-trends and examining contemporary policies. In addition, the Stable Unit Treatment Value Assumption (SUTVA) must hold: redistribution in treated towns should not affect control towns. This assumption would be violated if towns excluded from the reform voted against DC to punish the government. We explore the validity of this assumption in Section 4.2, too.

Our strategy estimates the (local) treatment effect of the inclusion of a town in the reform area. This is an “intention to treat” estimate. We do not use variation in the intensity of actual redistribution because actual town-level redistribution is likely to depend on endogenous variables (e.g. the number of expropriable farms in each town may be correlated with

¹⁵Formally: In the unidimensional case: $f_t = \alpha_t^0 \cdot d_i + \alpha_t^1 \cdot d_i \times T_i$; In the multidimensional case $f_t = \sum_r [\alpha_{rt}^0 \cdot \text{lat}_i + \alpha_{rt}^1 \cdot \text{lat}_i^2 + \alpha_{rt}^2 \cdot \text{lat}_i \times \text{lon}_i + \alpha_{rt}^3 \cdot \text{lon}_i + \alpha_{rt}^4 \cdot \text{lon}_i^2]$, where d is distance to the border, lat latitude and lon longitude.

¹⁶We are not aware of established methods to define the optimal bandwidth in multidimensional RDD or in panel RDD. When considering unidimensional RDD on the average DC vote share in post-reform elections, the optimal bandwidth is approximately 17 km with Calonico et al. (2014) method and approximately 30 km with Imbens and Kalyanaraman (2012) or Ludwig and Miller (2007) designs.

¹⁷Section 2 suggests that our empirical strategy is not suitable in the South. Appendix D.1, shows that key identification assumptions fail in the South. In particular, pre-trends in vote shares at the border are not parallel.

many other town-level characteristics).

4.2 Testing the Identification Assumptions

In this section, we test the identification assumptions of our empirical strategy, i.e. parallel trends at the border, absence of correlated shocks and SUTVA.¹⁸

4.2.1 Parallel Pre-Trends at the Border

The key identification assumption is the presence of parallel trends at the border. We provide support for this assumption by studying pre-trends of voting and census variables and by examining contemporary policies.

We begin by discussing pre-trends. We estimate the following RDD model:

$$y_{ir} = \alpha + \beta_i T_i + f(\text{geographic location}_i) + \eta_r + \epsilon_{ir}, \quad (2)$$

where the outcome variable is the change in the baseline covariate of interest (1946-1948 for election outcomes and 1936-1951 for census variables). Figure 2 presents the results with our two specifications (distance and latitude-longitude) for every bandwidth between 10 and 50 km.

Christian Democrat Vote Share: 1946-1948. We first present estimates of Equation (2) when the outcome variable is the change in vote shares of DC. In both specifications, we find evidence of parallel pre-trends between treatment and control towns at the border: the coefficient on the DC pre-trend is never significant and, with the exception of very small bandwidths, it is always lower than 0.02, from a control mean of 0.12. Figure 4-Panel B presents a bin scatter and shows graphically the continuity at the border.¹⁹

One concern is that only two elections took place before the land reform and after World War II. Appendix Figure C.3 reports treatment coefficients from a panel RDD regression that includes the 1919, 1921, and 1924 elections (the last ones before the Fascist dictatorship), as well as the 1946 and the 1948 ones. We report results for the two specifications at 25 km in Panels A and B. Panels C and D estimate a single coefficient for all pre-1946 elections for every bandwidth between 10 and 50 km. Across specifications and bandwidths, parallel trends hold

¹⁸To our knowledge, there is no standard test to check the smoothness of the density when the discontinuity is multidimensional. Appendix C.2 discusses the result of a McCrary test run on the distance to the border.

¹⁹Other panels in the Figure 2 show parallel pre-trends for all other major parties, including the Communist party. Appendix D.4 discusses these results in detail.

for the Italian Popular Party (PPI), the Catholic Party to which most DC founders belonged before the war (Appendix Figure C.3-Panel A).

Census Variables. We digitized town-level data from the 1936 and 1951 population censuses. Overall, we observe parallel pre-trends both in population and employment variables. Point estimates for the changes in the share of active population and for employment shares in agriculture and public sector are never significant at conventional levels. Log population and manufacturing share exhibit a handful of significant coefficients in the graphs, but across bandwidths and specifications most of the coefficients are small and insignificant.

The pre-trend analysis of these census variables spans a longer period than the electoral ones (1936-51 vs. 1946-48) and it ends right at the time of the reform (1951). Together with the results of electoral pre-trends, this analysis indicates that no relevant change occurred differentially across the border neither in the decades leading to the reform nor in the few years immediately preceding it.

Balance. Since town fixed effects capture time-invariant differences, our panel RDD design strategy does not require balance in levels. Nevertheless, in Appendix Figure C.1, we also test for balance on the two sides of the border at the time of the reform for the following variables: *i*) land distribution, including the share of expropriable estates and the share of farms and land managed by the owner (i.e., a proxy for small-scale cultivation);²⁰ *ii*) vote shares for all the major parties in 1948, the last parliamentary election before the reform, and for the 1946 mayor elections; *iii*) 1951 census variables, including population, labor force participation, sectoral employment share, and plants per capita, as well as 1952 fiscal transfers; *iv*) geographical variables, including town area, elevation, crop suitability and distance from coast and Rome.

Overall, at the border, treatment and control towns are quite similar in terms of baseline land distribution, vote shares and economic variables. Balance in these key outcomes provides additional confidence in our research design.²¹ We do observe unbalance in some

²⁰As explained in Section 3, Medici (1948) reports the number and the value of estates broken down by 11 separate categories of estate value. We consider expropriable estates as those with value in one of the top 4 categories of value. All estates in these categories were worth at least £20'000. The reform bill prescribed expropriation for estates worth £30'000 or more (Appendix Figure A.3).

²¹To the best of our knowledge, town-level data on *Coldiretti* membership do not exist (Lanza, 1991 p.58, footnote 36). In Section 5.3 we measure the presence of *Coldiretti* at town level indirectly, using the results of the elections of *Casse Mutue*. Three observations suggest balance of *Coldiretti* before the reform: first, small landowners are balanced in 1929, which implies that reform towns did not have more potential *Coldiretti* members before 1951; second, the balance in DC vote shares in 1946-48 speaks against greater activity of the association in treated towns before 1951 (since *Coldiretti* was very effective at promoting DC candidates); third, the strongest tool that *Coldiretti* had to influence its members were the *Casse Mutue* which were established only after the reform, in 1954.

geographical variables (e.g., town area, distance from Rome, and maize suitability). However, only the difference in town area is robust across specifications (distance and coordinates) and bandwidths, while the other differences are not. Importantly, town fixed effects in our panel RDD absorb time-invariant differences.

4.2.2 Contemporary Policies

Could other contemporary policies confound the effect of the reform? In the years following WWII, Italian governments implemented a number of policies to promote economic development. Examples include the malaria eradication program (1947-51; see Buonanno et al., 2019), the Marshall plan (1948-51; see Bianchi and Giorcelli, 2018; Giorcelli, 2019), the *Piano Casa* (1949-63), and the *Cassa del Mezzogiorno* (1950-92). In those years, Italian governments also signed the General Agreement on Tariffs and Trade (1950) and joined the European Coal and Steel Community (1951).

These contemporary policies do not represent a threat for our empirical design. First, many of these policies targeted entire provinces or large urban centers: for instance, the *Cassa del Mezzogiorno* mostly followed province borders, and the *Piano Casa* built homes in major cities. The land reform instead targeted a subset of towns in some province and our results are robust to the inclusion of province fixed effects. Second, Appendix Figure C.4 shows that exposure to these policies did not change discontinuously at the borders of the land reform.²²

4.2.3 Stable Unit Treatment Value Assumption

The reform could change voting in control towns relative to a counterfactual where no reform takes place. This may happen if towns that are (barely) left out of the reform areas resent exclusion and punish DC. If this were the case, SUTVA would not hold.²³

We propose four approaches to mitigate this concern. First, we collect new data on land invasions in the years following the land reform. We ask whether control towns experienced more invasions following land redistribution, an extreme form of resentment. Second, we consider heterogeneity across control towns in potential benefits from the reform: we test whether control towns with a higher share of agricultural workers experience a reduction

²²In addition, Maremma did not overlap with *Cassa del Mezzogiorno* target areas. In the 25 km bandwidth, only 8 control towns (2% of the control group) were included in the *Cassa del Mezzogiorno*.

²³The reform would also affect outcomes in control towns mechanically if some of the beneficiaries of the land reform came from control areas. This type of spillover is not a concern because *in practice* almost all beneficiaries were resident of treated towns (cf. Section 2). Section 6.1 discusses migration in depth.

in DC support after the reform. Third, we consider heterogeneity across control towns in visibility of the reform: we study whether control towns with a higher share of perimeter bordering reform areas experience a reduction in DC support after the reform. Fourth, in the spirit of a “Donut RDD” (Barreca et al., 2016), we estimate Equation (1), but drop towns close to the reform border. Similar to the previous exercise, if voters in control towns resented the reform and punished DC after 1950, we expect this effect to be larger close to the border, where voters were likely to be more aware of the reform. Appendix C.10 presents the analysis in detail. None of these tests finds evidence of resentment

4.2.4 Discussion

The evidence of this section suggests that the identification assumptions required for the panel Regression Discontinuity hold. Pre-trends are parallel at the border for voting, patronage and other economic variables. In addition, the balance at the border in land distribution, elections, geography, economy, agriculture as well as patronage and targeted benefits suggests that towns located just inside the reform areas were mostly similar to those just outside. Finally, we find no evidence that contemporary policies affected differentially treated and control towns at the border.

5 The Effects of the Reform on Voting and Clientelism

This section presents the main results of the paper. As a preliminary step, we show that the reform altered land distribution. We then show the electoral effect of the reform, which persisted for four decades. Next, we document the emergence of clientelism in reform towns: we focus on two reversible practices that are common in clientelist systems: targeted benefits controlled by political brokers and patronage via public sector employment. Finally, we discuss several robustness checks.

5.1 The Effect of the Reform on Farm Ownership and Tenancy

As a preliminary result, we show that the land reform changed land distribution in treated areas. The 1961 agricultural census reports how many farms and how many hectares are managed directly by the owner of the farm (as opposed to tenants). This is a proxy for smallholder agriculture (the 1961 agricultural census does not report town-level farm size).

Figure 3 shows the effect of the reform on the share of farms (Panels A-B) and on the share of land (Panels C-D) managed by the owner, using the two specifications of the RDD

model in Equation 2. We report coefficients for bandwidths from 10 to 50 km. Treated towns have on average between 10 and 15 percentage points (p.p.) more owner-operated farms, from a control average of about 70 percent. The effect is significant and stable across bandwidths in the distance specification and grows with the bandwidth in the coordinate specification. Similarly, in treated towns, the share of land in owner-operated farms is 10-15 p.p. higher than in control towns, from a control average of 41-47 percent. These results are not affected by controlling for the same variable in 1929 (Appendix Figure D.4) which was balanced between treatment and control (see Appendix Figure C.1). Taken together, these results indicate that the reform significantly increased the presence of small-holding agriculture in treated areas.

5.2 The Electoral Effect of the Reform

We now discuss results on the electoral effect of the land reform.

Preliminary Graphical Evidence. We start with graphical evidence of the discontinuity at the border. Figure 4-Panels C shows the change in the average DC vote share from pre-reform elections (1946-48) to post-reform elections (1953-92) as a function of the distance to the reform border, visualizing the specification of Equation 1 that controls for distance. The graph highlights that in 1953-1992 elections, relatively to control towns, DC experienced an increase of 4 p.p.²⁴ Appendix Figure C.6 shows these effects on a map of Italy, one way of visualizing the effect after controlling for coordinates. The map confirms that border towns inside of the reform area swung in favor of DC.

Panel RD Estimates. Next, we estimate Equation (1) for elections 1946-92. Figure 5-Panels A-B report point estimates and 95% confidence intervals for β_t : the effect of the land reform on DC vote shares in every election. Panel A presents estimates controlling for distance to the border and Panel B for the polynomial in coordinates. The sample consists of 490 towns within 25 km from the reform border in the North. The baseline year is 1948, the last election before the reform. The treatment coefficients in the 1946 election indicate that treated and control towns at the border were on parallel pre-trends. The treatment coefficient in the 1953 election suggests that in treated towns DC vote share increased by between 2 and 4 p.p. during the first election after the land reform, from a control mean of 36.4%: an increase of between 4 and 11 percent. The effect is larger in the unidimensional specification with distance, but precisely estimated in both specifications.

²⁴As the graph shows, DC's vote share falls in 1953-92 compared to 1946-48.

While it is striking to observe a large effect already in 1953, the timing is plausible. Expropriations in Maremma and Delta Padano were complete by the 27th January 1953, and Reform Boards assigned land immediately after (Russo, 1955). Thus, when on the 7th of June 1953 Italians voted, the process of redistribution was well-advanced and likely to be in the minds of many voters. Indeed, our estimates of the impact of the reform on DC votes in the 1950s are in line with anecdotes and descriptive statistical evidence produced at the time. Amintore Fanfani, a DC leader, noted in 1956 that “in the reform areas, the *Scudo Crociato* [the DC symbol: a crusader shield] shines while the hammer and sickle rust” (Ufficio DC, 1956). A 1957 academic study records the gains that DC made in the elections following the reform (Toldo, 1957). In one dramatic episode, 220 PCI members who received land, publicly tore down their membership cards and joined DC (Il Mattino, 1951). More broadly, the impact in the first election is consistent with the literature of the short-term electoral impact of public policies (Manacorda et al., 2011; Zucco Jr, 2013).

DC gains remain large and stable until 1992 (Figure 5-Panels A-B; in 1953-92, the average DC vote share in control towns ranges between 29% and 36%). When we pool together all post-reform elections until 1992 in Figure 5-Panels C-D, we find that the effect is stable across bandwidths for both specifications. During the four decades following land redistribution, the land reform quickly lost prominence in political debates and Italian society changed profoundly. Nevertheless, DC maintained firm control of the Italian government throughout the period. The long-term persistence of the voting effects of the reform contrasts with the short-term electoral impact of other redistribution policies (Bechtel and Hainmueller, 2011).

DC support in treated towns came mainly at the expense of the PCI and had minor impact on other parties and turnout.²⁵ This may not be surprising as the land reform was designed to reduce the influence of PCI in the countryside (Section 2). Appendix D.4 discusses these effects in detail.

Persuasion Rate. To assess the magnitude of the electoral effects, we follow DellaVigna and Gentzkow (2010) and compute the *persuasion rate* of the reform. The persuasion rate is the percentage of beneficiaries who start voting DC among those who were not already DC supporters.²⁶ With the distance (coordinates) specification we find a persuasion rate of 0.64

²⁵1992 elections are an exception. In 1992, turnout fell substantially, possibly in response to early scandals about major parties. The decrease was stronger in treated towns (marginally significant).

²⁶For the persuasion rate p we adapt equation (1) of DellaVigna and Gentzkow (2010) to:

$$p = \frac{dc^T - dc^C}{b^T - b^C} \cdot \frac{1}{1 - dc^0}.$$

In this equation, dc^T and dc^C are the DC vote share in treated and control towns, b^T and b^C are the

(0.48): out of three people who received land and were not already DC voters, two started voting for DC as a result of the reform (one out of two). The effect is large but plausible, given the magnitude of the asset transfer. We note that positive spillover on non-beneficiaries (e.g. on beneficiaries' relatives) would imply a lower persuasion rate. In contrast, if envy and resentment among non-beneficiaries *living in treated towns* lead them to stop voting for DC, this would imply a higher persuasion rate.

Support for DC Agenda: the 1974 Divorce Referendum. We next study whether voters in treated towns also support DC *policy agenda* with a 1974 referendum. In 1970, Law n.898 (so-called *Legge Fortuna-Baslini*) introduced the divorce in Italy. Shortly thereafter, Catholic groups promoted a referendum to repeal the law. During a highly divisive campaign, DC politicians passionately sided with the repeal, but ultimately lost 3-to-2.

Using the panel RDD approach of Equation (1), we test whether support for the repeal of divorce in the 1974 referendum was higher in treated towns. For comparability, we report the effect of the reform on the 1974 referendum along with its effect on Parliament elections by decade. Appendix Figure D.13-Panels A-B shows that repeal received between 2.3 and 2.6 p.p. more preferences in treated towns, from an average of 36% in control towns. The effect is quantitatively sizable; it is marginally significant in the distance specification (p-value=0.11) and significant in the coordinate specification (p-value=0.04). Appendix Figure D.13-Panels C-D shows that these effects are relatively stable across bandwidths and specifications. Overall, these results suggest that treated towns did not vote with DC only out of economic interest and instead aligned themselves more broadly with DC agenda.²⁷ Section 6.3 elaborates on this point.

The effect after 1992. In 1993, a major corruption scandal led to the break-up of DC. Newly created Christian Democratic parties participated in the elections of 1994, 1996 and 2001. The symbols, platforms and politicians of these new parties came directly from the former DC party. However, none of these parties gained a majority, and none of them

share of people who benefited from the reform in treated and control towns, and $1 - dc^0$ is the share of people who would *not* vote DC if there was no reform. We use the following numbers in our calculations: $dc^T - dc^C = 0.04$ or 0.03, the effect of the reform on DC vote share in Figure 5-Panels C-D; $dc^0 = 0.43$ the share of DC in control towns before the reform; $b^C = 0$, the share of beneficiaries in control towns; $b^T = 0.11$, the share of net beneficiaries in treated towns. The share of net beneficiaries in treated towns is equal to the number of households receiving land in the average reform town (244) minus the average number of landlords expropriated (7). We assume that every household casts three votes, so that the average town has $(244 - 7) \times 3 = 711$ net beneficiaries. 6500 voters live in the average town, so net beneficiaries over voters is about 0.11.

²⁷Divorce was still rare in 1981 (0.25% of couples). Moreover, divorces in 1981 are mostly continuous at the border (Appendix Figure D.14) indicating that voters on the two sides of the border did not have different individual interest in seeing divorce repealed.

was able to form a government. In Figure 5-Panels A-B we aggregate the votes of post-1993 DC parties and plot the treatment effect in blue. We do not find significant support for Christian Democrat parties in reform areas in 1994-2001: by 2001 the DC advantage halves in the coordinate specification and entirely disappears in the distance specification. The dramatic change to the Italian political system after 1992 invites caution when interpreting these results.²⁸ Nevertheless, they are consistent with the unraveling of the clientelist system, once Christian Democrats lost access to the government and was unable to credibly commit to transfer public resources to its historical constituencies.

To summarize, the reform generated persistent political benefits for the Christian Democrats. Voters supported DC candidates and its policy agenda, as shown in the analysis of the divorce referendum. After the 1992 corruption scandal, these electoral benefits for Christian democrats decrease substantially, possibly because the candidates were not longer able to commit to benefit its constituents.

5.3 The Effect of the Reform on Clientelist Practices

We present evidence on one source of persistence of the electoral effects: clientelism. We hypothesize that the land reform represented the initial ingredient of a long-lasting system of clientelistic exchange between DC and voters. Gratitude for the initial land transfer was the first step for this process.²⁹ We study the impact of the reform on two reversible practices typical of a clientelist system: targeted benefits controlled by political brokers and patronage. While the land reform was irreversible, these subsequent benefits were easier to withdraw. The increase in clientelist practices may thus have contributed to the enduring political support for DC in reform areas, which lasted after the end of the land redistribution and in spite of profound economic and social changes (e.g., many of the original beneficiaries died and agriculture lost importance by the end of our sample).

Political Brokers: Coldiretti. A large literature emphasizes the role of grassroots organizations as political brokers between voters and politicians (Larreguy et al., 2016; Stokes et al., 2013). *Coldiretti*, one of Italian farmers' associations, is widely acknowledged to play such role for DC. As we discussed Section 2, *Coldiretti* held significant sway among its members because it provided several important services including credit and access to markets.

²⁸For instance, in 1994 and 1996, former DC politicians ran in the lists of major center-right or center-left parties. Thus, we cannot observe vote shares for these Christian Democrat politicians separately.

²⁹Indeed, there is evidence of vivid memory of the reform even many years after its implementation (e.g. Nigrelli and Bonini, 2017; Zucco et al., 2011; Bonini, 2012).

Arguably, *Coldiretti*'s most influential tool was its control of *Casse Mutue*, local institutions providing high-quality healthcare to farmers. While *Casse Mutue* were public institutions, their boards were elective, with members from different farmers' associations running for office. *Coldiretti* candidates received on average 75% of the votes in the elections of *Casse Mutue* boards, and in around one every five elections *Coldiretti* candidates obtained 100% of preferences. Lanza (1991) estimates that in 1955 and 1958 *Coldiretti* effectively controlled around 97% of *Casse Mutue*, and used targeted benefits provided by these institutions to influence voters. To understand the role of *Coldiretti*, we digitized new town-level data on *Casse Mutue* budgets and board elections.³⁰

We find two results. First, Figure 6 shows RDD estimates with data from the 1965 *Casse Mutue* budgets. We observe a treatment effect of 30-40% in both revenues (Panels A-B) and expenses (Panels C-D) per inhabitant. *Casse Mutue*'s boards had some discretion in the use of these funds, and *Coldiretti* members sitting on the boards often used this discretion politically (Lanza, 1991). Larger *Coldiretti* budgets in treated areas suggest that these towns were more exposed to the influence of DC political brokers.

Second, Figure 6 shows RDD results on the board elections 1955-70. Consistent with the previous results, the number of voters in the board elections is substantially higher in treated towns (by 1.4 voters per capita, or approximately 45%: Panels E-F), suggesting again that more people relied on the *Casse Mutue*'s service provision in reform towns. Panels G-H show that approximately 80% of the new voters chose *Coldiretti*, which controlled most *Casse Mutue*.³¹ These results indicate that *Coldiretti* could access a larger base of voters in the reform areas and thus could be more effective in brokering votes for DC.

To summarize, the land reform not only increased the potential member base of *Coldiretti* (i.e. the number of small land owners), but also its actual penetration among small landowners. On average 43% of small landowners who received land through the land reform supported *Coldiretti*. This compares with 25% among pre-reform landowners (71% higher).³² This result suggests that the reform strengthened the role of DC brokers in reform areas.

Public Sector Employment. Governments' patronage in public sector employment is common around the world (Shefter, 1977, cf. Acemoglu et al., 2011 for a theoretical

³⁰We also searched for *Coldiretti* town-level membership, but, as far as we know, it does not exist.

³¹The reform had no significant effect on *Coldiretti* share ($\beta=0.02-0.03$, s.e.=0.03-0.04), which was already very high at baseline (73%).

³²In 1961, there were 12.2 small farms per 100 inhabitants in our main sample. *Coldiretti* received on average 3.1 votes every 100 people: 25% of small farmers supported *Coldiretti*. Treatment effects for small farmers per capita and *Coldiretti* votes per capita are 3% and 1.3% respectively, implying that 43% of the new farmers supported *Coldiretti*.

treatment). The practice was widespread in post-war Italy, where governments routinely appointed political supporters to public offices (Chubb, 1982, Ferrera, 1996, Golden, 2003, Alesina et al., 2001). Using the panel RDD of Equation (1), we test whether reform towns experienced an increase in public sector employment.

Figure 7-Panels A-B presents the dynamic coefficients. In treated towns, the share of public sector employment was not significantly different at the time of the reform ($\beta = -0.01$, s.e.=0.01). Treated towns experience a differential increase in public sector employment in each of the decades following the reform: all coefficients are highly significant in the coordinate specification and they are significant in 1961 and 1981 in the distance specification. In 1981, the year with the highest coefficient, the treatment effect is one-third of the average in control towns (6.2%). When we pool the post-reform data, the treatment coefficient is around 1-1.6 p.p. (20-30% of control mean) in both specifications and relatively stable across bandwidths (Figure 7-Panels C-D). The results are consistent with the idea that DC used patronage to reward its voters in treated towns, especially during the 1980s, when its clientelist system was rampant (Allum, 1997).

Discussion. The results in this section indicate that in the decades after the land reform, treated towns saw an increase in clientelist practices. In treated towns, DC brokers *Coldiretti* had a greater base and more resources to spend on targeted benefits. Moreover, treated towns had a greater incidence of patronage, as proxied by public sector employment. Thus, an irreversible reform that reduced poverty and inequality had the consequence of increasing the incidence of reversible clientelist practices.

We also examine the impact of the reform on fiscal transfers from the central government to the town governments. While distributive politics (pork) is not always associated with clientelism (Hicken, 2011), it was commonly used by DC to maintain consensus (Golden and Picci, 2008). Unfortunately, only very sparse town-level data on municipal budgets survive: ISTAT published town budgets for the years 1952, 1955 and, 1959, but then discontinued the publication for several decades.³³ We present results in the Appendix Figure D.5. The reform had no impact on transfers in 1955, around the end of land redistribution, and two years after the first post-reform Parliament had taken office. In contrast, in 1959, we find with the distance specification a sizable (25%) and marginally significant difference between treatment and control towns (Panel A: $\beta=0.27$, s.e.=0.12, p-value=0.097). In the coordinates specification, the coefficient remains positive and sizable, but it is smaller (10%) and non-significant (Panels B and D). These results thus provide some suggestive support to the idea

³³In Appendix Figure C.1 we show that treated and control towns had similar level of (log) municipal transfers per capita in 1952.

that DC rewarded treated towns with pork after the land reform was completed. However, the differences in the results across specifications grant caution and prevent us from drawing definitive conclusions.³⁴

To conclude, the evidence here is consistent with the land reform leading to a clientelist system based on political brokers and patronage, possibly complemented with targeted transfers.

5.4 Robustness

We have shown throughout that our results are robust to alternative bandwidths. Here, we show that they are also robust to a battery of additional checks.

Alternative Specifications. In Appendix Tables C.1 and C.2, we experiment with different specifications. Panel A reports results for DC vote share, Panel B-E for *Coldiretti* and Panel F for public sector employment. In col. 1 of both tables we report our main specification (Equation 1), controlling for a linear polynomial in distance (C.1) or a quadratic polynomial in latitude and longitude (C.2). In col. 2 we drop provincial seats from the sample (including Rome); in col. 3 we control for a 2nd order polynomial in distance (C.1) or a linear polynomial in coordinates (C.2); in col. 4 we interact the linear polynomial in distance with reform area dummies (C.1) or we no longer allow the effect of coordinates to vary by reform area (C.2). In the remaining columns, we include three different fixed effects \times decade: eight electoral districts (col. 5), ten segments (col. 6), and twenty-nine provinces (col. 7). Results from these specifications are robust and similar to our baseline: if anything, more parsimonious specifications such as a linear polynomial in coordinates point to stronger effects.³⁵

We also split the reform border into 10 segments and assign every town to one of these segments (Appendix Figure C.7). We then estimate the effect of the reform after dropping towns close to each of these 10 segments. Figure C.8 presents the results for DC (Panels A-B), *Coldiretti* (Panels C-J) and public sector employment (Panels K-R). The coefficient of public sector employment in the last decade (1991) becomes insignificant when we drop one

³⁴Contrary to a large literature (e.g. Brollo and Nannicini, 2012; Martinez-Bravo, 2014), we find no evidence that local politicians helped attract pork. Appendix Figure D.6 asks whether treated towns with DC mayors in 1950 received greater fiscal transfers in the 1950s, and shows that the impact of the reform did not vary by mayor's alignment. The result suggests that national politicians controlled pork, consistent with both anecdotes (Bracalini, 2016) and systematic evidence in Golden and Picci (2008).

³⁵When we fit the linear polynomial in coordinates, we find positive and significant pre-trends in PCI (but not DC) vote shares at larger bandwidths. Therefore, we prefer to report the second order polynomial in coordinates as main specification for the multidimensional RDD.

segment, but with this exception no specific section of the reform border drives the results.

Instrumental Variable: the Borders of the Original Legge di Riforma Agraria.

Reform areas that redistributed land largely followed the definition of “Zone B” spelled out in the *legge di riforma agraria*. As we discuss in Section 2, the reform areas were drawn with the inputs of agricultural technical consultants, with no evidence of strategic manipulation. Later changes in the border definition were minor (8%). Here, we show that our results hold when we consider the original borders of the *legge di riforma agraria* as instruments for the actual reform borders.

We draw a new map of the area that the *legge di riforma agraria* assigned to the “Zone B” (Appendix Figure A.1-Panel A). With these new borders we create alternative treatment and distance variables. Next, we use the proposed reform treatment and distance as instruments for the corresponding variables from the actual reform. Reduced form, first stage and IV estimates for all our outcome variables are respectively in Appendix Tables C.3 and C.4 and confirm all our baseline results. Historical records suggest that border changes in the reform areas in Northern and Central Italy were minor and not strategic. The IV in this section confirms that these changes do not drive our results.

Placebo Borders. Appendix Figure C.9 presents the results of the following experiment. We simulate 20 fictitious reforms, by moving the reform border inside and outside the reform area in steps of 2.5 km. For each of these fictitious reforms, we estimate a single coefficient for the impact of the reform. In Figure C.9 in Panels on the left we plot the 20 coefficients of these regressions (on the y-axes) against the location of the fictitious border (on the x-axes). In the same graphs we also report the real coefficient (in red). In Figure C.9 in Panels on the right we repeat the exercise but plot the *t*-statistics of our coefficients. The Figures shows that for most outcomes the *t*-stat estimated on the real border is higher than every other *t* estimated on fictitious borders. When it is not the highest, the true *t* is only slightly lower than the highest *t*-stat, which lies 2.5 km outside (e.g. public sector employment) of the border. This exercise can be seen as a form of non-parametric evidence (in the spirit of randomization inference), and its results should not be affected by the special form of correlation of error terms. Taken together, these results show that the actual border of the reform is the only source of discontinuity in our sample.

Spatial Standard Errors. Appendix Table C.7 reports results with standard errors robust to spatial correlation (Conley, 1999). In this exercise, we allow errors to have any correlation over time. In addition, we allow non-zero spatial correlation across towns, and assume that spatial correlation decays linearly until a cutoff. We experiment with different

cutoffs, and report standard errors and significance on Appendix Table C.7. Correcting for spatial correlation has no material impact on the significance of our main results, and for some outcomes it reduces standard errors in specifications with larger bandwidths (*Coldiretti* budgets).

6 Other Effects of the Reform and Alternative Explanations

Since the Italian land reform never extended to control areas, our empirical strategy allows us to credibly trace the impact of the reform over several decades. In Section 5 we showed that the reform created persistent electoral gains for DC and we documented the emergence of clientelism in reform areas. In this Section we consider alternative explanations for the persistent electoral effects including migration, growth and development, voters' conservatism, and mechanical persistence arising from serial correlation in voting.

6.1 Migration

The reform and its consequences may impact migration from or to reform areas. Here, we first analyze population levels and population composition data and then discuss the implications of these results.

Total Population. The reform may affect the patterns of migration. To explore this channel, we estimate Equation (1) with the log of the number of eligible voters as dependent variable. Appendix Table D.1-Columns 1 and 4 show the estimated β_t with our two specifications. There is little evidence of differential migration in the 1950s, the first decade after the reform: the point estimate is zero in the coordinates specification and negative but small when controlling for distance from the border. Starting from the 1960s treated towns appear to lose population. The effect is more precisely estimated in the coordinates specification and indicates that by the early 1990s treated towns were 15% less populous than control towns.

Population Composition. We next consider how out-migration affected the demographic composition of treated towns by looking at the decadal censuses.³⁶ Appendix Table D.3 presents the results with our two specifications. The table indicates that emigrants were

³⁶When the dependent variable comes from the decadal censuses we estimate the effect relative to the 1951 census. This census was taken before land expropriations started and captures population characteristics before the reform. Relatedly, we find (non-significant) out-migration also when we estimate Equation (1) with log of census population or with an estimated measure of net migration as dependent variable.

more likely to be young (cols. 1-4 and 6-9) men (cols 5 and 10). By 1991, treated towns had 0.4% fewer males (base = 50%) and between 0.2% and 1.6% fewer under-20 (base = 25%). Similar to the total population, these effects grow over time and are precisely estimated only when we control for a polynomial in coordinates. The emigration of young males is consistent with common patterns of economic migration across developing countries. For instance, Galán (2018) finds that the children of land reform recipients exhibit higher migration to urban centers.

Discussion. Migration is unlikely to explain our main electoral results for several reasons. First, the pattern of treatment effects on population changes does not match those on electoral results: the patterns uncovered in Appendix Tables D.1 and D.3 suggest that differential out-migration was relatively small in the 1950s, right after the reform, and then it grew over the following decades. In contrast, Figure 5 shows that support for DC in treated towns increased sharply immediately after the reform, and then remained stable over the following forty years.

In principle, immediate out-migration of PCI supporters from treated towns could help explain the increase in DC vote share. However, we find little evidence of emigration from treated towns in the 1950s. In addition, in Appendix Table D.1 cols 2 and 4 we report the β_t of equation (1) when the dependent variable is the log of the *number* of DC votes. Importantly, the table shows that in the 1950s the number of DC votes in treated towns increased by 9.4 p.p. in the distance specification (*p*-value = 0.108) and by 7.2 p.p. in the latitude-longitude specification (*p*-value = 0.030). These new DC voters were unlikely to be moving in from control towns because the vast majority of beneficiaries were previous resident of treated towns (Section 2). Instead, it is likely that the land reform persuaded local residents to support DC. A simple back-of-the-envelope exercise suggests that the increase in the number of DC votes explains at least three-quarters of the DC vote share effect (as opposed to outmigration of voters of other parties).³⁷

Migration patterns are also relevant for the analysis of clientelism. A large literature suggests that, by increasing voters' mobility and outside options, migration may weaken clientelism (e.g. Stokes, 2005; Kitschelt et al., 2007). In the case of the Italian land reform, we do not observe such an effect. Treated towns experience greater outmigration by the

³⁷For this exercise, we assume, consistent with Figure D.12, that there is no change in turnout. Given the baseline DC vote share of 43.1% in 1948, keeping constant the total number of voters, the 1953 treatment effect for the number of DC votes (7.1% with distance and 3.6% with coordinates) corresponds to an increase in the DC vote share of 3.1% (1.6%) for the distance (coordinate) specification. The 1953 treatment effect on the vote share is 4% (1.6%) with the distance (coordinate) specification, which implies that the absolute increase in DC votes can explain between 76% and 95% of the observed effect on vote shares.

1980s but this is not accompanied by a reduction in support for DC nor by less patronage.

6.2 Economic Growth and Development Patterns

The land reform may also have political consequences through its impact on economic growth and development.³⁸ In this section, we consider the effect of the reform on several economic outcomes.

Wealth and Income. We start with two obvious outcomes that would be affected if the reform had major economic consequences: wealth and income. Our wealth indicator is the home-ownership rate, as most Italian households accumulate wealth by buying their home (Rossi, 2019). Second, we look at income per capita in 1981, the earliest year for which this variable is available.

Appendix Table D.15 reports results for Equation (1) with home-ownership as dependent variable. The treatment coefficients in the five decades after the reform are small and almost never significant. The table shows that the reform had a small positive effect on home ownership in 1961 (only in the coordinates specification). This effect, however, vanished already from the following decade and remains small and insignificant for the rest of the period. While the reform may have promoted higher home ownership in 1961, the effect appears small, short-lived and not robust across specifications. This speaks against major effects of the reform on wealth accumulation.

Appendix Figure D.16 report estimates of Equation (2) with income per capita as dependent variable. Both specifications indicate that the reform did not make treated towns richer in 1981. Together, these results speak against a persistent effect of the reform on wealth or income.

Agricultural Productivity. We then consider the role of agricultural productivity. While, to the best of our knowledge, town-level data on agricultural productivity do not exist, it is plausible that, at least in the short run, the reform increased agricultural productivity (Barbero, 1960). However, two observations suggest that agricultural productivity is unlikely to explain the electoral results. First, the electoral effects of the reform appear already in the 1953 elections, which took place after the expropriations, but before most of the reform beneficiaries received the land (and even fewer had collected the first harvest). Second, if the electoral impact of the reform was driven by its impact on agricultural productivity, we would expect it to fade overtime, as agriculture lost importance during the second half of the

³⁸Much empirical work on land reform focuses on economic outcomes, such as agricultural productivity (Montero, 2018; Adamopoulos and Restuccia, 2020), poverty reduction (Banerjee et al., 2002; Besley and Burgess, 2000), and structural transformation (Galán, 2018).

twentieth century (Appendix Figure D.18-Panel A). This is not what we find, as the effect of the reform is stable until 1992.

Labor Force Participation and Structural Transformation. In Section 5.3, we showed that the reform increased public sector employment, a form of patronage. Here, we provide additional details on labor force participation and sectoral labor shares. Appendix Table D.2 shows a decline in labor force participation in treatment areas. This decline already emerges in 1961 and it is estimated more precisely in the coordinates specification. We conjecture that *measurement* issues drive this decline: the reform is likely to have induced a shift from employed work in agricultural to own-farm work. Census enumerators were instructed to count women primarily occupied on household activities (including those working part-time on the household farm) as out of the labor force (ISTAT, 1955, Dati sommari per Comune - Avvertenze). Thus, the wives of land beneficiaries may have been counted among the rural workers in the 1951 Census, and as out of the labor force afterwards, when they helped their husbands on the new family farm. Two pieces of evidence confirm this hypothesis. First, the decline in the labor force share is driven primarily by a reduction in the share of workers in agriculture (cols. 4 and 10). Second, these declines are found only among women, not among men (cols. 3 and 9).³⁹

We find little evidence of declines in manufacturing or services (i.e., if anything, the service share increases by a small amount). We conclude that the reform neither hindered nor fostered structural transformation.⁴⁰

Firm Growth. Third, we consider firms' growth, using information from the economic census (including the newly digitized 1951 and 1961). We focus on two variables: number of plants per capita and number of workers per plant. Appendix Table C.1 showed that these outcomes are balanced in 1951. Appendix Table D.4 reports estimates of the panel spatial RDD Equation (1). There is no effect on the number of plants per capita. We find suggestive evidence of a reduction in the number of workers per plant, starting in 1971. These results are consistent with the decline in population we discussed earlier (Table D.1).

Discussion. To summarize, analysis in this section suggests limited impact of the reform on measures of growth, including wealth and income, sectoral reallocation, and firm development. Only plant size appears to fall in treated areas. However, the effect emerges

³⁹On the contrary, the results on public sector employment (Section 5.3) are not driven by women.

⁴⁰In a subsequent paper, Albertus (2020) looks at the effect of the land reform in Maremma and finds that treated towns in this area are less developed in 2011 and had a *higher* share of workers in agriculture. He estimates this effect on a repeated cross-section of 100 matched towns on the two sides of the Maremma border. We do not detect this effect in our larger sample and using panel RDD (town fixed effects).

two decades after the reform, and it is not robust across specifications. We conclude that the electoral impact of the reform, which appears immediately and remains stable overtime, is unlikely to be driven by these changes.

6.3 Economic Conservatism

We now turn to a different explanation: economic conservatism. Evidence from other countries shows that wealthier voters become economically conservative (Di Tella et al., 2007), endorsing parties that promote free markets and oppose redistribution (De Janvry et al., 2014).⁴¹ This phenomenon may offer another plausible explanation to the persistent electoral effect of the reform, as beneficiaries of the land redistribution became richer than control farmers who did not receive land. Land beneficiaries may then be more conservative than families on the other side of the border simply because they had more wealth. Their children may also be more conservative because of inter-generational transmission of wealth, of values—or both. Several pieces of evidence do not support this interpretation. First, the premise of the economic conservatism argument is that voters are richer in treated towns, but we already showed the reform did not have any significant long-term impact on wealth (home ownership) or income per capita. Second, economic conservatism cannot explain why in treated towns voters support DC agenda on *family* policies in the 1974 divorce referendum. In this section, we present a third additional piece of evidence that speaks against this interpretation.

Support for Forza Italia and Other Right-wing Parties. The crisis of the Italian political system in the early 1990s provides an opportunity to test whether treated towns differentially support right-of-center parties other than DC. Between 1946 and 1994 DC was the only major right-wing party. Thus, for this period, it is not possible to disentangle support for DC from more general support for economically conservative parties. The crisis in the 1990s led to the break-up of DC, and ushered in power Berlusconi, the leader of the newly founded *Forza Italia*, who campaigned on a strong pro-market and low-tax platform. DC split in several smaller parties, representing the various factions of the party.

If richer voters in treated towns favor more conservative policies regardless of the identity of the right-wing party that implements them, we would expect greater support for Berlusconi in reform areas. We test this idea with the RDD model and data on Berlusconi's party vote shares in the elections of 1994, 1996 and 2001. Appendix Figure D.17-Panels A-F report

⁴¹Wealth is an important determinant of preferences for redistribution (Giuliano and Spilimbergo, 2013; Fisman et al., 2015), and these preferences in turn affect voting (Fisman et al., 2017).

the treatment coefficient on *Forza Italia* vote share for multiple bandwidths. Treatment coefficients are small and insignificant in each election year. Moreover, controlling for DC vote share in 1948 has no effect on point estimates. When we look at the entire center-right coalition, we obtain similar results (Appendix Figure D.17-Panels G-L). This evidence speaks against the intergenerational transmission of conservative values as a driver of electoral persistence.

Discussion. To sum up, economic conservatism is unlikely to explain the persistence of our electoral results for three reasons. First, available data does not suggest that treated towns became richer. Second, this mechanism cannot explain the support for the DC *family policy* agenda during the 1974 referendum. Third, support for DC did not translate in support for the next right-wing party after 1994.

6.4 Mechanical Persistence

The initial electoral effect of the reform may persist simply because town-level vote shares are highly correlated over time. However, since this correlation is less than one, purely mechanical persistence would result in a treatment effect fading overtime. Appendix Figure D.18-Panel B presents correlation in vote shares across each pair of elections in our sample and indicates that pairwise correlation is well below one even in successive elections. This in turn suggests that mechanical persistence of electoral preferences is unlikely to explain the *stable* effect of the reform over 40 years. For instance, the town-level correlation of voting results between the 1953 and the 1992 elections is only about 25%.

7 Conclusion

How do large redistribution policies affect clientelism? On the one hand, by decreasing poverty and inequality, redistribution may reduce pervasiveness of clientelistic systems (Robinson and Verdier, 2013). On the other hand, redistribution may induce beneficiaries' reciprocity, a crucial ingredient to sustain clientelist exchange (Finan and Schechter, 2012). In this paper, we examine this relation in the context of the Italian land reform, which redistributed land from large owners to small producers. We exploit the fact that the reform redistributed land within well-defined reform areas and identify the long-term causal effect of the reform with a panel spatial regression discontinuity.

After the reform, treated towns experience an increase in the electoral support for DC, the major party that promoted the reform. These electoral gains persist for over 40 years. The

reform laid the foundation for a long-lasting clientelistic system, characterized by political brokers and patronage, well-known hallmarks of DC rule during this period (Chubb, 1982; Lanza, 1991). The combination of initial large-scale redistribution and subsequent reversible benefits (e.g. brokers-controlled health care and patronage) highlights how even irreversible policies may be part of the bundle of strategies used by clientelist parties to generate sustained support.

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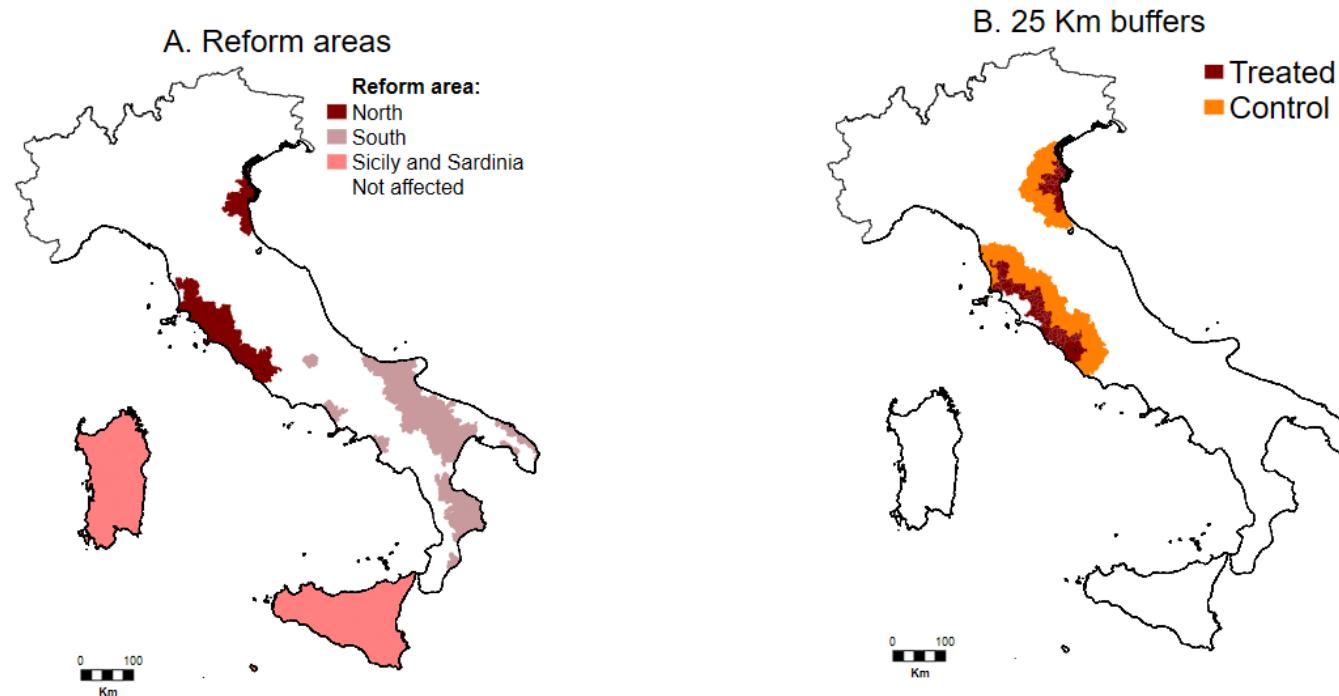
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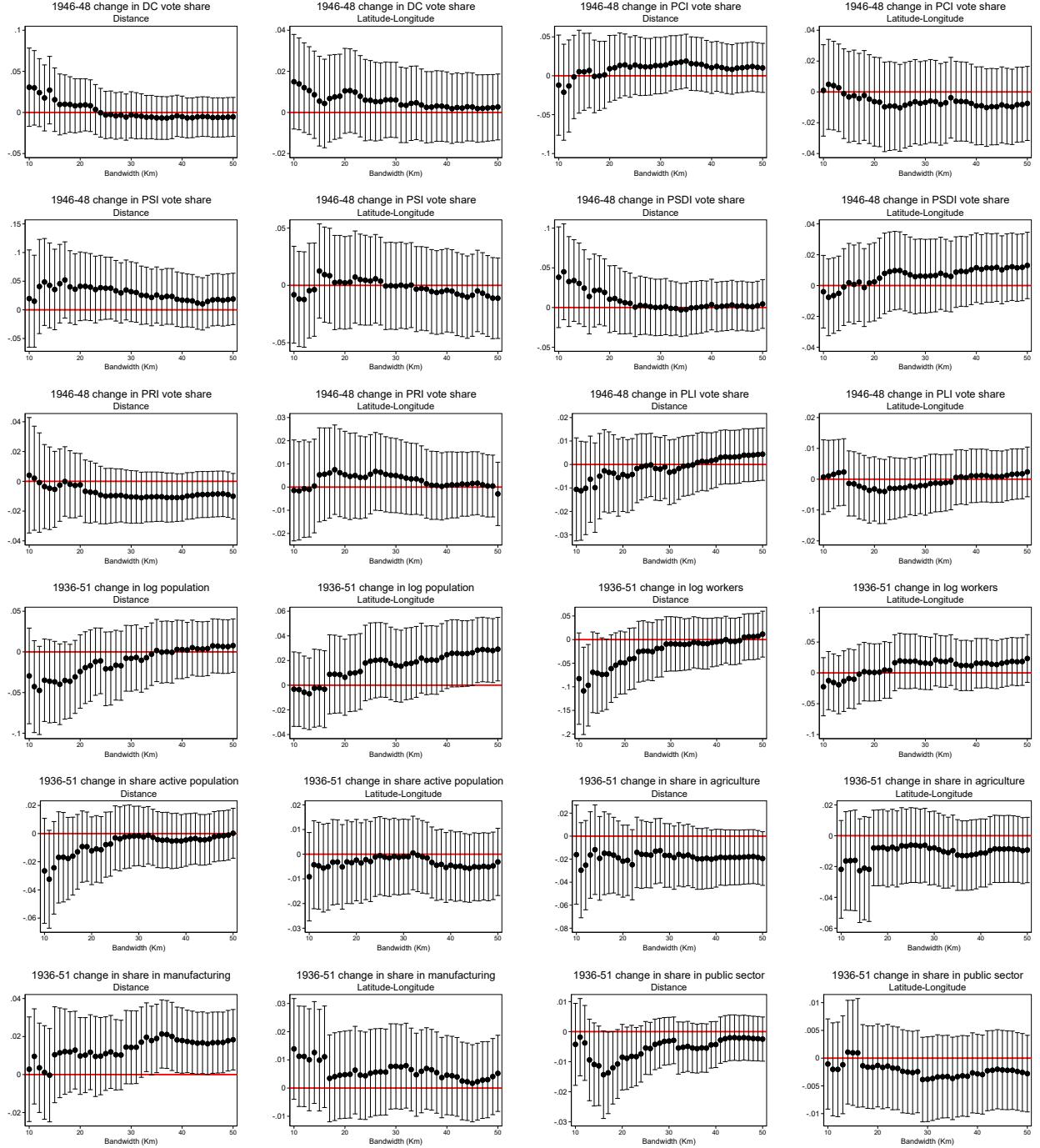
Figures

Figure 1: Reform Areas and Buffers



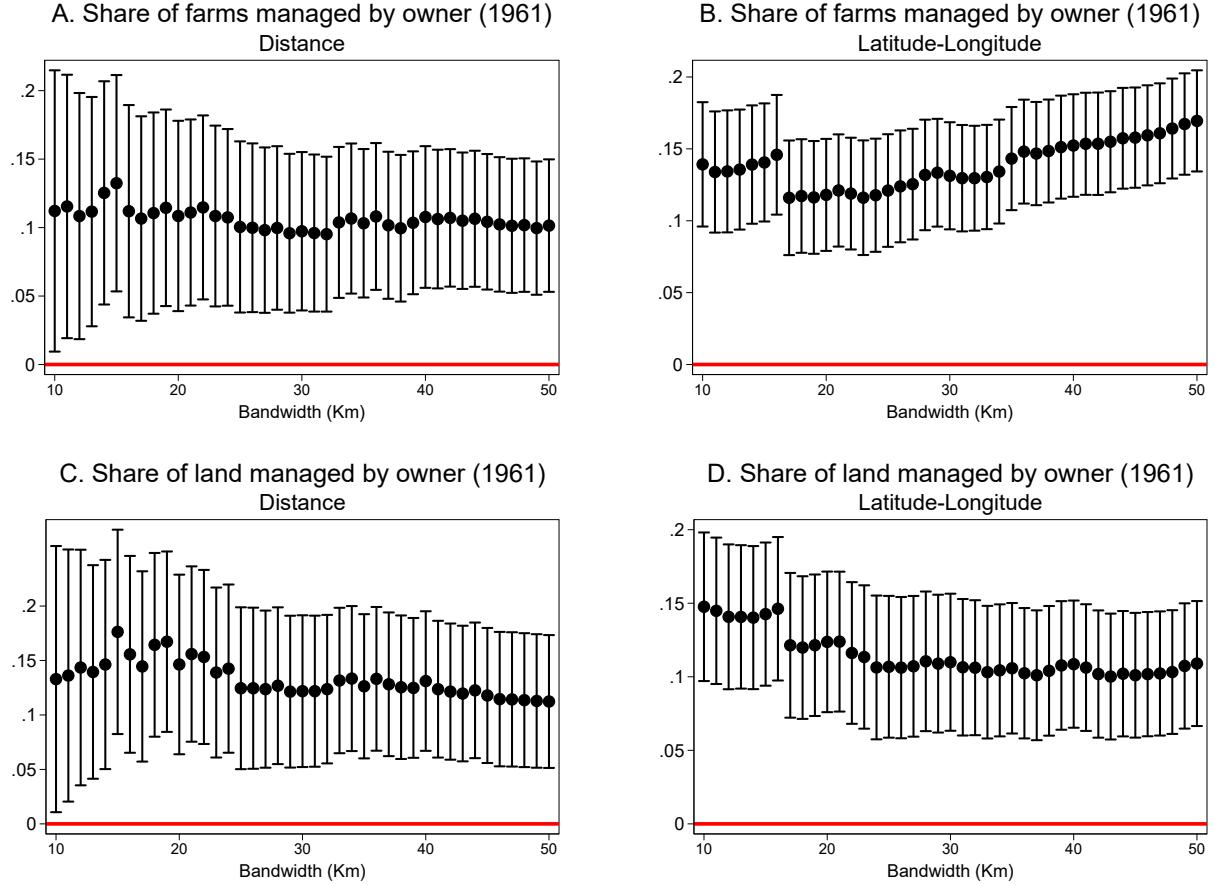
Notes: Panel A: land reform areas as defined in the 1951 executive orders (D.P.R.66/1951; D.P.R.67/1951; D.P.R.68/1951; D.P.R.69/1951; D.P.R.70/1951; D.P.R.264/1951; D.P.R.265/1951). In dark red the areas of Delta Padano (North-East) and Maremma (Center-West). In light brown the areas of Fucino (Center), Opera Combattenti (South-West), Puglia and Lucania (South-East), Sila (South). In pink the islands of Sicily and Sardinia. Panel B: 25 km buffers inside and outside the border of Delta Padano and Maremma (preferred bandwidth).

Figure 2: Pre-Trends at the border



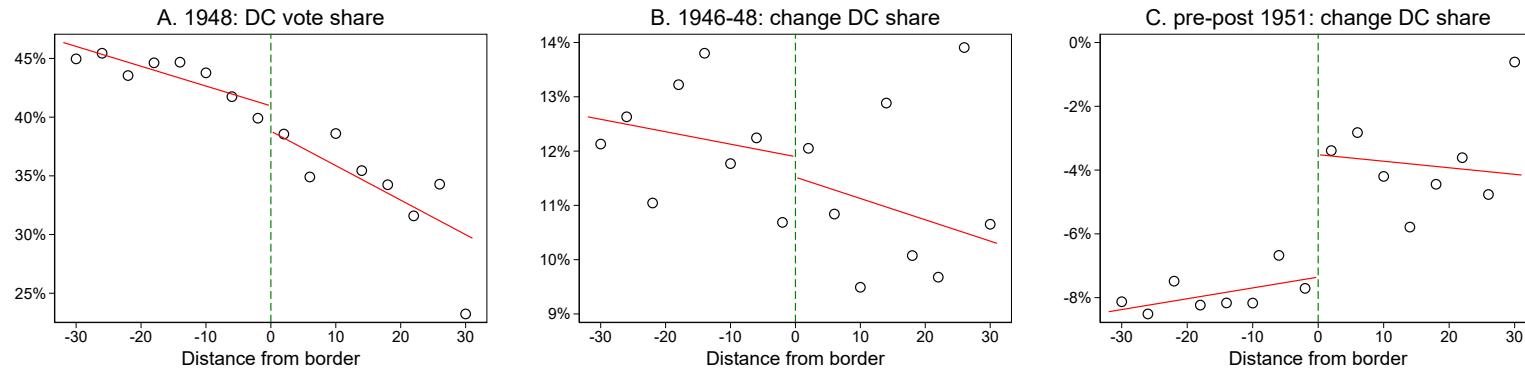
Notes: The graphs report the coefficients of regressions of the RDD specification in Equation (2) along with 95% C.I. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. Dependent variables are changes in the outcome variable between 1946 and 1948 for electoral variables and between 1936 and 1951 for demographic and economic variables. See Appendix B for data description and their sources. Units of observation are towns. The sample consists of all towns close to the reform borders of Delta Padano and Maremma.

Figure 3: The impact of the reform on farm management.



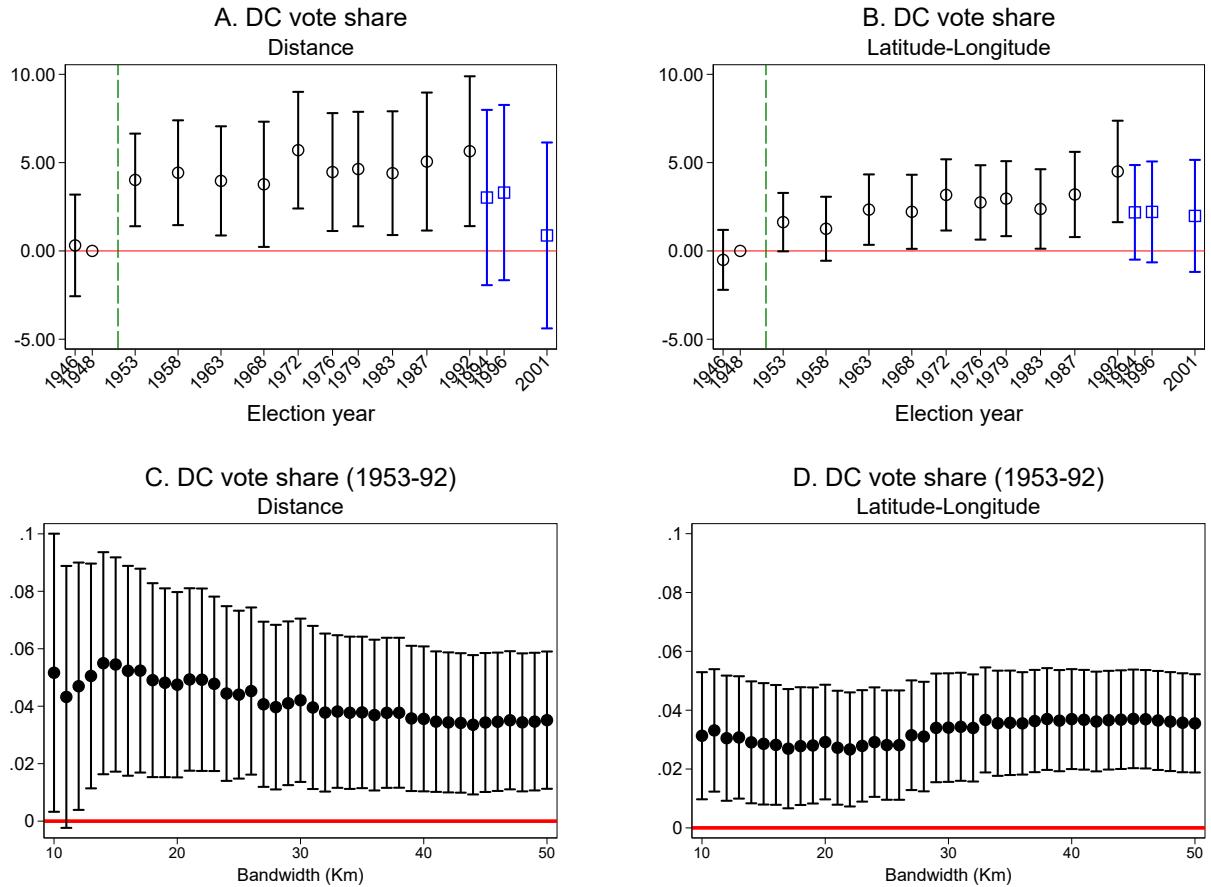
Notes: The graphs reports β of RDD regression Equation (2) and 95% confidence intervals, for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. Panels A and B: dependent variable is share of *farms* managed by the farms owner in 1961. Panels C and D: dependent variable is the share of *land* managed by the farm owner in 1961. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Results are similar when controlling for the dependent variable observed in 1929. Source of 1961 farm management is the 1961 Agricultural Census; source of 1929 variables is ISTAT (1936). Units of observation are towns. We estimate heteroschedasticity robust standard errors and plot 95% confidence intervals as bars around the coefficients.

Figure 4: Balance, pre-trends and effect of reform on DC vote share: graphical evidence



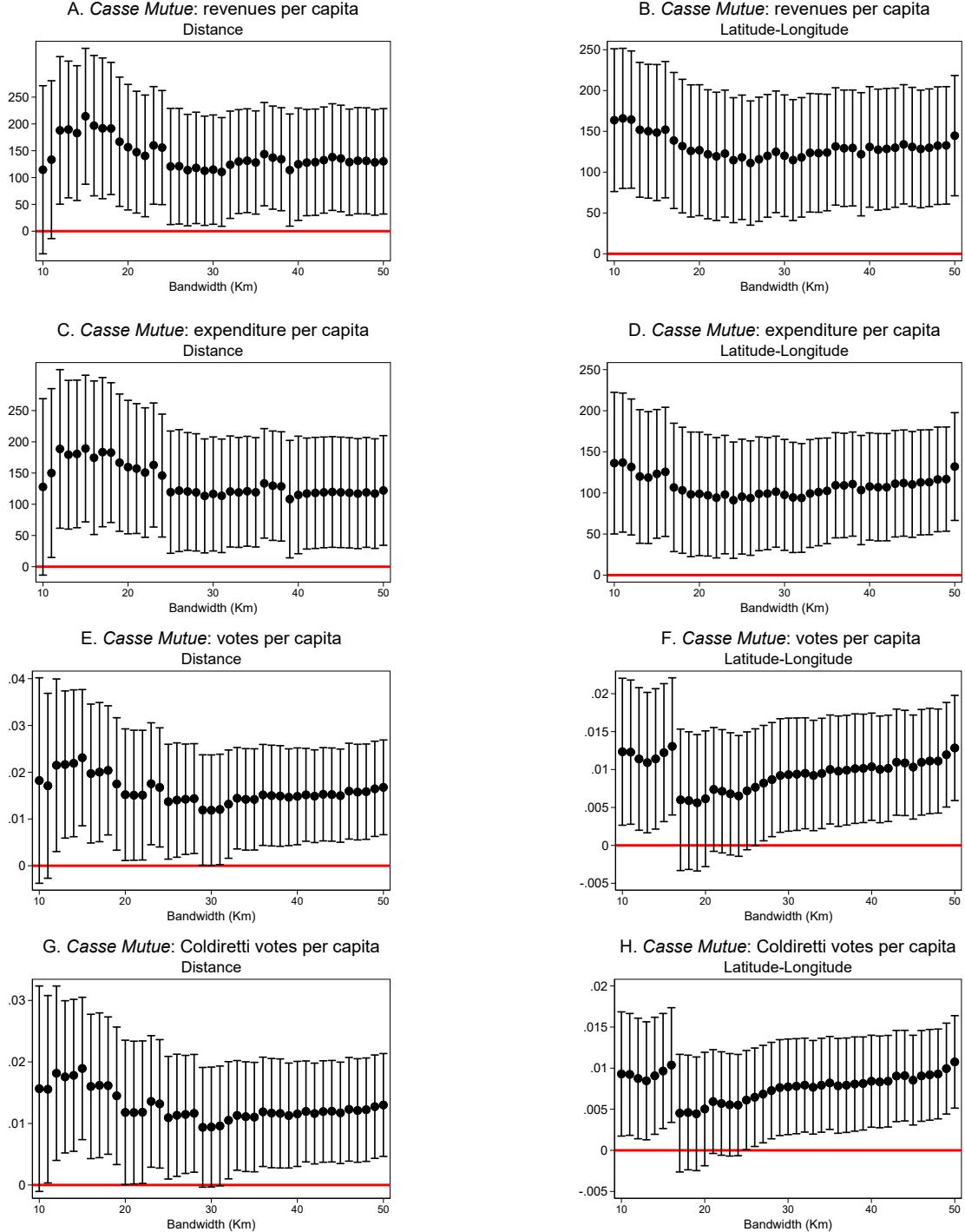
Notes: The Figure presents graphical evidence on the panel RDD. On the y-axes we plot electoral outcomes; on the x-axes the distance to the border. In each Panel, we bin data in 4 km intervals. Treated towns have positive distance and control towns have negative distance. The red lines report linear fits from regressions of the outcome on the distance from the border (separately for the two sides of the discontinuity). Panel A: dependent variable is Christian Democrats (DC) vote share in 1948 (the last election before the land reform). Panel B: dependent variable is change in DC vote share between the 1946 and 1948 (the two elections before the land reform). Panel C: dependent variable is change in DC vote share between pre- (1946-48) and post-reform elections (1953-92). Electoral data are from Corbetta and Piretti (2009). The sample consists of all towns close to the reform borders of Delta Padano and Maremma.

Figure 5: The Electoral Impact of the Reform: Panel RDD Results



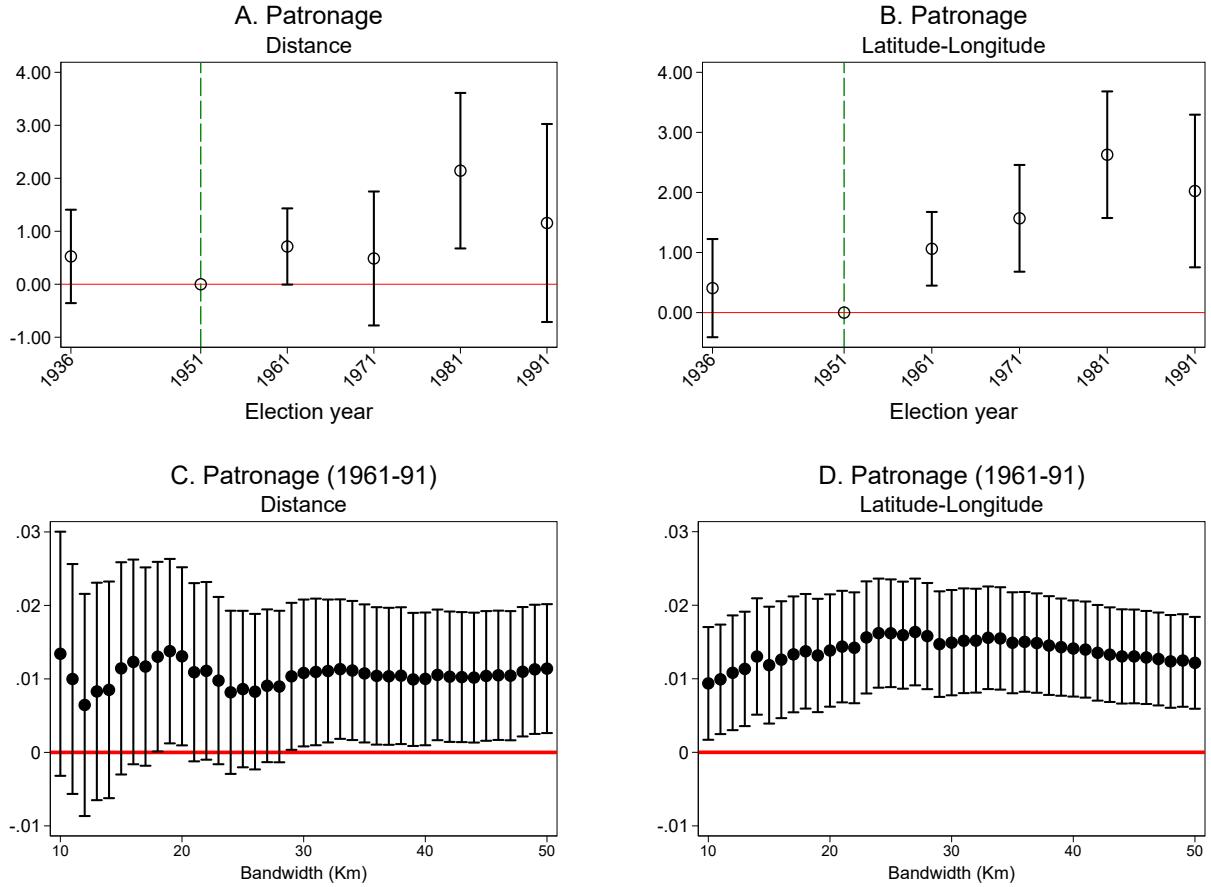
Notes: Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients β_t from the panel RDD Equation (1), $t = \{1946, \dots, 2001\}$. The omitted category is the β of 1948 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The vertical dashed lines mark the 1951 land reform. In the post-1992 elections we consider DC the following parties: *Italian Popular Party* and *Patto Segni* (1994); *Italian Popular Party, Lista Dini*, the Christian Democratic Center and the Christian Democratic Union (1996); *Margherita*, Christian Democratic Center and the Christian Democratic Union (2001). Panels C and D report pooled panel RDD coefficients for all years between 1953 and 1992. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for year \times reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure 6: The Impact of the reform on *Coldiretti*: RDD Results



Notes: The graphs display the β of RDD Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. Regressions in Panel A, C, E, and G control for a linear polynomial in distance, regressions in Panel B, D, F, and H control for a quadratic polynomial in latitude and longitude. Panel A, B, C, D use data from 1965 *Casse Mutue* budgets (Fed. Naz. Casse Mutue, 1966). Units of observation are towns. Panel E, F, G, and H use data from 1955-70 *Casse Mutue* elections (All. Naz. dei Contadini, 1970). Units of observation are town-years: we pool all years and add year \times reform area fixed effects. Standard errors heteroskedastic robust (Panel A to D) and clustered at the town level (Panel E to H).

Figure 7: The impact of the reform on patronage.

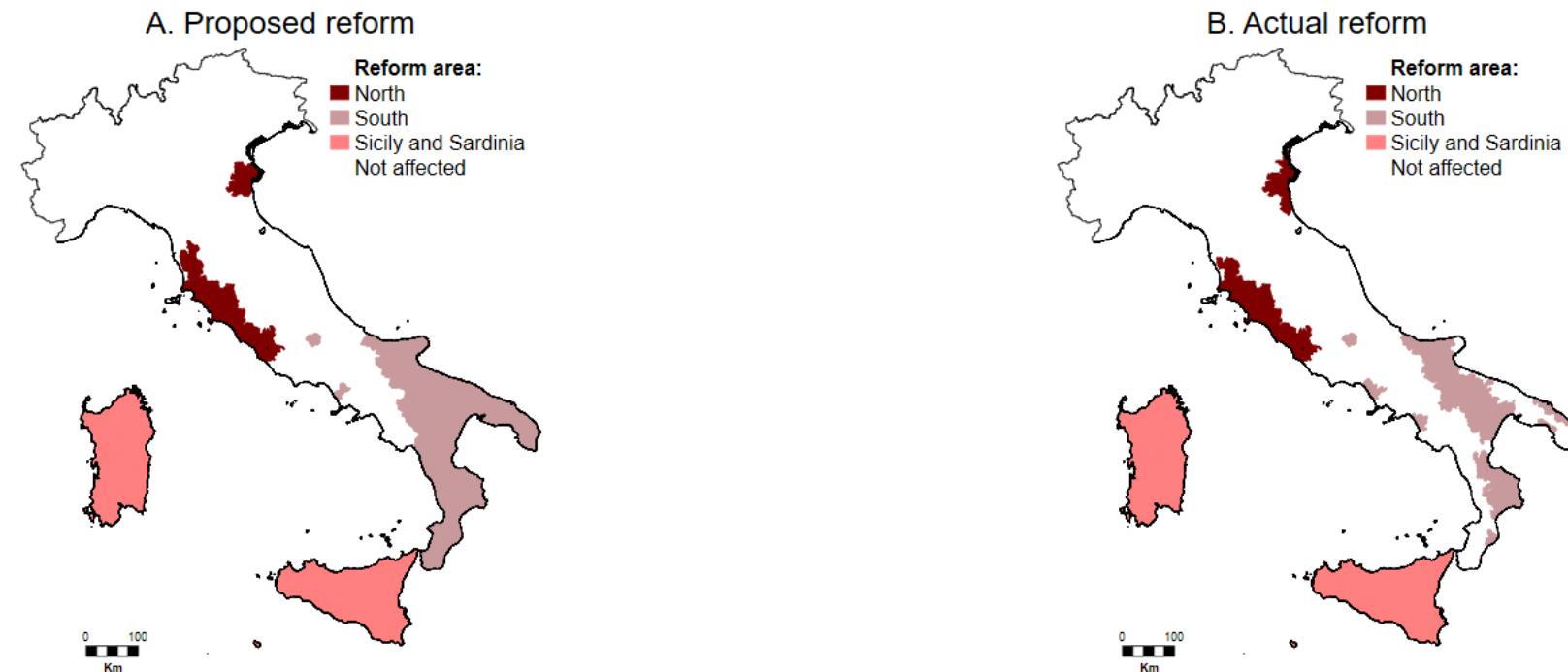


Notes: The dependent variable is the share of active workers employed in the public sector, a form of patronage. Panels A and B display coefficients β_t from the panel RDD Equation (1), $t = \{1946, \dots, 2001\}$. The omitted category is the β of 1951 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The vertical dashed lines mark the 1951 land reform. Panels C and D report pooled treatment effects for all census years between 1961 and 1991. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for year \times reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Appendix (For Online Publication)

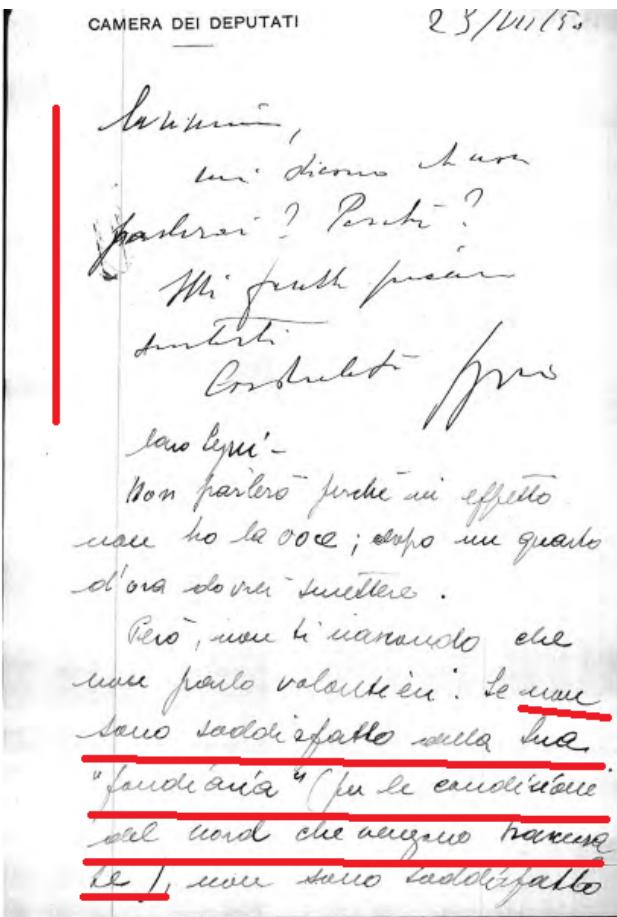
A The 1950 Italian Land Reform

Figure A.1: Proposed and actual reform

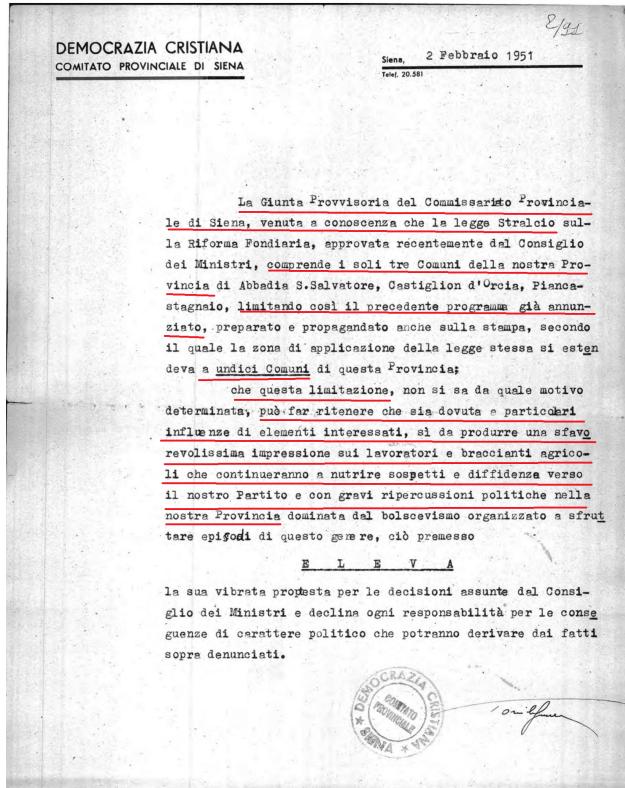


Notes: Panel A: land reform areas as defined in the April 1950 *legge di riforma agraria*, which was proposed but later modified. Panel B: land reform areas as defined in the 1951 Executive Orders. In dark red the areas of Delta Padano (north-east) and Maremma (center-west). In light brown the areas of Fucino (centre), Opera Combattenti (south-west), Puglia and Lucania (south-east), Sila (south). In pink the islands of Sicily and Sardinia.

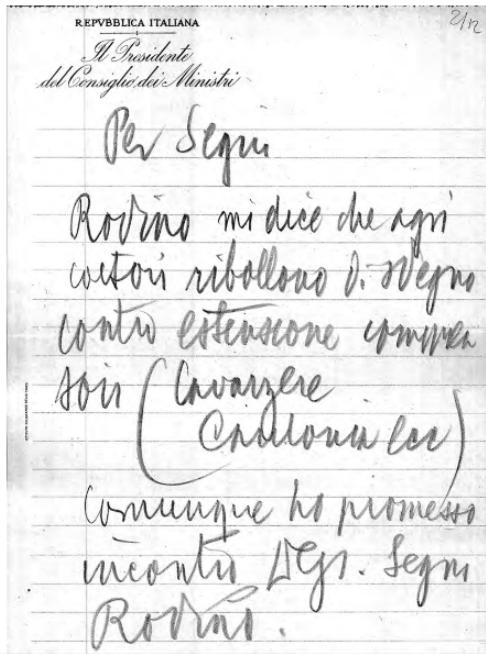
Figure A.2



(Panel A) Messages between Segni and Sampietro

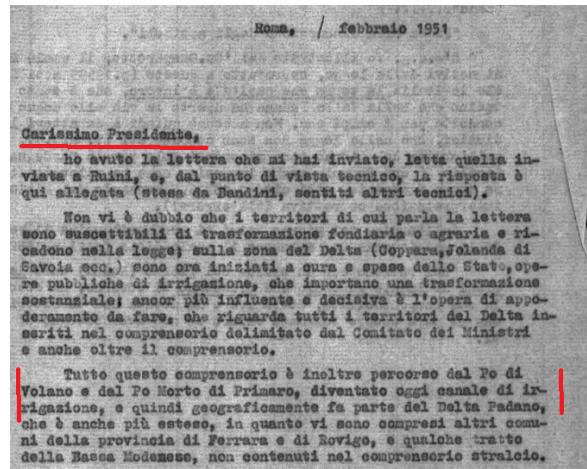


(Panel B) Letter to Segni from DC Siena Committee



(Panel C) Note to Segni from De Gasperi

Notes: Panel A: Exchange between Ministry of Agriculture Segni and DC MP Sampietro on official Italian Parliament stationery (1950). Highlighted text: Segni: "They tell me you won't talk. Why? I would be happy to listen to you." Sampietro: "[...] I am not happy with your land reform (because the situation in the North is neglected) [...]" Panel B: letter to the Ministry of Agriculture Segni from the DC Provisional Committee of Siena (1951). Highlighted text: "The Committee, has learnt that the *legge stralcio* [...] only includes three towns in the province of Siena, [...] instead of the planned [...] eleven; such revision, [...] which may appear to have been influenced by interested parties [landowners], will produce a very negative impression on rural workers, who will continue looking at our party with mistrust and suspicion and it will have negative political consequences in our province [...]." Panel C: note to the Ministry of Agriculture Segni from the Prime Minister De Gasperi on an official PM stationery (1951). "To Segni. Rodinò [a marquis, president of the landowner's association *Confagricoltura*] tells me that landowners seethe in resentment for the extension of the reform area (Cavarzere [in Delta Padano, eventually included in the reform area] ecc.). In any case I promised a meeting Segni Rodinò." Panel D. Letter to the PM De Gasperi from Ministry of Agriculture Segni (1951). Highlighted text: "Dear President. [...] The entire area [he refers here to towns in Delta initially not included in the reform area] is cut by the *Po di Volano* and *Po Morto di Primaro*, today an irrigation canal, thus the area belongs geographically to the Po estuary [Delta Padano]." Panel E: Technical report prepared for the Ministry of Agriculture to help him respond to criticism for modified reform borders (Bandini, 1951). Highlighted text: "Maremma. The final border was smaller than what we ought have done – for the well-known financial reasons [...]" Images ©Fondazione Antonio Segni: Serie IV (Attività politica), Sottoserie 2 (anni 1947-51 - ministro MAF), Fascicolo 1. All rights reserved. With thanks to Fondazione Antonio Segni and Salvatore Mura.



(Panel D) Letter to De Gasperi from Segni

2) **MAREMMA** – La delimitazione fatta – per le ricordate ragioni di ordine finanziario – è più ristretta di quel che si sarebbe dovuto largamente fare. Non si sono compresi i territori estensivi della provincia di Siena, di gran parte della campagna romana, di alcune parti di Pisa, Lì vorno, Latina, Frosinone, appunto per tali considerazioni.

(Panel E) Technical Report for the Ministry of Agriculture

Figure A.3: Expropriation Criteria

		Percentuali di scorporo riferite agli scaglioni di reddito imponibile								
		Imponibile medio per Ha.								
		Lire								
SCAGLIONI DI REDDITO IMPONIBILE TOTALE		1000 e oltre	900	800	700	600	500	400	300	200
Lire		e meno								
Fino a 30.000		— — — — — — — — — —								
Da oltre 30.000 a 60.000		— — — — — — 0 15 30 55								70
» 60.000 a 100.000		— — — — — — 0 10 30 60								70
» 100.000 a 200.000		35 40 47 55 60 65 70 75 84								90
» 200.000 a 300.000		45 50 55 60 65 70 75 80 87								95
» 300.000 a 400.000		52 57 60 65 70 75 80 85 90								95
» 400.000 a 500.000		60 64 66 71 76 80 85 90 95								95
» 500.000 a 600.000		64 70 76 78 80 85 90 95 95								95
» 600.000 a 700.000		68 74 79 82 85 90 95 95 95								95
» 700.000 a 800.000		72 78 82 85 90 95 95 95 95								95
» 800.000 a 900.000		76 82 86 90 93 95 95 95 95								95
» 900.000 a 1.000.000		82 86 90 93 95 95 95 95 95								95
» 1.000.000 a 1.200.000		90 92 95 95 95 95 95 95 95								95
Oltre 1.200.000		95 95 95 95 95 95 95 95 95								95

Notes: Annex to the 1951 Law. The Table specifies the share of land to be expropriated for different estates. On the x-axis estates are ranked according to their productivity (average taxable income per ha: on the left the most productive, on the right the least productive). On the y-axis estates are ranked according to size (total taxable income: at the top the smallest, at the bottom the largest). Every cell specifies the percentage of land to be expropriated.

Figure A.4: Original applications

49

Mod. D. A. T. U. N. 3.60
 Rosso!!!
 B non agric.
 azienda
 prof. Edile
 Uniti Regg.
 H/M
 Cognato (1) M. di categoria 7382
 N. di registrazione
 23

ENTE PER LO SVILUPPO DELL'IRRIGAZIONE
 E LA TRASFORMAZIONE FONDIARIA IN PUGLIA E LUCANIA
SEZIONE SPECIALE PER LA RIFORMA FONDIARIA
 Via Dante Alighieri N. 40 - BARI - Via Dante Alighieri N. 40

OGGETTO: Domanda di assegnazione di terreni.
 22

Il sottoscritto Maurizio Naurino di Cerignano
 e di Rolli Lucchesi nato a Montebelluno 15 e residente
 in Montebelluno Taranto Via Eruca N. _____ avendo la qualifica
 principale di (2) Braccianti e la secondaria di (3) Contadino
 così come risulta dal proprio Libretto di Lavoro N. _____ o dal tesserino personale rilasciato dall'Ufficio
 Comunale del Lavoro di _____ fa domanda per ottenere la assegnazione di una quo-
 di terreno ai sensi degli articoli 16 e 17 della Legge 12 maggio 1950, n. 230 e dell'art. 21 della Legge 21 ott
 bre 1950, n. 841.
 A tale scopo, presa conoscenza delle notizie contenute nella presente scheda, dichiara che esse corris-
 sono a verità per cui né assume piena responsabilità.
Montebelluno, il 6-5-1952

ATAQ. 10 DI TARANTO

FIRMA DEL RICHIEDENTE

ASSIGNATARIA

RISERVATA - 1 FEB 1951
 ALLA MM. SPCG. RIF. FOND.
 SERVIZIO SEGR. ED AFF. GEN.
 BARI
 184

Riferimento nota n. 3429/229/Begr.A.G. del
 17/1/1951 di pari oggetto, mi comunica che da accor-
 rentimenti esposti la MORELLI è risultata non possa-
 sedere alcun bene di fortuna all'industria i cui
 Si fa inoltre presente che la di lei fi-
 glia non compresa nello stato di famiglia passava
 a nozze a giorni.
 È risultato che la predetta famiglia
 politicamente è orientata per l'estrema sinistra.
 Si rendono l'ing. e via a fondo
 IL DIRETTORE DEL CENTRO
 (Dr. Carlo Caramico)
 184

Con le presenti congedo, desidero mandare alla Coop. di Taranto
 quale è desiderio di questo Centro di bisogni sommersi di L.
 richiesta del servizio
 di offrire delle assunzioni dei bisogni del mio paese.

Figure A.5: Posters for electoral propaganda by *Coldiretti* (1955-1962).



Notes: Posters for electoral propaganda by *Coldiretti*, supporting DC (1955-1962). The poster on the left states: *Farmer, agricultural worker, against communism, criminal killer of Hungarian farmers and workers, vote DC.* The poster on the right states: *Self-employed farmer, against Nenni and Togliatti [communist leaders] accomplices of the slaughterers of 10 million Russian farmers, vote DC.* On the bottom, the name of the association sponsoring the poster: *Confederazione Nazionale Coltivatori Diretti*, in short *Coldiretti*. Catalogo Generale dei Beni Culturali (<http://www.catalogo.beniculturali.it/>), Crea - Centro Grafico Pubblicitario, Treviso.

B Data Appendix

B.1 The Map of Italian Towns in 1951

We construct our data from a 1951 map of Italy. We create this map by combining two complete lists of towns, one from 1951 and one from 2001,⁴² with a shapefile of 2001 Italian towns⁴³. We use province and town name to match the two lists and construct the 1951 map taking into account merging and splitting events that happened between 1951 and 2001. We end up with a map and a dataset of 7792 towns. We drop 7 towns less than 50 km far from a reform border because they were merged into another town and it is not possible to reconstruct their borders in 1951.⁴⁴ We compute the distance between the town centroid and each reform area border and assign each town uniquely to its closest reform area.

We take into account splitting and merging events to add data from years after 1951. In the case of a town splitting after 1951, we aggregate the data for the towns that were a unique entity in 1951. When more towns merged after 1951, we assign weights based on population or area and we match the weighted measures to the relevant 1951 towns. This procedure causes variables for different years to have a different number of observations.

B.2 Variable Construction

Treatment

Treated town (actual reform). Treated towns lie inside reform areas as defined by the 1951 executive orders. In these towns, reform bodies had the power to expropriate and redistribute land. The list of treated towns is specified in the executive orders enacting the land reform (D.P.R. 66/1951, D.P.R. 67/1951, D.P.R. 68/1951, D.P.R. 69/1951, D.P.R. 70/1951, D.P.R. 264/1951, D.P.R. 265/1951).

Treated town (proposed reform). Treated towns in the proposed reform lie inside the “Zone B” as defined in Table 3 of 1950 *legge di riforma agraria*, which was proposed but never enacted (P.L.977/1950). The definition of “Zone B” formed the basis of the actual

⁴²We find these lists on <http://www.elesh.it>.

⁴³From ISTAT. ISTAT provides a shapefile for 1991 towns, but ELESH website does not have a 1991 list of towns.

⁴⁴Nicastro, Sambiase and Sant'Eufemia Lamezia were joined into Lamezia Terme; Carrara San Giorgio and Carrara Santo Stefano were joined into Due Carrare; Contarina and Donada were joined into Porto Viro. Other small holes in our map, inside the 50 km buffer, are caused by towns created from territories that in 1951 were part of several towns. For example: Semproniano was created in 1963 with territories taken from Manciano, Roccalbegna and Santa Fiora; Sellia Marina was created in 1957 with territories from Albi, Soveria Simeri, Sellia, Cropani and Magisano. These events are balanced at the reform border.

reform borders, although these were closer to the proposal in the North than in the South (see Section 2).

Distance to actual reform border. We define continuous reform borders by conflating all contiguous towns inside reform areas. We then use ArcGIS to compute the distance between the centroid of each town and the closest reform area border.

Distance to proposed reform border. We define continuous reform borders of the 1950 *legge di riforma agraria* by conflating all contiguous towns inside the “Zone B” (P.L.977/1950) We then use ArcGIS to compute the distance between the centroid of each town and the closest proposed reform area border.

Electoral outcomes

Town-level electoral results in 1919-24 and 1946-2001 come from Corbetta and Piretti (2009). We correct vote shares larger than 100% with data from the Ministry of the Interior. 1946 elections nominated members of the Constitutional Assembly. For the years 1919-24 1948-2001 we look at elections for the lower chamber of the Italian Parliament.

DC vote share: 1946-2001. Vote share is total DC votes divided the total votes cast. In the post-1992 elections we consider DC the following parties: *Italian Popular Party* and *Patto Segni* (1994); *Italian Popular Party*, *Lista Dini*, the Christian Democratic Center and the Christian Democratic Union (1996); *Margherita*, Christian Democratic Center and the Christian Democratic Union (2001).

log DC votes: 1946-2001. The variable is the natural logarithm of total DC votes. We made no adjustment for zeros as there were none.

PCI vote share: 1946-2001. Vote share is total PCI votes divided the total votes cast. We use the total votes for the Popular Democratic Front (FDP) in 1948 and the total votes for the Democratic Party of the Left (PDS) in 1992. In the post-1992 elections we consider PCI the following parties: Democratic Party of the Left (1992); Democratic Party of the Left and Communist Refoundation Party (1994-96); Democrats of the Left, Communist Refoundation Party and Communist Party (2001).

log PCI votes: 1946-2001. The variable is the natural logarithm of total PCI votes. We made no adjustment for zeros as there were none.

DC vote share: 1919-24. We take the *Italian Popular Party* (PPI) to be the Christian Democrats in 1919, 1921 and 1924. Vote share is total PPI votes divided the total votes cast.

PSI vote share: 1919-48. The Socialist Party ran under the name of Italian Socialist Party (PSI) in 1919, Official Socialist Party (PSU) in 1921 and United Socialist Party (PSU) in 1924. After the war, it ran as Italian Socialist Party (PSI) in 1946 and together with the Communist Party in the Popular Democratic Front (FDP) in 1948. Vote share is total votes for one of these parties divided the total votes cast.

PCI vote share: 1921-48. The Communist Party (PCI) was founded in 1921 and ran in both 1921 and 1924 elections. After the war, it ran as Italian Communist Party (PCI) in 1946 and together with the Socialist Party in the Popular Democratic Front (FDP) in 1948. Vote share is total votes for one of these parties divided the total votes cast.

Share “yes” in divorce referendum: 1974. Town-level returns from the 1974 divorce referendum is from Ministry of Interior (1977). Share of “yes” votes is total votes for repealing the divorce law divided total votes cast.

Forza Italia vote share: 1994-2001. Vote share is total *Forza Italia* votes divided total votes cast.

log of eligible voters: 1946-92. Between 1946 and 1972 all citizens above 21 were eligible to vote. In 1975 the age limit was reduced to 18. The variable is the natural logarithm of eligible voters.

Voter turnout: 1946-92. This variable is number of votes cast by number of eligible voters.

Mayor affiliation: 1946. We compile a new database with the affiliation of mayors at the time of the reform from historical newspapers published after the mayor elections in 1946. We use *L’Avvenire d’Italia*, *L’Unitá* and *La Voce Repubblicana*.

Land distribution

Share of expropriable estates (number): 1948. Expropriable estates data is from Medici (1948), Table 2. The table reports town-level number of estates in 1948, broken down by 11 separate categories of estate value. We use this information to construct the share of estates that the reform allowed to expropriate. We consider estates that could be expropriated as those with value in one of the top 4 categories of value. All estates in these categories were worth at least £20'000. Share of expropriable estates (number) is the number of expropriable estates divided the total number of estates.

Share of expropriable estates (value): 1948. Expropriable estates data is from Medici (1948), Table 2. The table reports town-level value of estates in 1948, broken down

by 11 separate categories of estate value. We use this information to construct the share of estates value that the reform allowed to expropriate. We consider estates that could be expropriated as those with value in one of the top 4 categories of value. All estates in these categories were worth at least £20'000. Share of expropriable estates (value) is the total value of expropriable estates divided by the total value of estates.

Share of owner-operated farms (number): 1961. Data on number of farms by type of operation is from ISTAT (1962a), Table 11. Share of owner-operated farms (number) is number of owner-operated farms divided the total number of farms.

Share owner-operated farms (land): 1961. Data on farm size by type of operation in 1961 is from ISTAT (1962a), Table 11. Share owner-operated farms (land) is total land of owner-operated farms divided total farmland.

Share of owner-operated farms (number): 1929. Data on number of farms by type of operation is from ISTAT (1936), Table III.I.4. Share of owner-operated farms (number) is number of owner-operated farms divided the total number of farms.

Share owner-operated farms (land): 1929. Data on farm size by type of operation in 1961 is from ISTAT (1936), Table III.I.4. Share owner-operated farms (land) is total land of owner-operated farms divided total farmland.

Casse Mutue

Casse Mutue elections. We digitize new data on board elections of *Casse Mutue*: farmers' local healthcare providers. From 1955, elections of *Casse Mutue*'s board of directors were generally held every three years (L.1136/1954). We digitize all available election results (1955-1970) from the fonds of the All. Naz. dei Contadini (1970) at the National Historical Archive of Italian Farmers' Movements conserved by the *Istituto Alcide Cervi* in Gattatico (RE). The 15 members of the board of directors of each *Cassa Mutua* were chosen among candidates in two slates using a plurality at-large electoral system. Using plurality rules, fifty percent of the voters can win all the seats, since the 15 candidates receiving the highest number of votes are elected and no quota is provided for the minority slate. We compute the per capita votes earned by the slate of candidates connected to *Coldiretti (Bonomiana)* and per capita number of casted votes.

Casse Mutue budgets. We digitize the 1965 budget of all municipal *Casse Mutue* from Fed. Naz. Casse Mutue (1966). We measure the relative size of *Casse Mutue* with total revenues and total expenditure per agricultural worker (from 1961 population census).

Other public policies

Transfers per capita: 1952, 1955, 1959. Municipal budgets are from ISTAT, 1962b. The variable is the inverse hyperbolic sine of per-capita transfers from the central government. We use 1951 total population.

Piano Casa dummy. Information on *Piano Casa* housing projects built between 1949 and 1955 is from Ministry of Labour's *Piano incremento occupazione operaia case per lavoratori*, (1959). The variable is an indicator for the presence of at least one project.

Piano Casa houses per 100'000 inhabitants . Information on *Piano Casa* housing projects built between 1949 and 1955 is from Ministry of Labour's *Piano incremento occupazione operaia case per lavoratori*, (1959). The variable is equal to the total number of apartment built divided by 100'000 inhabitants (from the 1951 census).

Cassa del Mezzogiorno. We delimit the area affected by the *Cassa del Mezzogiorno* as described in the law that created it in 1950 (L.646/1950). In the 25 km bandwidth from the Maremma reform border, only 8 control towns (2% of the control group) received *Cassa del Mezzogiorno* funds.

log Marshall Plan aid per capita. Information on US aid transferred through the Marshall Plan is from the “Mutual Security Agency” bulletins and was newly digitized by Bianchi and Giorcelli (2018): we thank them for sharing their data. The variable is equal to the natural logarithm of one plus the value of non-agricultural projects funded divided by 1951 population (from the census).

Endemic malaria 1934. We find towns where malaria was endemic in 1934 in a map prepared by Missiroli (1934). To digitize this data, we superimpose Missiroli's map to our map and code every town in the malaria areas as having malaria in 1934. We infer the intensity of the 1947-52 malaria eradication program from the presence of malaria in 1934.

Economic and demographic characteristics

Most economic and demographic characteristics come from the province records of decadal population (1936, 1951, 1961, 1971, 1981, 1991) and economic censuses (1951, 1961, 1971, 1981 , 1991). We digitized the earliest population and economic censuses (ISTAT, 1937, 1955, 1954). Additionally, we use Marbach and Ciapparelli (1983) to measure 1981 income.

log population: 1936-1991. Population data is from the following tables of the decadal population censuses: Table 4 (1951, 1961 and 1981); Table 3 (1971), Table 5.2 (1991). The

variable is the natural logarithm of total population. We made no adjustment for zeros as there were none.

Share of active population: 1936-1991. Active population data is from the following tables of the decadal population censuses: Table 6 (1951, 1961 and 1971), Table 7 (1981), Table 5.4 (1991). The variable is active population divided total working age population. In 1936 the working age is not specified. In 1951 and 1961 working age is 10 and in 1971 14. From 1981 on we observe population by detailed age group, and use 14 as the cutoff for working age population to allow comparison with 1971.

Share of workers in agriculture: 1936-1991. Sector of employment of workers is from the following tables of the decadal population censuses: Table 6 (1951 and 1961), Table 7 (1971), Table 8 (1981), Table 5.5 (1991). The variable is number of workers employed in agriculture divided total active population. In 1961 and 1971 forestry is included in agriculture.

Share of workers in manufacturing: 1936-1991. Sector of employment of workers is from the following tables of the decadal population censuses: Table 6 (1951 and 1961), Table 7 (1971), Table 8 (1981), Table 5.5 (1991). The variable is number of workers employed in manufacturing divided total active population. The manufacturing sector includes extractive and manufacturing industry. In 1981 manufacturing is the sum of economic sectors 2, 3 and 4 in Table 8.

Share of workers in public sector: 1936-1991. Sector of employment of workers is from the following tables of the decadal population censuses: Table 6 (1951 and 1961), Table 7 (1971), Table 8 (1981), Table 5.5 (1991). The variable is number of workers employed in manufacturing divided total active population. In 1981 public sector is economic sector 9.A.

Share of workers in GATT affected sectors: 1950. Sector of employment of workers is from Table 6 of the 1951 population Census. Sectors affected by the General Agreement of Tariffs and Trade are defined roughly as those sectors producing goods that are easier to trade: agriculture, manufacturing and transport. The variable is number of workers employed in these three sectors divided total active population.

Share of workers in ECSC affected sectors: 1951. Share of workers employed in firms affected by the Coal and Steel Community Agreement (1951). Firms affected by the agreement are in extraction (metallic and non-metallic minerals), metallurgy, mechanical engineering and manufacturing of non-metallic minerals: we source this information from

the 1951 economic census. We normalize by the total number of workers from the 1951 population census.

log number of firms in ECSC affected sectors: 1951. The variable is the natural logarithm of the number of firms affected by the Coal and Steel Community Agreement (1951). Firms affected by the agreement are in extraction (metallic and non-metallic minerals), metallurgy, mechanical engineering and manufacturing of non-metalling minerals: we source this information from the 1951 economic census. We adjust for zeros by adding one to the number of firms of every town.

Number of workers per plant. The number of workers per plant is the total number of workers employed in manufacturing, extraction, construction, commerce, trasport, utilities, social services, banking and insurance divided by the total number of plants in these sectors. Both number of plants and number of workers are from the decadal economic censuses. We exclude agriculture and health services as this information is not available in all economic censuses.

Number of plants per capita. The number of plants per capita is the total number of plants operating in manufacturing, extraction, construction, commerce, trasport, utilities, social services, banking and insurance divided by the town population in the same year. Number of plants comes from the decadal economic censuses and population from the decadal population censuses.

Change in log population: 1936-1951. The variable is the natural logarithm of population in 1951 minus the natural logarithm of population in 1936. We made no adjustment for zeros as there were none.

Change in log active population: 1936-1951. The variable is the natural logarithm of active population in 1951 minus the natural logarithm of active population in 1936. We made no adjustment for zeros as there were none.

Change in sectoral share (agriculture, manufacturing, public sector): 1936-1951. These variables are the difference between the share of active population in agriculture, manufacturing and public sector in 1951 and the share of the same sectors in 1936.

Share of males: 1951-1991. Population data is from the following tables of the decadal population censuses: Table 4 (1951, 1961 and 1981); Table 3 (1971), Table 5.2 (1991). The variable is number of males divided by total population.

Share of people in age groups (<21, 21-45; 46-65; >65): 1951-1991. Popu-

lation data is from the following tables of the decadal population censuses: Table 4 (1951, 1961 and 1981); Table 3 (1971), Table 5.2 (1991). The variable is population in specific age groups divided by total population.

Home ownership: 1951-1991. Home ownership data is from the following tables of the decadal population censuses: Table 9 (1951), Table 10 (1961), Table 17 (1971), Table 16 (1981), Table 5.18 (1991). The town-level is number of homes owned by their residents divided by total population.

Divorced couples per 10'000 married couples: 1981. Civil status of the population is from Table 3 of the 1981 census. The variable is number of divorced couples divided by the number of married couples, both measured in 1981. We multiply the variable times 10'000.

log income per capita: 1981. Town-level income is from Marbach and Ciapparelli (1983). The variable is the natural logarithm of 1981 income divided by 1981 total population. We made no adjustment for zeros as there were none.

Geographic characteristics

Provinces: 1951. Each town is assigned to its 1951 province.

Coordinates. Towns's latitude and longitude corresponds to the coordinates of their centroids in the 1951 map. They are measured in degrees in the WGS84 UTM32N coordinate system.

Distance to coast. We compute the distance to the coast of towns' 1951 centroid in ArcGIS.

Distance to Rome. We compute the distance between Rome's centroid and towns' 1951 centroid in ArcGIS.

Gothic line map. We draw the “Gothic line” using the map contained in (Oland, 1995).

Slope. Slope data is from the US Geological Survey database (USGS, 2005). The data is defined on 3-arc seconds grid covering the entire planet (approximately 462.5×462.5 meters). We join the raster to the map of 1951 Italian towns and assign to every town the average slope of all grid cells falling inside the town limits.

Elevation. Elevation data is from the US Geological Survey database (USGS, 2005). The data is defined on 3-arc seconds grid covering the entire planet (approximately $462.5 \times$

462.5 meters). We join the raster to the map of 1951 Italian towns and assign to every town the average elevation of all grid cells falling inside the town limits.

Potential yield: wheat. Potential yield data is from FAO-GAEZ (FAO, 2015). This data is defined on a 9.25×9.25 km grid covering the entire planet. We join the raster to the map of 1951 Italian towns and assign to every town the average potential yield of wheat with medium-level of inputs of all grid cells falling inside the town limits.

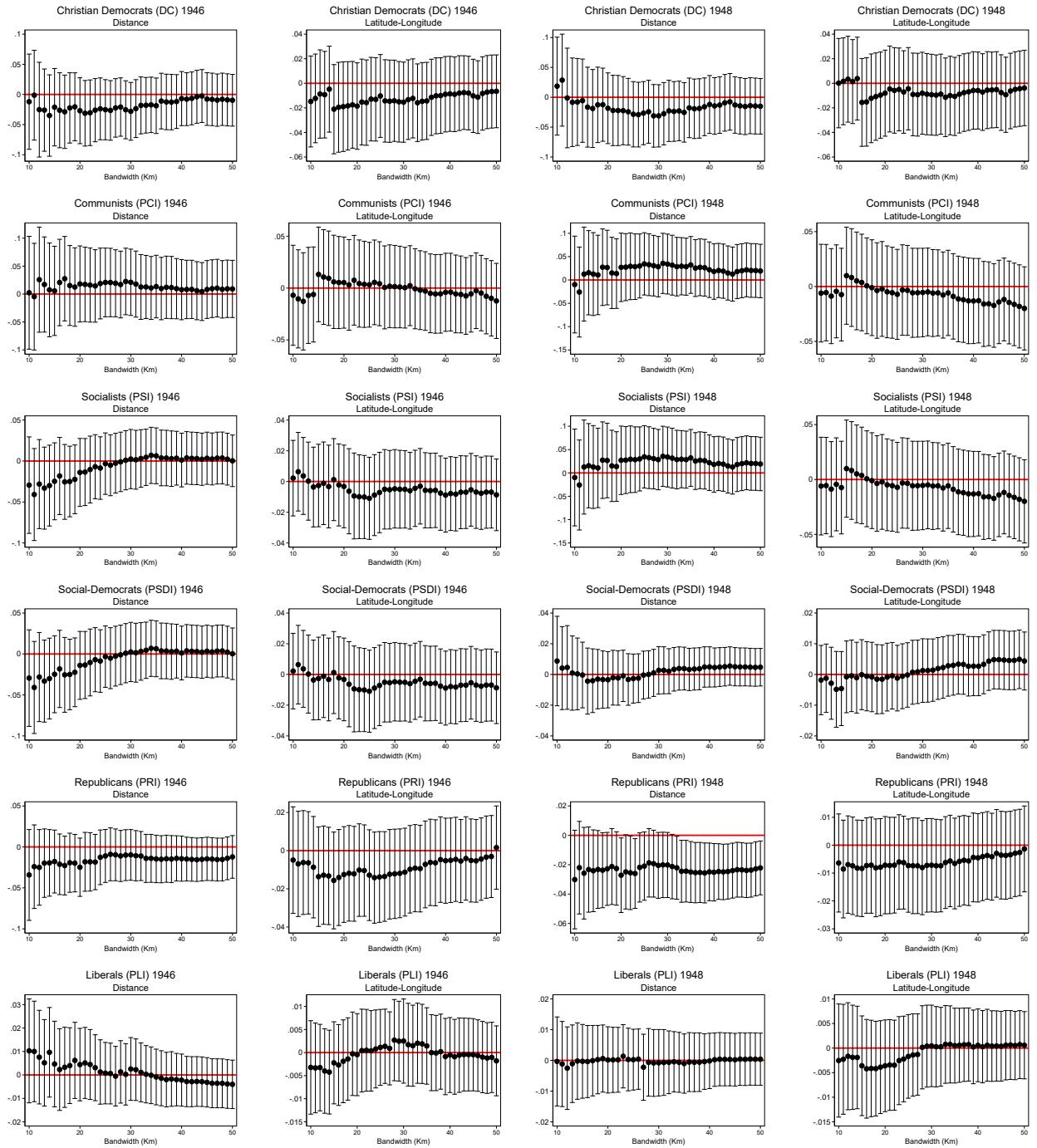
Potential yield: maize. Potential yield data is from FAO-GAEZ (FAO, 2015). This data is defined on a 9.25×9.25 km grid covering the entire planet. We join the raster to the map of 1951 Italian towns and assign to every town the average potential yield of maize with medium-level of inputs of all grid cells falling inside the town limits.

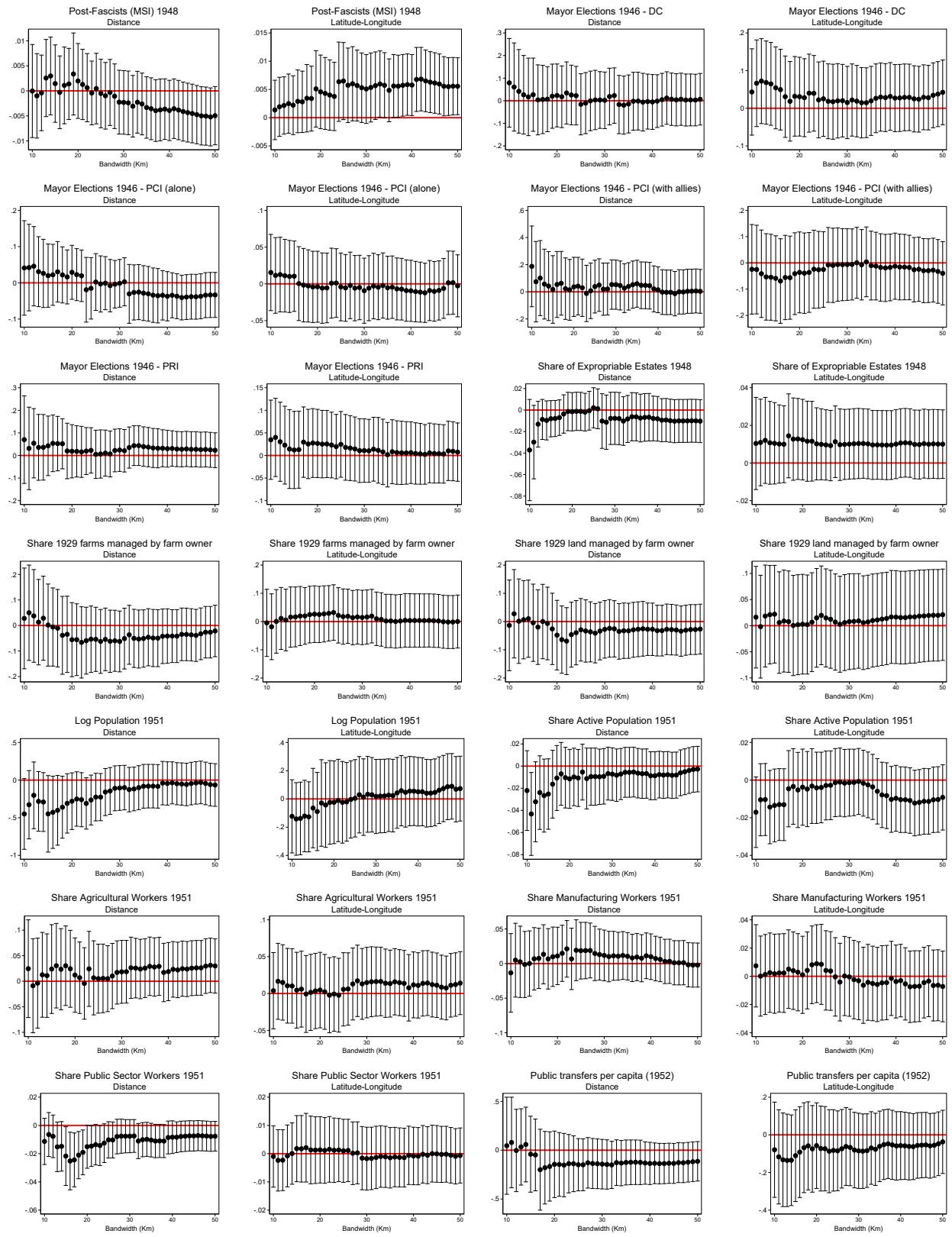
Share of border exposed to treatment. The variable is the length of the town limits that touch treated towns divided by the total length of these limits.

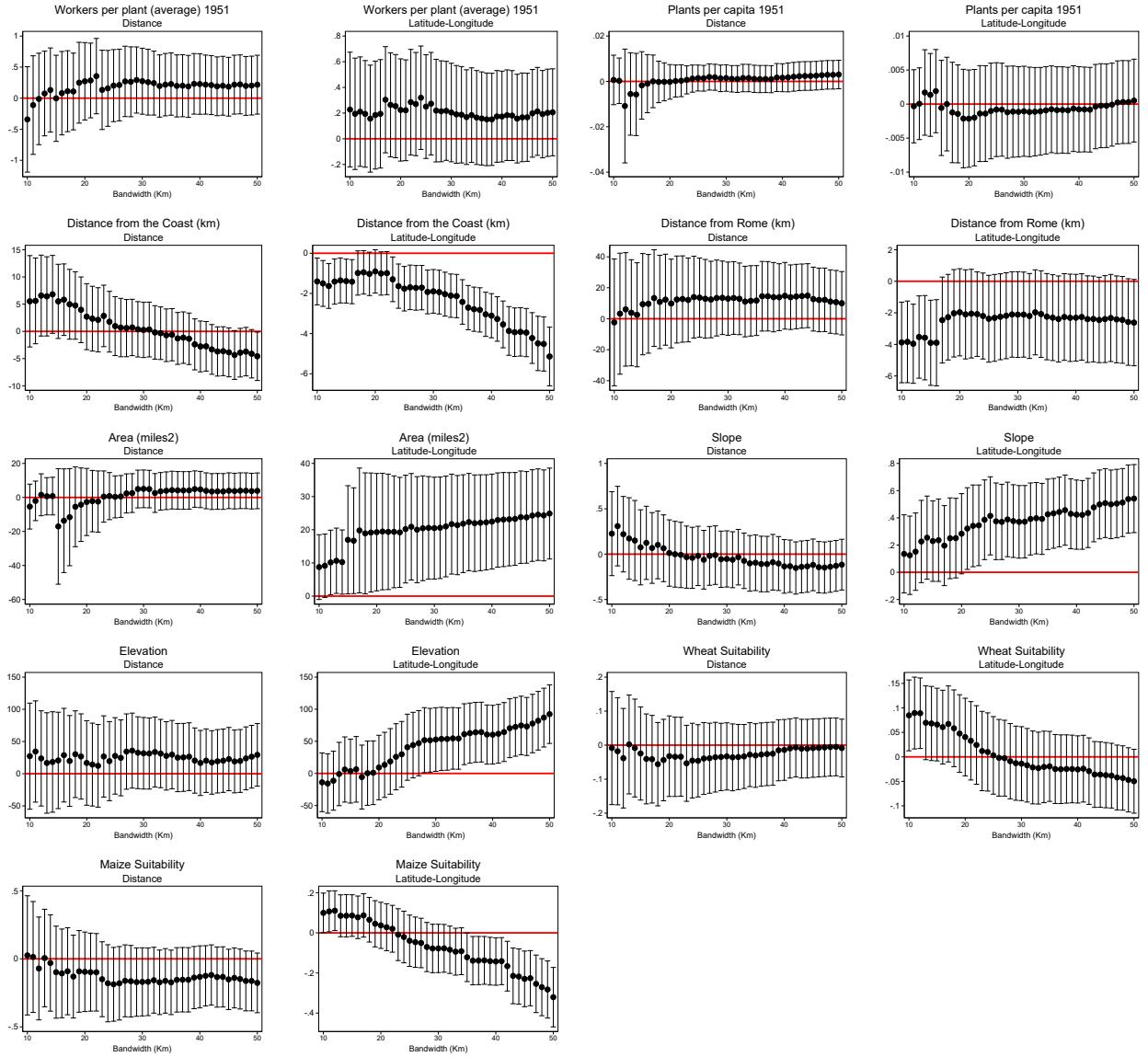
C Robustness and Alternative Specifications

C.1 Balance – Northern Italy

Figure C.1: Balance Northern Italy







Notes: The graphs report the coefficients of separate regressions of the RDD specification in Equation (2). We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. The subtitle *Distance* indicates the RDD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RDD regressions controlling for a quadratic polynomial in latitude and longitude. See Appendix B for a detailed description of each of these variables and their sources. Units of observation are towns. We estimate heteroskedasticity robust standard errors and plot 95% confidence intervals as bars around the coefficients.

C.2 Continuity of the Running Variable

To our knowledge, there is no standard McCrary test for two-dimensional RDD studies. This appendix discusses results from a McCrary test on the distance to the border (unidimensional RDD).

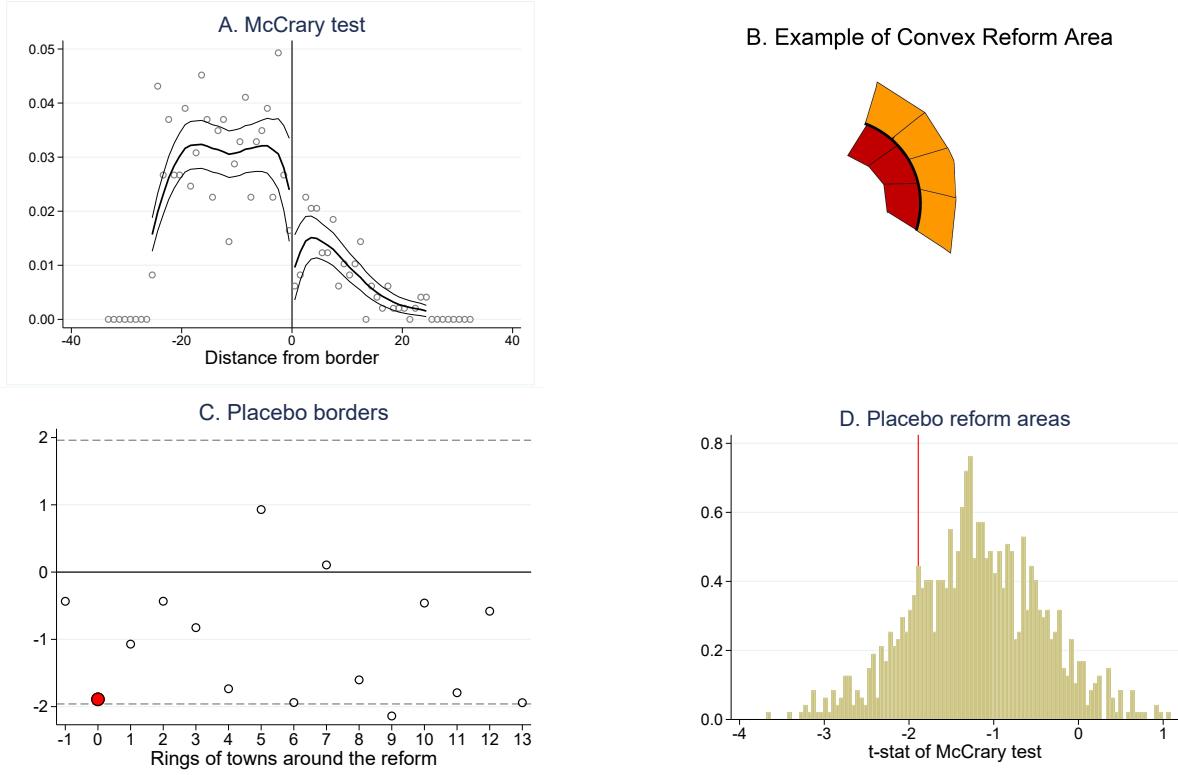
Appendix Figure C.2-Panel A presents the density approximation of the number of towns in the North (y-axis) as a function of the distance to the border (x-axis). The formal McCrary test has a t -statistic of -2.07 and rejects the null of no jump at the border. We believe that the jump in the density is a result of the geometry of the land reform. The reform areas are defined on clusters of towns that are on average convex sets (see Figure 1). In this case, for a given distance to the border, the area outside of the border will be greater than the area inside of it. Indeed, control towns within 25 km of the border occupy around 50% more area than treated towns within the same distance (roughly 12'000 vs 18'000 km²). Thus, it must be the case that there are more towns outside than inside the reform area (see Appendix Figure C.2-Panel B). We would then expect a greater number of control towns at every given absolute value of distance to the border, including values very close to the border (see Appendix Figure C.2-Panel A).

We validate this intuition with two separate exercises. In the first exercise, we move the border of Maremma inside and outside of the actual reform area by including progressively every “ring” of towns touching the previous border. For each of these fictitious borders “parallel” (so to speak) to the actual reform, we re-estimate the McCrary test. Appendix Figure C.2-Panel C reports the t -statistics of the McCrary test (y-axis) against the number of rings of towns we moved the reform border (x-axis). Most fictitious borders have McCrary estimates close to the McCrary t -statistics of Maremma towns (-1.89).

In the second exercise we use randomized inference and we simulate 999 separate fictitious reform areas on the true map of northern Italy. To build these fictitious reform areas, we follow rules that replicate the actual reform: we draw areas of (i) contiguous towns, (ii) located at least partially on the coast and (iii) that cover the same area of the actual reform. For each of these replications we estimate the McCrary test. We plot the distribution of the 999 t -statistics on Appendix Figure C.2-Panel D. The average t -statistic is -1.21 and our observed t -statistic (-1.89) lies at the 19th percentile of the distribution.

Taken together, these exercises suggest that the discontinuous drop in the number of towns at the border is not the result of manipulation, but a mechanical consequence of the geography of convex clusters of towns in Italy.

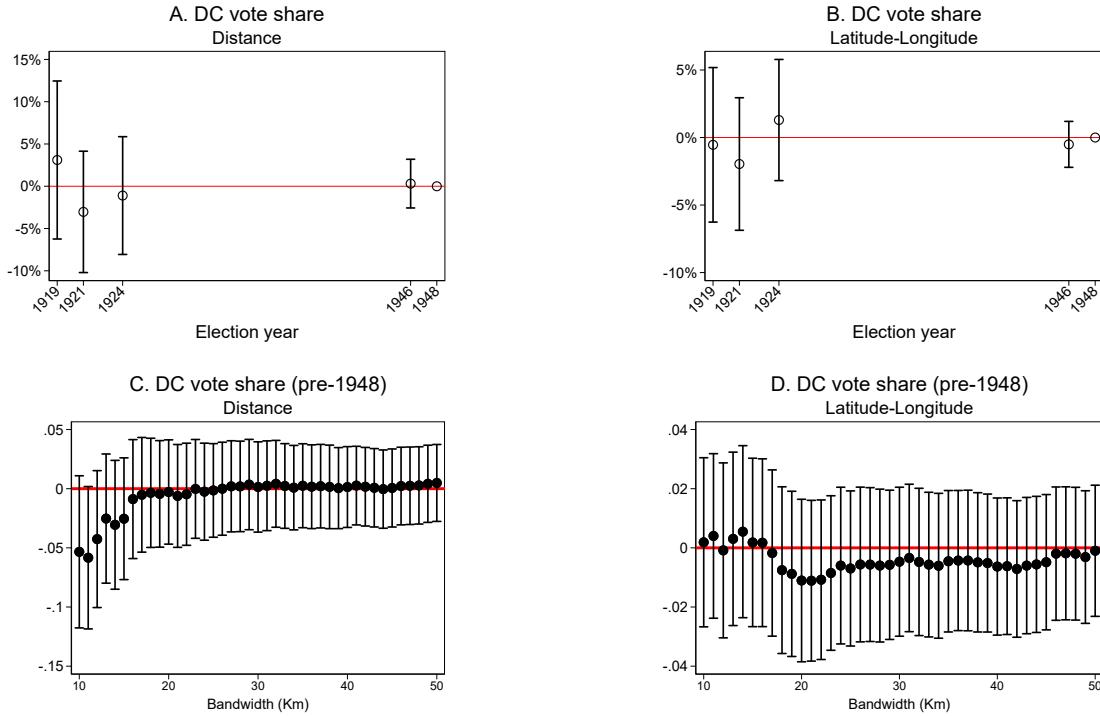
Figure C.2: McCrary test, Conjecture and Simulation Exercises



Notes: Panel A: density approximation of the number of towns within 25 km from the border of Delta Padano and Maremma. The approximation estimates separate densities on the two sides of the border and it is the basis of the test proposed by McCrary (2008). The t -statistic of the test is -2.07. Panel B: an example of convex reform area with towns of similar size on the two sides of the border. In this case the number of towns just outside of the border (orange) is greater than the number of towns just inside (red). Panel C and D: t -statistics of McCrary tests estimated on fictitious reform areas. Panel C: 14 fictitious reform areas; y-axis: t -statistics of the McCrary tests. The first area is created by removing from Maremma all treated towns lying on the reform border (point -1 on the x-axis). The other 13 areas are created by expanding Maremma so that it includes all towns lying on each successive reform border (points 1-13 on the x-axis). The t -statistic of the McCrary test of the true Maremma area is in red (point 0 on the x-axis). Panel D: 999 randomly generated fictitious reform areas. Each of these areas consist of contiguous towns with the same area as Maremma. We calculate the t -statistic of the McCrary test for each of them on the sample of towns that lie within 25 km from these fictitious borders. The Figure reports the distribution of these t -statistics. The red vertical line marks the t -statistic of the McCrary test of the true Maremma area.

C.3 1919-1948 Pre-Trends

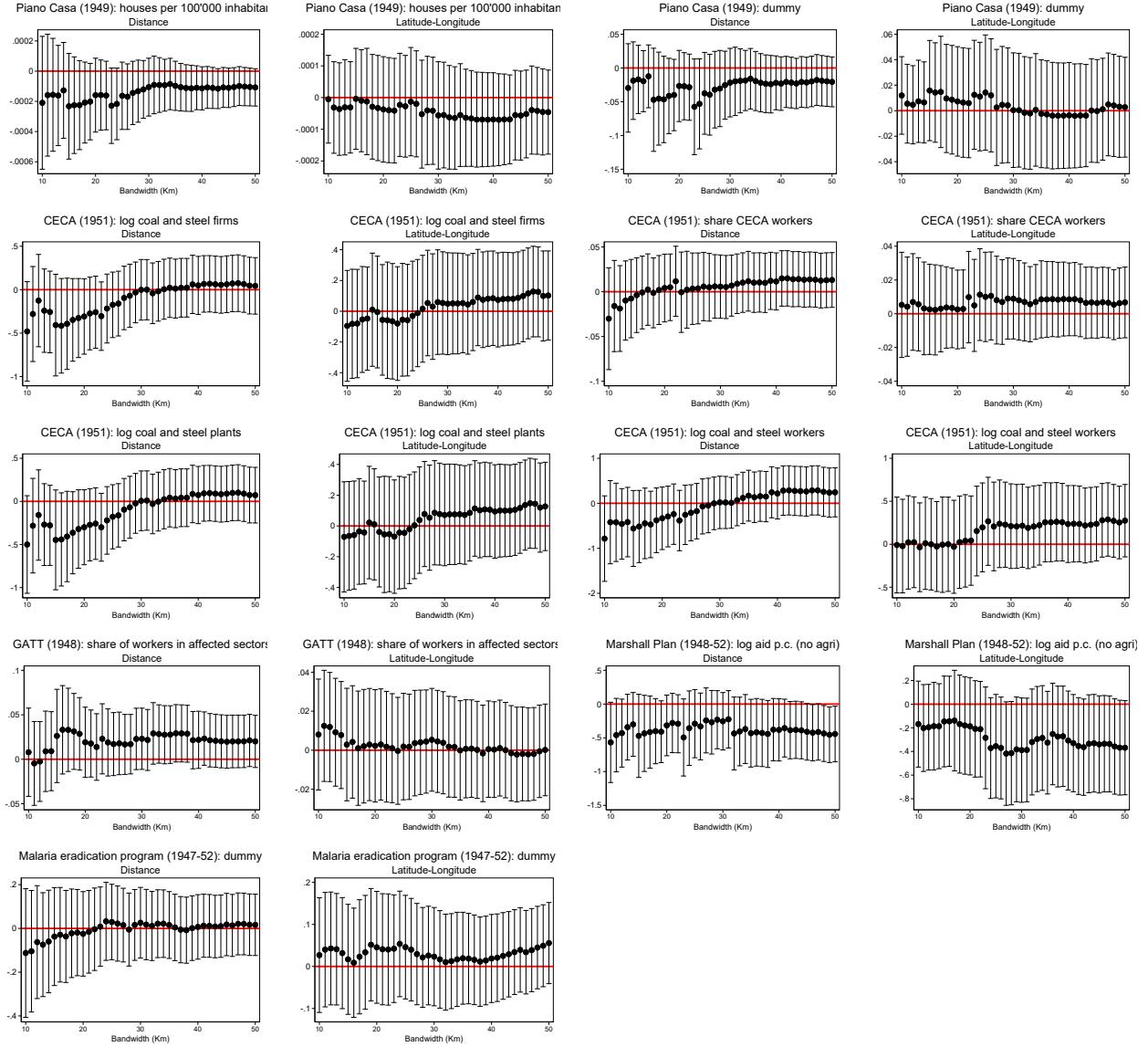
Figure C.3: Pre-Fascism Elections



Notes: Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients β_t for each year before the reform from the panel RDD Equation (1). The omitted category is the β of 1948 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. Panels C and D report pooled treatment effects for all electoral years between 1919 and 1946. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for year \times reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. We use the vote share of the Italian Popular Party (PPI) in the years 1919-24. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

C.4 Contemporaneous Policies

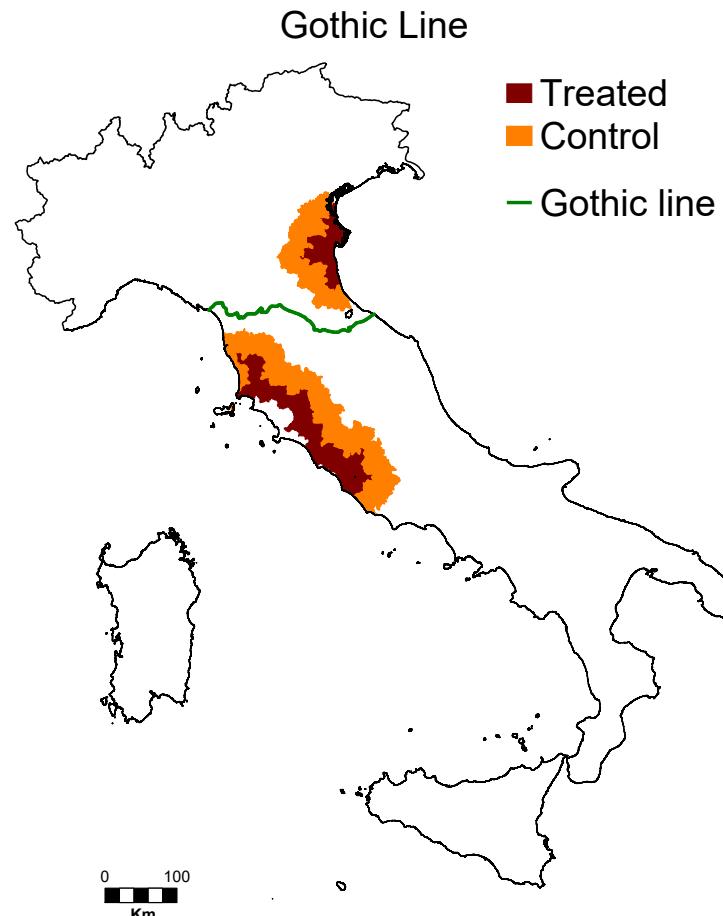
Figure C.4: Contemporaneous policies



Notes: The graphs report the coefficients of separate regressions of the RDD specification in Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. The dependent variables are measuring contemporaneous policies. The subtitle *Distance* indicates the RDD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RDD regressions controlling for a quadratic polynomial in latitude and longitude. See Appendix B for a detailed description of each of these variables and their sources. Units of observation are towns. The sample consists of all towns close to the reform borders of Delta Padano and Maremma.

C.5 Gothic Line

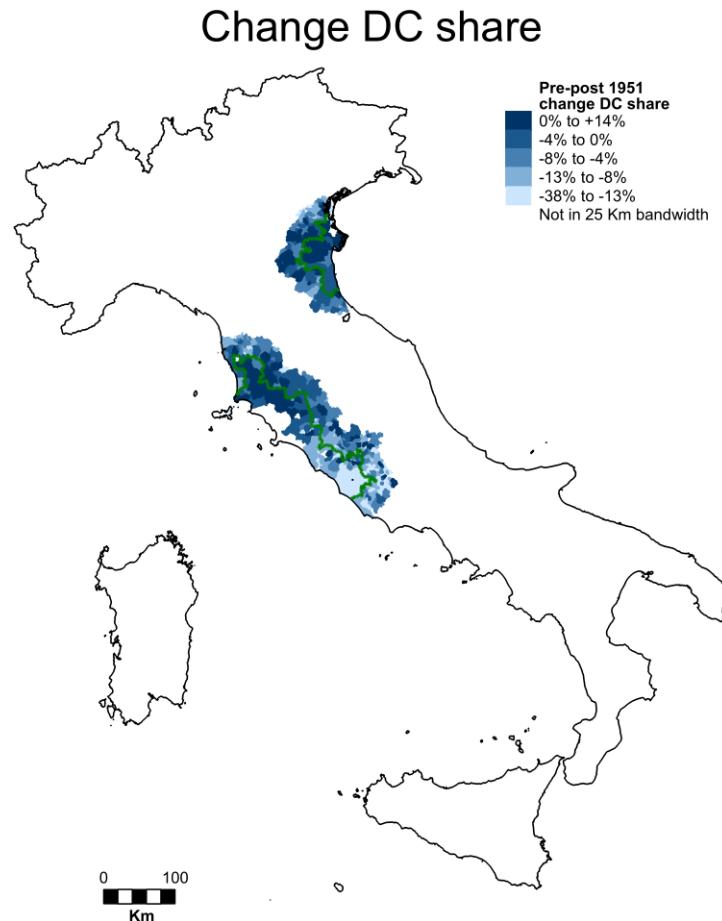
Figure C.5: Gothic Line



Notes: The map shows the position of the Gothic line (green) and the towns within 25 km from the reform borders of Delta Padano and Maremma (orange, outside; red, inside).

C.6 Electoral effect of the reform: visualization of impact on a map

Figure C.6: Electoral effect of the reform: visualization of impact on a map



Notes: Change in DC vote share between pre- (1946-48) and post-reform elections (1953-92) in the towns within 25 km from the reform borders of Delta Padano and Maremma.

C.7 Robustness to alternative specifications

Table C.1: Robustness to alternative specifications. Distance.

	Distance						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	No prov. seats	Distance ²	Reform area × distance	Elec. dist. FEs	Segment FEs	Prov. FE
A. DC vote share							
Treatment × 1950s	0.041*** [0.013]	0.041*** [0.013]	0.048** [0.024]	0.042*** [0.014]	0.038*** [0.013]	0.031** [0.013]	0.033** [0.015]
Treatment × 1960s	0.037** [0.016]	0.036** [0.016]	0.061** [0.027]	0.040** [0.016]	0.039** [0.015]	0.029* [0.016]	0.036** [0.016]
Treatment × 1970s	0.047*** [0.016]	0.047*** [0.016]	0.064** [0.025]	0.051*** [0.016]	0.052*** [0.016]	0.036** [0.016]	0.046*** [0.016]
Treatment × 1980s	0.048*** [0.018]	0.048*** [0.018]	0.047* [0.028]	0.052*** [0.018]	0.051*** [0.018]	0.036** [0.018]	0.045** [0.018]
B. Casse Mutue revenues p.c.							
Treatment	120.716** [55.278]	117.220** [55.307]	240.532*** [80.653]	112.402** [52.770]	147.549*** [56.198]	103.226* [55.713]	124.183** [55.303]
C. Casse Mutue expenses p.c.							
Treatment	119.382** [49.953]	116.118** [50.004]	232.458*** [73.101]	111.576** [49.140]	124.428** [49.475]	100.840** [49.800]	98.217** [48.821]
D. Casse Mutue votes p.c.							
Treatment	0.014** [0.006]	0.014** [0.006]	0.027** [0.011]	0.013** [0.006]	0.017*** [0.006]	0.016*** [0.006]	0.014** [0.006]
E. Coldiretti votes p.c.							
Treatment	0.011** [0.005]	0.011** [0.005]	0.022*** [0.008]	0.011** [0.005]	0.014*** [0.005]	0.012*** [0.005]	0.011** [0.005]
F. Public Sector Employment							
Treatment × 1936	0.005 [0.004]	0.005 [0.004]	0.014* [0.008]	0.005 [0.005]	0.006 [0.004]	0.002 [0.004]	0.003 [0.004]
Treatment × 1961	0.007* [0.004]	0.007* [0.004]	0.011** [0.006]	0.008** [0.004]	0.008** [0.004]	0.005 [0.003]	0.009** [0.004]
Treatment × 1971	0.005 [0.006]	0.004 [0.006]	0.020* [0.010]	0.005 [0.006]	0.008 [0.006]	0.005 [0.006]	0.010 [0.006]
Treatment × 1981	0.021*** [0.007]	0.021*** [0.007]	0.034*** [0.012]	0.022*** [0.008]	0.025*** [0.007]	0.018*** [0.007]	0.024*** [0.007]
Treatment × 1991	0.012 [0.010]	0.011 [0.010]	0.026* [0.015]	0.012 [0.010]	0.017** [0.008]	0.010 [0.008]	0.019** [0.008]

Notes: The Table reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area × year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area × year fixed effects. Panel A: The omitted category is the β of the elections of 1946 and 1948. Panel F: omitted category is the β of 1951. Col. 1: baseline specification controlling for a linear polynomial in distance. Col. 2: drop 10 provincial seats (including Rome). Col. 3: control for 2nd order polynomial in distance interacted with decades and reform area on both side of the border. Col. 4: controls for a linear polynomial in distance interacting it with reform area identifiers. Col. 5: controls for electoral districts × year fixed effects. Col. 6: control for ten segments × year fixed effects (see Figure C.7). Col. 7: control for province × year fixed effects. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns within 25 km of the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level (Panels A, D, E, F) or heteroscedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

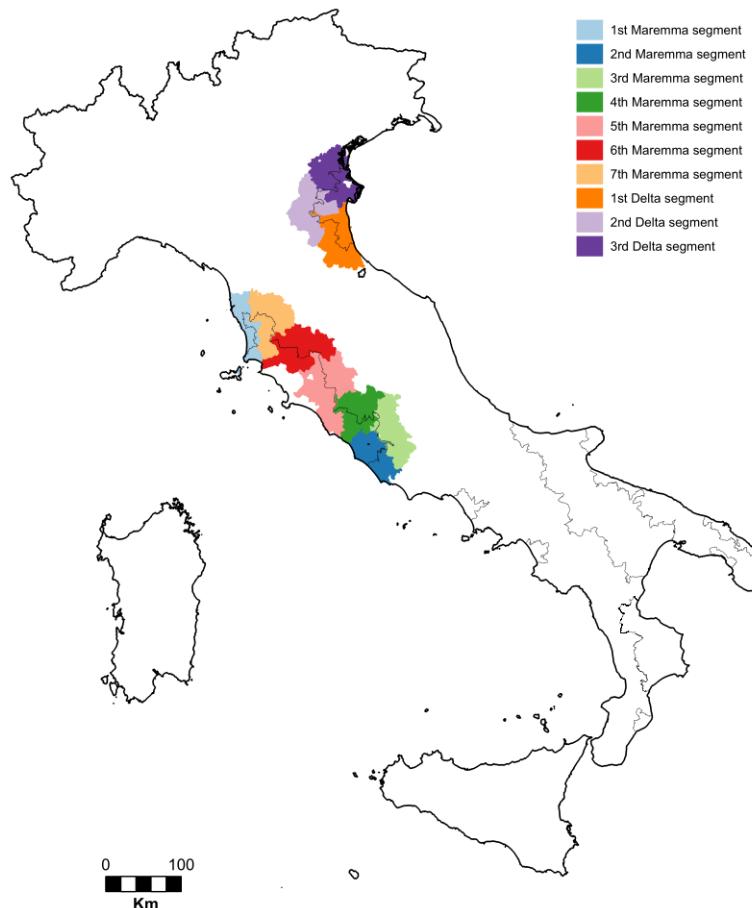
Table C.2: Robustness to alternative specifications. Latitude-Longitude.

	Latitude-Longitude						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	No prov. seats	Linear	No reform area interactions	Elec. dist. FEs	Segment FEs	Prov. FE
A. DC vote share							
Treatment × 1950s	0.017** [0.008]	0.018** [0.008]	0.029*** [0.007]	0.020*** [0.008]	0.018** [0.008]	0.020** [0.009]	0.033** [0.015]
Treatment × 1960s	0.025** [0.010]	0.026*** [0.010]	0.039*** [0.009]	0.030*** [0.009]	0.028*** [0.010]	0.029*** [0.010]	0.036** [0.016]
Treatment × 1970s	0.031*** [0.010]	0.032*** [0.011]	0.055*** [0.009]	0.041*** [0.010]	0.036*** [0.011]	0.029** [0.011]	0.046*** [0.016]
Treatment × 1980s	0.035*** [0.012]	0.035*** [0.012]	0.067*** [0.010]	0.048*** [0.011]	0.039*** [0.013]	0.037*** [0.013]	0.045** [0.018]
B. Casse Mutue revenues p.c.							
Treatment	118.237*** [38.874]	118.335*** [38.864]	159.361*** [31.844]	143.625*** [34.311]	109.049*** [38.243]	122.715*** [40.753]	106.066** [41.997]
C. Casse Mutue expenses p.c.							
Treatment	95.466*** [35.648]	93.760*** [35.830]	145.429*** [27.502]	146.915*** [29.892]	89.730** [36.416]	103.736*** [38.666]	95.372** [38.991]
D. Casse Mutue votes p.c.							
Treatment	0.007* [0.004]	0.007* [0.004]	0.014*** [0.003]	0.013*** [0.003]	0.007* [0.004]	0.007* [0.004]	0.009* [0.004]
E. Coldiretti votes p.c.							
Treatment	0.006** [0.003]	0.006* [0.003]	0.009*** [0.003]	0.010*** [0.003]	0.006** [0.003]	0.006** [0.003]	0.005 [0.003]
F. Public Sector Employment							
Treatment × 1936	0.004 [0.004]	0.004 [0.004]	0.004 [0.004]	0.005 [0.004]	0.003 [0.004]	0.002 [0.003]	0.005 [0.005]
Treatment × 1961	0.011*** [0.003]	0.011*** [0.003]	0.009*** [0.003]	0.012*** [0.003]	0.010*** [0.003]	0.007** [0.003]	0.010** [0.004]
Treatment × 1971	0.016*** [0.005]	0.016*** [0.005]	0.010** [0.004]	0.018*** [0.004]	0.015*** [0.005]	0.012** [0.005]	0.013** [0.006]
Treatment × 1981	0.026*** [0.005]	0.028*** [0.005]	0.023*** [0.005]	0.028*** [0.005]	0.024*** [0.005]	0.018*** [0.005]	0.023*** [0.007]
Treatment × 1991	0.020*** [0.006]	0.021*** [0.006]	0.019*** [0.006]	0.023*** [0.006]	0.020*** [0.006]	0.010 [0.007]	0.022** [0.009]

Notes: The Table reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area \times year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area \times year fixed effects. Panel A: The omitted category is the β of the elections of 1946 and 1948. Panel F: omitted category is the β of 1951. Col. 1: baseline specification controlling for a quadratic polynomial in latitude and longitude, interacted with reform area identifiers. Col. 2 drop 10 provincial seats (including Rome). Col. 3: control for 1st order polynomial in latitude and longitude interacted with decades and reform area on both side of the border. Col. 4: controls for a quadratic polynomial in latitude and longitude without interacting it with reform area identifiers. Col. 5: controls for electoral districts \times year fixed effects. Col. 6: control for ten segments \times year fixed effects (see Figure C.7). Col. 7: control for province \times year fixed effects. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns within 25 km of the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level (Panels A, D, E, F) or heteroschedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

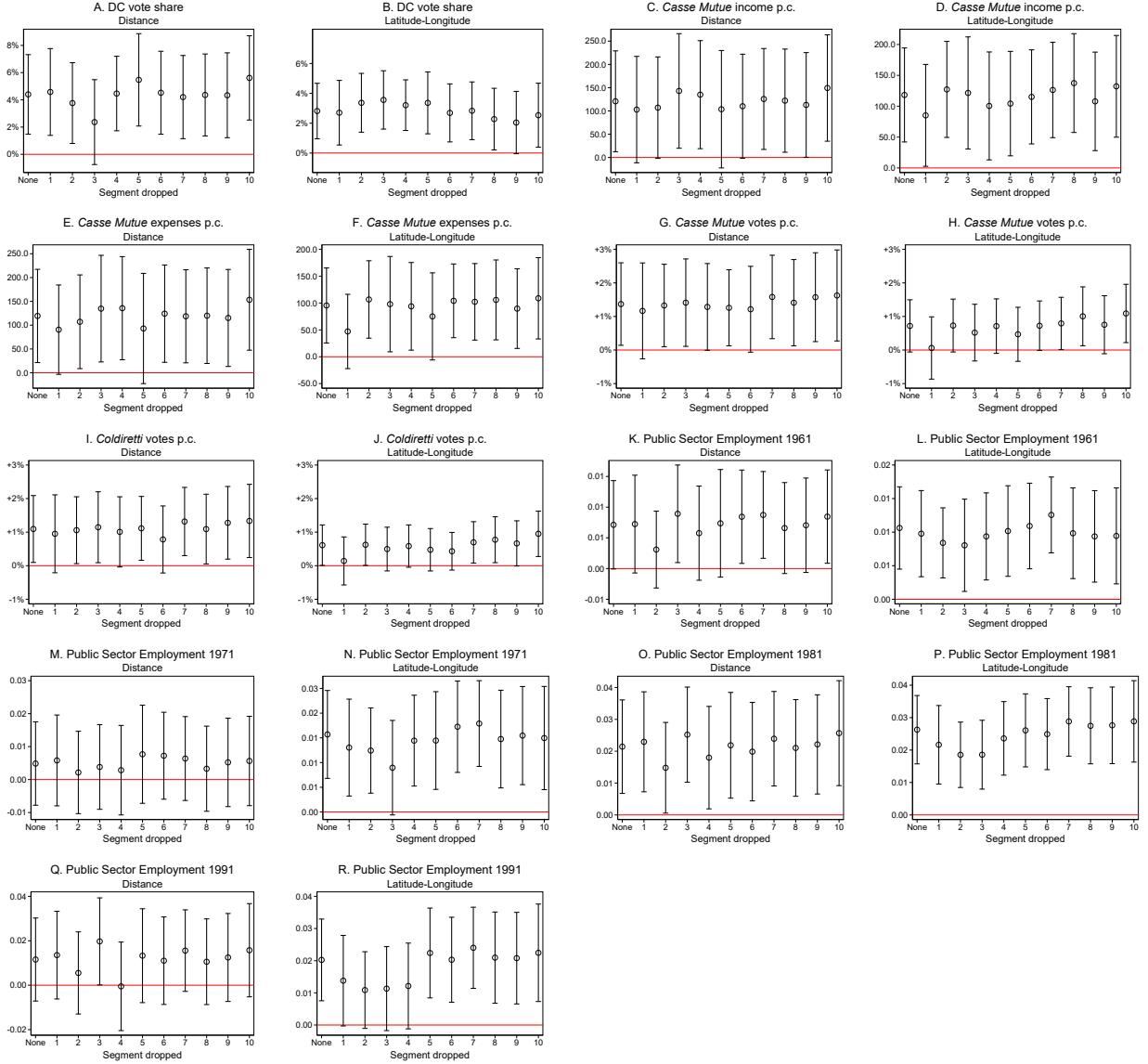
C.8 Robustness to Dropping Portions of the Reform Border

Figure C.7: Map: splitting the border in 10 segments



Notes: The Map shows how we split the borders of Delta Padano and Maremma into 10 segments of equal length. Each towns within 25 km of the border of Delta Padano and Maremma is assigned to the closest segment. We report estimates of Equation (1) when we drop each of these segments in Figure C.8.

Figure C.8: Treatment coefficients when dropping portions of the sample.



Notes: The Figure reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, B, and K to R) and (2) (Panels C to J). Panels A, B, and K to R: regressions control for reform area \times year and town fixed effects. Panel C to F: regressions control for reform area fixed effects. Panels G to J: regressions control for reform area \times year fixed effects. Panels A, B, and K to R: we estimate a single coefficient for treated towns in the post-reform years (1953-92). The first estimate (point "None" on the x-axis) corresponds to our baseline coefficient. We obtain the other coefficients after dropping all towns closest to one of the 10 segments marked on Map C.7. Units of observation are town-years (Panels A, B, and G to R) or towns (Panels C to F). The sample consists of all towns within 25 km of the reform borders of Delta Padano and Maremma. The subtitle *Distance* indicates all regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates all regressions controlling for a quadratic polynomial in latitude and longitude. We estimate standard errors clustered at the town level (Panels A, B, and G to R) or heteroskedasticity robust (Panels C to F) and plot 95% confidence intervals as bars around the coefficients.

C.9 Instrumental Variable

C.9.1 Reduced Form

Table C.3: RF: the impact of the land reform.

	Distance			Latitude-Longitude		
	<i>Preferred Bandwidth</i>	<i>Alternative Bandwidths</i>		<i>Preferred Bandwidth</i>	<i>Alternative Bandwidths</i>	
		< 25km	< 10km		< 25km	< 50km
	(1)	(2)	(3)	(4)	(5)	(6)
A. DC vote share						
Treatment × 1950s	0.045*** [0.010]	0.045*** [0.015]	0.040*** [0.009]	0.023*** [0.007]	0.027*** [0.009]	0.022*** [0.006]
Treatment × 1960s	0.043*** [0.012]	0.051*** [0.016]	0.040*** [0.011]	0.034*** [0.008]	0.034*** [0.011]	0.037*** [0.008]
Treatment × 1970s	0.054*** [0.013]	0.058*** [0.016]	0.047*** [0.011]	0.043*** [0.009]	0.045*** [0.011]	0.044*** [0.008]
Treatment × 1980s	0.054*** [0.014]	0.042** [0.018]	0.056*** [0.013]	0.044*** [0.010]	0.045*** [0.013]	0.041*** [0.009]
B. Casse Mutue revenues p.c.						
Treatment	66.648 [43.492]	86.063 [59.531]	53.626 [41.008]	110.591*** [33.277]	134.328*** [40.095]	118.182*** [33.313]
C. Casse Mutue expenses p.c.						
Treatment	101.647*** [37.621]	124.556** [51.853]	92.771** [36.123]	98.394*** [29.612]	117.636*** [36.976]	124.473*** [29.291]
D. Casse Mutue votes p.c.						
Treatment	0.009** [0.005]	0.007 [0.006]	0.015*** [0.004]	0.010*** [0.003]	0.016*** [0.004]	0.010*** [0.003]
E. Coldiretti votes p.c.						
Treatment	0.008** [0.004]	0.009* [0.005]	0.012*** [0.003]	0.009*** [0.002]	0.013*** [0.003]	0.010*** [0.002]
F. Public Sector Employment						
Treatment × 1936	0.008** [0.004]	0.006 [0.005]	0.006** [0.003]	0.007* [0.004]	0.006* [0.003]	0.008*** [0.003]
Treatment × 1961	0.006* [0.003]	0.004 [0.004]	0.009*** [0.002]	0.010*** [0.002]	0.006** [0.003]	0.008*** [0.002]
Treatment × 1971	-0.001 [0.005]	-0.001 [0.007]	0.006 [0.004]	0.016*** [0.004]	0.007* [0.004]	0.013*** [0.003]
Treatment × 1981	0.013** [0.007]	0.015* [0.009]	0.020*** [0.005]	0.025*** [0.004]	0.022*** [0.005]	0.021*** [0.004]
Treatment × 1991	0.000 [0.009]	0.003 [0.010]	0.012 [0.007]	0.021*** [0.005]	0.015** [0.006]	0.018*** [0.005]

Notes: The Table reports reduced form coefficients β_t and β from alternative specifications of Equations (1) (Panels A, D, E, F) and (2) (Panels B, C). The independent variables are treatment and distance to the borders of “Zone B” in the *legge di riforma agraria* which was proposed and never enacted, but formed the basis of the actual reform. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns close to the *proposed* reform borders of Delta Padano and Maremma. Estimates reported for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Columns 1 to 3 use a linear specification in distance, columns 4 to 6 use a quadratic specification in latitude and longitude. Standard errors clustered at the town level (Panels A, D, E, F) or heteroscedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

C.9.2 First Stage and IV

Table C.4: IV: the impact of the land reform.

	Distance			Latitude-Longitude		
	<i>Preferred Bandwidth</i>	<i>Alternative Bandwidths</i>		<i>Preferred Bandwidth</i>	<i>Alternative Bandwidths</i>	
		< 25km	< 10km		< 25km	< 50km
	(1)	(2)	(3)	(4)	(5)	(6)
First stage (cross-section)						
Treatment	0.611*** [0.063]	0.631*** [0.086]	0.641*** [0.057]	0.689*** [0.042]	0.706*** [0.047]	0.728*** [0.039]
F-test	438.40	95.04	447.35	267.77	229.89	348.64
Observations	488	222	855	490	222	863
A. DC vote share						
Treatment × 1950s	0.073*** [0.019]	0.075** [0.032]	0.063*** [0.015]	0.022* [0.012]	0.039*** [0.013]	0.024** [0.010]
Treatment × 1960s	0.065*** [0.022]	0.075** [0.037]	0.063*** [0.017]	0.034** [0.014]	0.049*** [0.016]	0.042*** [0.012]
Treatment × 1970s	0.081*** [0.023]	0.081** [0.036]	0.075*** [0.019]	0.043*** [0.015]	0.063*** [0.017]	0.055*** [0.012]
Treatment × 1980s	0.084*** [0.025]	0.054 [0.039]	0.088*** [0.021]	0.036** [0.017]	0.061*** [0.021]	0.052*** [0.014]
B. Casse Mutue revenues p.c.						
Treatment	85.006 [72.754]	114.222 [130.800]	97.562 [61.697]	190.130 [4417.984]	165.419*** [49.844]	162.370*** [44.973]
C. Casse Mutue expenses p.c.						
Treatment	130.516** [64.440]	175.062 [118.754]	147.807*** [55.802]	186.118 [3702.587]	156.252*** [47.575]	171.013*** [40.006]
D. Casse Mutue votes p.c.						
Treatment	0.020** [0.010]	0.022 [0.020]	0.033*** [0.009]	0.020 [0.492]	0.019*** [0.006]	0.017*** [0.005]
E. Coldiretti votes p.c.						
Treatment	0.017** [0.008]	0.024* [0.014]	0.028*** [0.007]	0.017 [0.360]	0.017*** [0.005]	0.015*** [0.004]
F. Public Sector Employment						
Treatment × 1936	0.011* [0.007]	-0.002 [0.013]	0.009* [0.005]	0.010 [0.151]	0.007 [0.005]	0.008* [0.004]
Treatment × 1961	0.007 [0.005]	-0.003 [0.009]	0.014*** [0.004]	0.015 [0.268]	0.009** [0.004]	0.010*** [0.003]
Treatment × 1971	-0.004 [0.009]	-0.019 [0.016]	0.007 [0.007]	0.022 [0.280]	0.010* [0.006]	0.013*** [0.005]
Treatment × 1981	0.021* [0.011]	0.014 [0.019]	0.030*** [0.008]	0.036 [0.327]	0.031*** [0.007]	0.025*** [0.006]
Treatment × 1991	-0.003 [0.015]	-0.010 [0.022]	0.014 [0.011]	0.029 [0.152]	0.021** [0.009]	0.016** [0.007]

Notes: The first part of the table reports RDD coefficients of a first stage using Equation (2). The second part of the Table reports IV coefficients β_t and β from alternative specifications of Equations (1) (Panels A, D, E, F) and (2) (Panels B, C). The instruments of treatment and distance from the actual reform border are treatment and distance to the borders of “Zone B” in the *legge di riforma agraria* which was proposed and never enacted, but formed the basis of the actual reform. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns close to the *proposed* reform borders of Delta Padano and Maremma. Estimates reported for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Columns 1 to 3 use a linear specification in distance, columns 4 to 6 use a quadratic specification in latitude and longitude. Standard errors clustered at the town level (Panels A, D, E, F) or heteroscedastic robust (Panels B, C) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.10 SUTVA

Higher vote shares for DC in the reform areas may indicate stronger support among the voters of treated towns: this is our preferred interpretation. However, the reform may also create resentment in control towns, thus causing a reduction in DC support there. This would violate SUTVA and threaten our identification.⁴⁵

Land invasions after the reform. In the years following the reform, groups of rural workers invaded land across Italy to voice discontent and sometimes in the hope to obtain land. We digitize new data on land invasions (Ministry of Interior, 1952) in the two years following the reform and ask whether discontent was greater in control towns. Results are in Columns 1 of Table C.5: the coefficient is insignificant, and if anything suggests that land invasions were slightly more common in treated towns. Because land invasions are a form of resentment, these results are not consistent with greater grievances in control towns.

Heterogeneity by share of agricultural workers. Second, if resentment were a factor, the reduction in support for the Christian Democrats would likely be higher in control towns with a higher share of agricultural workers, as these workers would have higher benefits from the reform. To test this hypothesis, we study heterogeneity by the pre-reform share of agricultural workers in a difference-in-difference specification.⁴⁶

$$y_{irt} = \eta_i + \eta_{rt} + \beta \cdot Post_t \times T_i + \\ + \gamma \cdot Post_t \times \left(\frac{L_a}{L} \right)_i + \delta \cdot Post_t \times \left(\frac{L_a}{L} \right)_i \times T_i + u_{irt} \quad (\text{C.1})$$

In Equation (C.1), $Post_t$ is a dummy equal to 1 in every election after 1950 and $\left(\frac{L_a}{L} \right)_i$ is the share of workers employed in agriculture in 1951.⁴⁷ If resented potential beneficiaries in control towns punished DC, we would expect γ to be negative.

In Table C.5 we compare pre-reform elections (1946, 48) with the first two elections after the reform (1953, 58). Column 2 shows that, in control towns, places with high and low share of agricultural workers showed similar support for DC after the reform (if anything, support

⁴⁵The reform would also affect outcomes in control towns mechanically if some of the beneficiaries of the land reform came from control areas. This type of spillover is not a concern because *in practice* almost all beneficiaries were resident of treated towns (Dickinson, 1954, Rossi-Doria, 1958, Marciani, 1966).

⁴⁶We include towns in a 25 km bandwidth. We performed the same exercise with a more complex panel RDD specification, which requires more power than the difference-in-differences. However, the specification controlling for latitude and longitude suffers from overfitting. Results for the RDD controlling for a linear polynomial in distance are similar and available upon request.

⁴⁷We consider 1951 as a pre-treatment year. This is reasonable because very little land was expropriated (and virtually none redistributed) before the census was completed. Results using the share of workers employed in agriculture in 1936 are similar, though somewhat less precise.

for DC is higher in towns with more agricultural workers: $\gamma=0.021$, s.e.=0.016).⁴⁸

Heterogeneity by exposure to the reform. Next, we explore a different source of heterogeneity. If resentment were driving our results, we would expect a higher reduction in DC vote share in those control towns where the reform is very visible. We proxy visibility with the *portion* of the town border that overlaps with the reform area. For this exercise, we restrict the sample to those towns with at least a portion of their town limit *on the border* of the reform areas. Table C.5-Column 3 report the estimates of modified versions of Equation (C.1), where we interact $Post_t$ and T_i with the share of the town limits located on the reform border (B_i^T/B_i). The positive and insignificant coefficient on the interaction between $Post_t$ and B_i^T/B_i suggests that control towns where the reform was more visible did not vote against DC after the reform. Overall, these regressions suggest again that resentment in control towns is unlikely to drive our main results.

Donut Panel RDD. In the last exercise, we estimate Equation (1), but drop towns close to the reform border.⁴⁹ Similar to the previous exercise, if voters in control towns resented the reform and punished DC after 1950, we expect this effect to be larger close to the border, where voters were likely to be more aware of the reform. If this were true, dropping towns close to the border should shrink the estimated effect of the reform, because it would remove those towns where punishment against DC was stronger. In contrast, if the coefficient remains stable after dropping towns close to the border, it would be evidence that this mechanism is not important. Table C.6-Columns 1 and 5 report our baseline results: this is the increase in the support for DC after 1950 in treated towns within 25 km from the border. In Columns 2 through 4 and 6 through 8 of Table C.6, we estimate the same regressions after dropping towns that are within 1.5, 2.5 and 5 km from the border. Across these samples, the point estimate remains generally stable. The only exception are the coefficients for the 50s in the specifications controlling for a quadratic polynomial in latitude and longitude, where the coefficients are positive, but not significant. These results suggest again that resentment against DC in control towns closely exposed to the reform does not drive our results.

⁴⁸In addition, in treatment areas, towns with a strong presence of agricultural workers experience a differential increase in the electoral support for DC, though this is not significant ($\delta=0.051$, s.e.=0.036).

⁴⁹This exercise is reminiscent of the “donut-RD” proposed by Barreca et al. (2016) to address problems of bunching in RDDs. We use it instead to provide additional evidence supporting SUTVA.

Table C.5: Tests of resentment.

	Land Invasions	Christian Democrats	
	(1)	(2)	(3)
Treatment	0.097 [0.083]		
Treatment \times Post		-0.004 [0.021]	0.030* [0.015]
Share agricultural workers \times Post		0.021 [0.016]	
Share agricultural workers \times Treatment \times Post		0.051 [0.036]	
Share of town limit on reform border \times Post			0.013 [0.026]
Share of town limit on reform border \times Treatment \times Post			-0.017 [0.050]
Mean Y Control	0.06	0.36	0.31
Number of Towns	490	482	155
Observations	490	1925	620

Notes: The Table reports coefficients from Equation (2) (col. 1) and (C.1) (cols. 2 and 3). Col. 1 controls for reform area fixed effects; cols. 2 and 3 control for year \times reform area and town fixed effects. *Post* = 1 for elections after the land reform (1953-58). There are 2 elections before the reform: 1946 and 1948. Column 2: difference-in-differences with heterogeneity in share of agricultural workers. Column 3: difference-in-differences with heterogeneity in share of town limits touching the reform border. Column 1: dependent variable is land invasions in 1951-52. Columns 2 and 3: dependent variable is Christian Democrat (DC) vote share. Share of agricultural workers is from the population census. Units of observation are town-years (cols. 2 and 3) and towns (col. 1). Columns 1 and 2: the sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. Column 3: the sample consists of all towns touching the border of either Maremma or Delta Padano. Standard errors clustered at the town level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table C.6: Donut Panel RDD. Effect on Christian Democrats vote share.

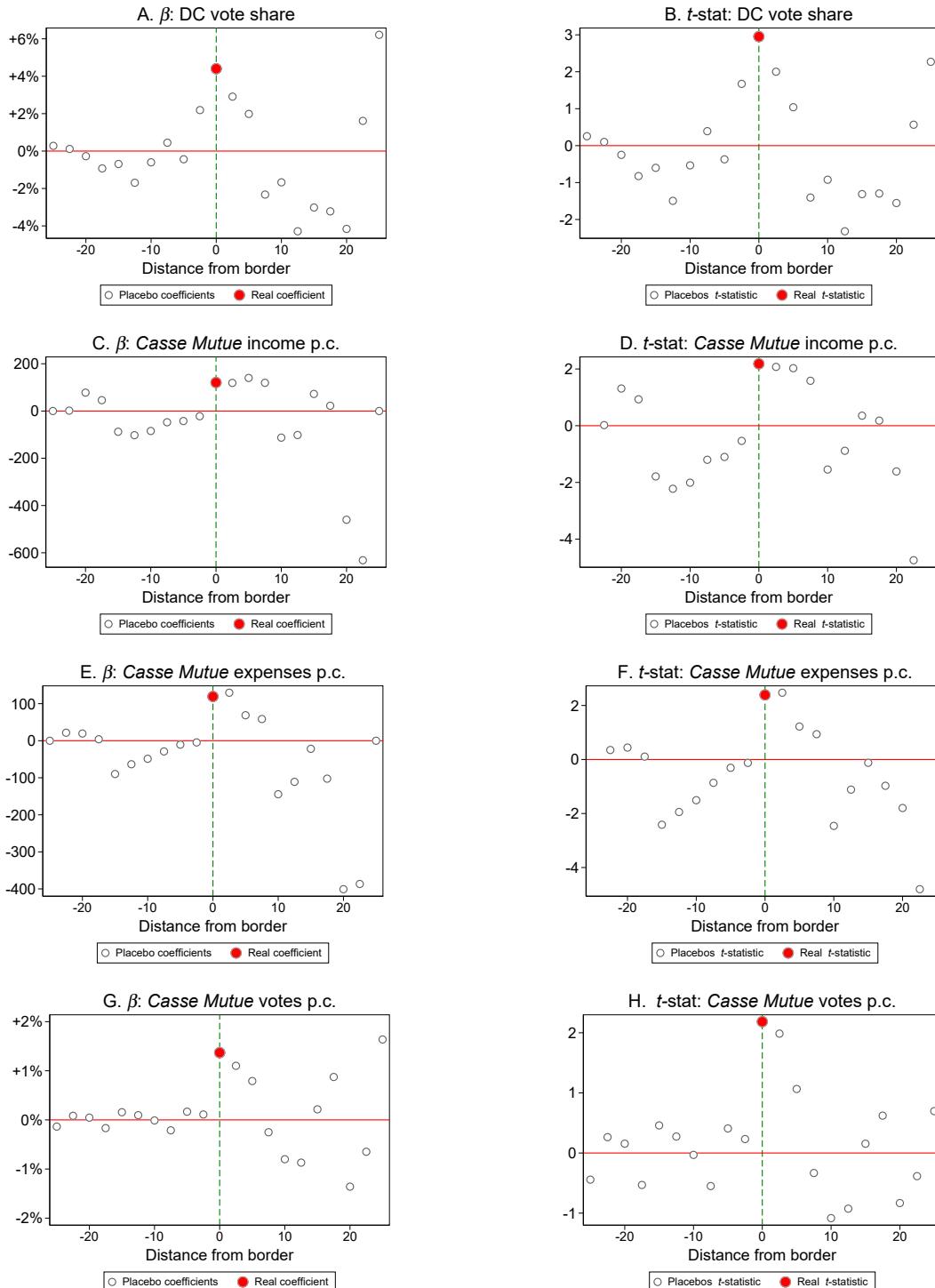
	Distance				Latitude-Longitude			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Donut: 1.5 km	Donut: 2 km	Donut: 2.5 km	All	Donut: 1.5 km	Donut: 2 km	Donut: 2.5 km
Treatment × 1950s	0.041*** [0.013]	0.033*** [0.012]	0.031** [0.012]	0.038*** [0.013]	0.017** [0.008]	0.011 [0.008]	0.010 [0.008]	0.012 [0.008]
Treatment × 1960s	0.037** [0.016]	0.032** [0.015]	0.029** [0.015]	0.034** [0.016]	0.025** [0.010]	0.020** [0.009]	0.021** [0.009]	0.023** [0.010]
Treatment × 1970s	0.047*** [0.016]	0.046*** [0.017]	0.043** [0.017]	0.042** [0.018]	0.031*** [0.010]	0.029*** [0.011]	0.030*** [0.011]	0.028** [0.011]
Treatment × 1980s	0.048*** [0.018]	0.052*** [0.018]	0.050*** [0.019]	0.053*** [0.020]	0.035*** [0.012]	0.035*** [0.012]	0.037*** [0.013]	0.037*** [0.013]
Mean Y Control Group	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.36
Number of Towns	490	471	461	444	490	471	461	444
Observations	5838	5615	5495	5291	5838	5615	5495	5291

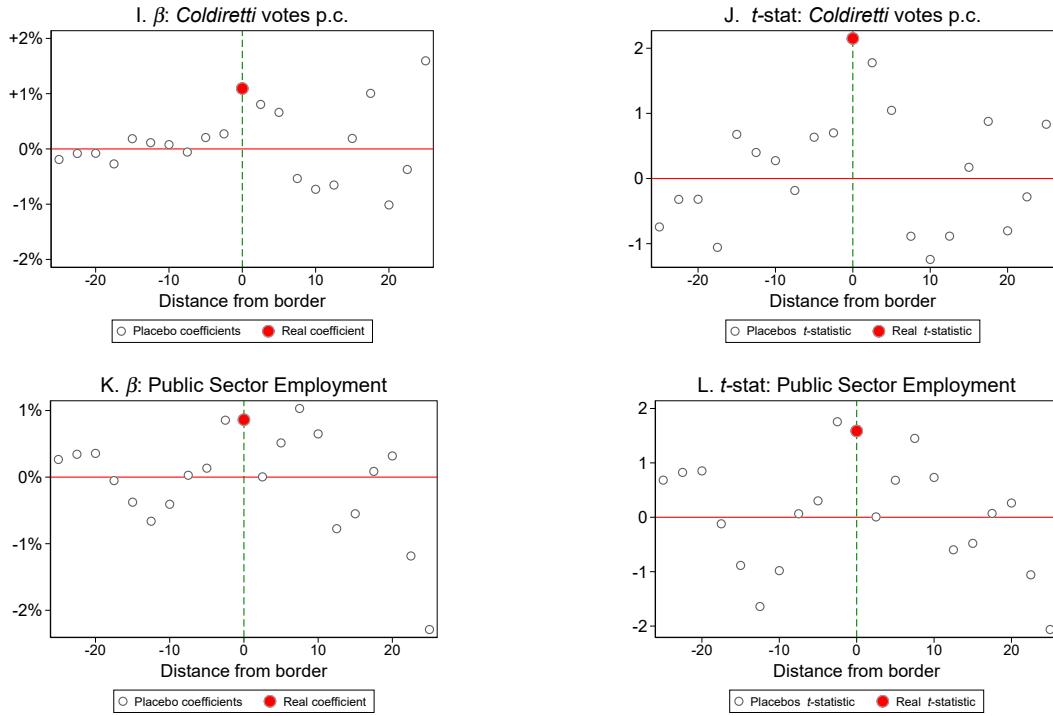
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Notes: The Table reports coefficients β_t from the panel RDD Equation (1), which controls for year \times reform area and town fixed effects. We include the 1992 election in the 1980s decade. The omitted category is the β of the elections of 1946 and 1948. The dependent variable is Christian Democrat (DC) vote share. Columns 1-4 control for a linear polynomial in distance, columns 5-8 control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. Column 1 and 5: baseline; the sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. Columns 2-4 and 6-8: “donut” RDD; the sample consists of all towns within 25 km but farther than 1.5, 2 and 2.5 km from the reform border. Standard errors clustered at the town level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

C.11 Placebo Borders

Figure C.9: Placebo borders – Coefficients and t -statistics





Notes: The Panels report results of placebo regressions discussed in Section 5.4. We simulate 20 fictitious reforms, by moving the reform border inside and outside the reform area in steps of 2.5 km and creating a new sample of all towns within 25 km from this new border. For each of these fictitious reforms, we estimate a single coefficient for the impact of the reform on in the post-reform years. Panels A, C, E, G, I and K: estimated β . Panels B, D, F, H, J and L: t -statistics calculated from standard errors clustered at the town level (Panels B, H, J and L) or heteroscedastic robust (Panels D and F). In every panel we plot in red the coefficient and t -statistic we obtain when we estimate the effect in the true reform area. Units of observation are town-years (Panels A, B and G-L) or towns (Panels C-F).

C.12 Spatial Standard Errors

Table C.7: Standard error robust to temporal and spatial correlation.

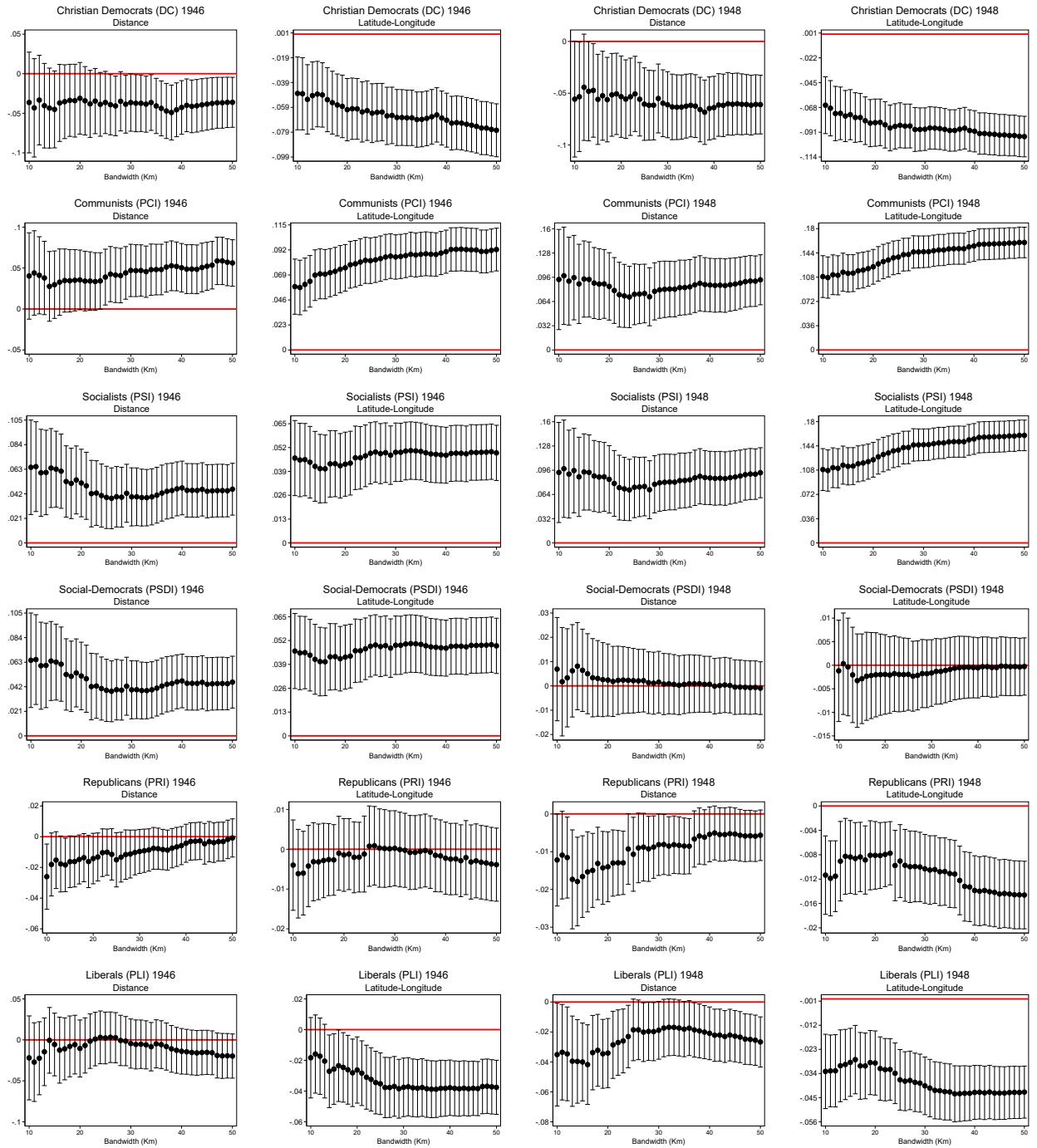
	DC	revenues p.c.	expenditure p.c.	Casse Mutue votes p.c.	Coldiretti votes p.c.	Public Sct. Employment
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Distance						
Treatment × Post	0.044	120.716	119.382	0.014	0.011	0.009
Cluster: town	[0.015]***	[55.278]**	[49.953]**	[0.006]**	[0.005]**	[0.005]
Conley s.e.: cutoff = 5 km	[0.015]***	[55.019]**	[50.298]**	[0.006]**	[0.005]**	[0.005]
Conley s.e.: cutoff = 10 km	[0.015]***	[55.947]**	[51.289]**	[0.006]**	[0.005]**	[0.006]
Conley s.e.: cutoff = 25 km	[0.017]***	[57.933]**	[52.866]**	[0.007]**	[0.006]**	[0.006]
Conley s.e.: cutoff = 50 km	[0.019]**	[54.394]**	[53.396]**	[0.007]**	[0.005]**	[0.007]
Conley s.e.: cutoff = 100 km	[0.021]**	[47.554]**	[50.035]**	[0.007]**	[0.005]**	[0.007]
Panel B: Latitude-Longitude						
Treatment × Post	0.028	118.237	95.466	0.007	0.006	0.016
Cluster: town	[0.009]***	[38.874]***	[35.648]***	[0.004]*	[0.003]**	[0.004]***
Conley s.e.: cutoff = 5 km	[0.009]***	[38.447]***	[35.645]***	[0.004]	[0.003]*	[0.004]***
Conley s.e.: cutoff = 10 km	[0.009]***	[39.504]***	[36.380]***	[0.004]	[0.003]*	[0.004]***
Conley s.e.: cutoff = 25 km	[0.008]***	[41.321]***	[38.885]**	[0.005]	[0.003]*	[0.005]***
Conley s.e.: cutoff = 50 km	[0.007]***	[41.922]***	[40.870]**	[0.005]	[0.004]	[0.006]***
Conley s.e.: cutoff = 100 km	[0.006]***	[37.453]***	[36.784]***	[0.005]	[0.004]	[0.006]**
Mean Y Control Group	0.36	437.79	316.00	0.03	0.02	0.05
Observations	5838	488	488	1451	1419	2939

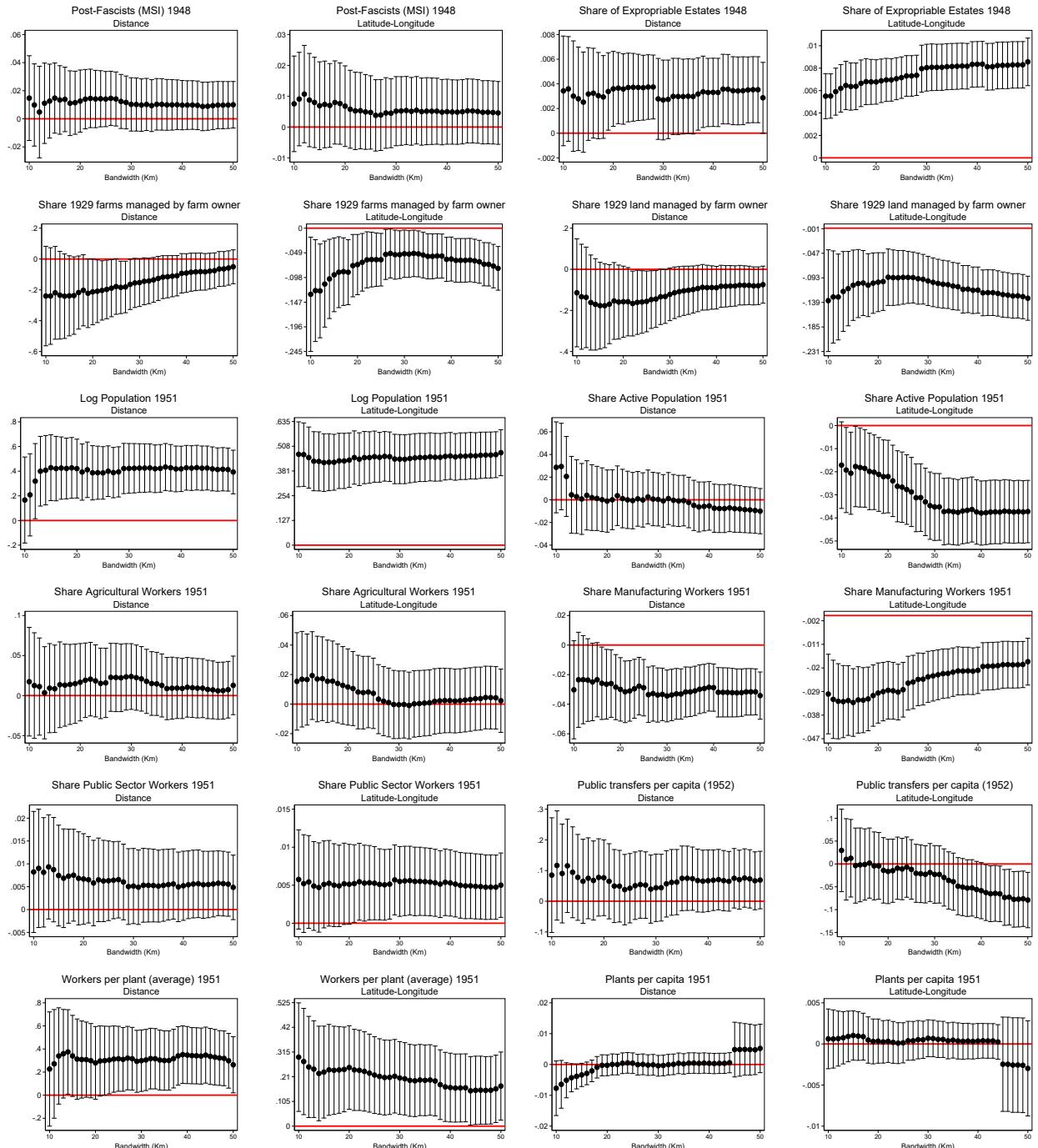
Notes: The Table reports on the first row coefficients β_t and β from alternative specifications of Equations (1) (Cols. 1, 4, 5, 6) and (2) (Cols. 2, 3). Panel A controls for a linear polynomial in distance, Panel B for a quadratic polynomial in latitude and longitude. Cols. 1, 6: regressions control for reform area \times year and town fixed effects. Cols 2, 3: regressions control for reform area fixed effects. Cols. 4, 5: regressions control for reform area \times year fixed effects. Units of observation are town-years (Cols. 1, 4, 5, 6) or towns (Cols. 2, 3). The sample consists of all towns within 25 km of the reform borders of Delta Padano and Maremma. Row 2 of each panel: standard errors clustered at the town level (Cols. 1, 4, 5, 6) or heteroschedastic robust (Cols. 2, 3) in parentheses. Rows 3-7 of each panel: standard errors robust to time-series and spatial correlation calculated with the formula of Conley (1999). In these estimates, spatial correlation is assumed to decay linearly until a cutoff. We report results from 5 different cutoffs: 5 km, 10 km, 25 km, 50 km and 100 km.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

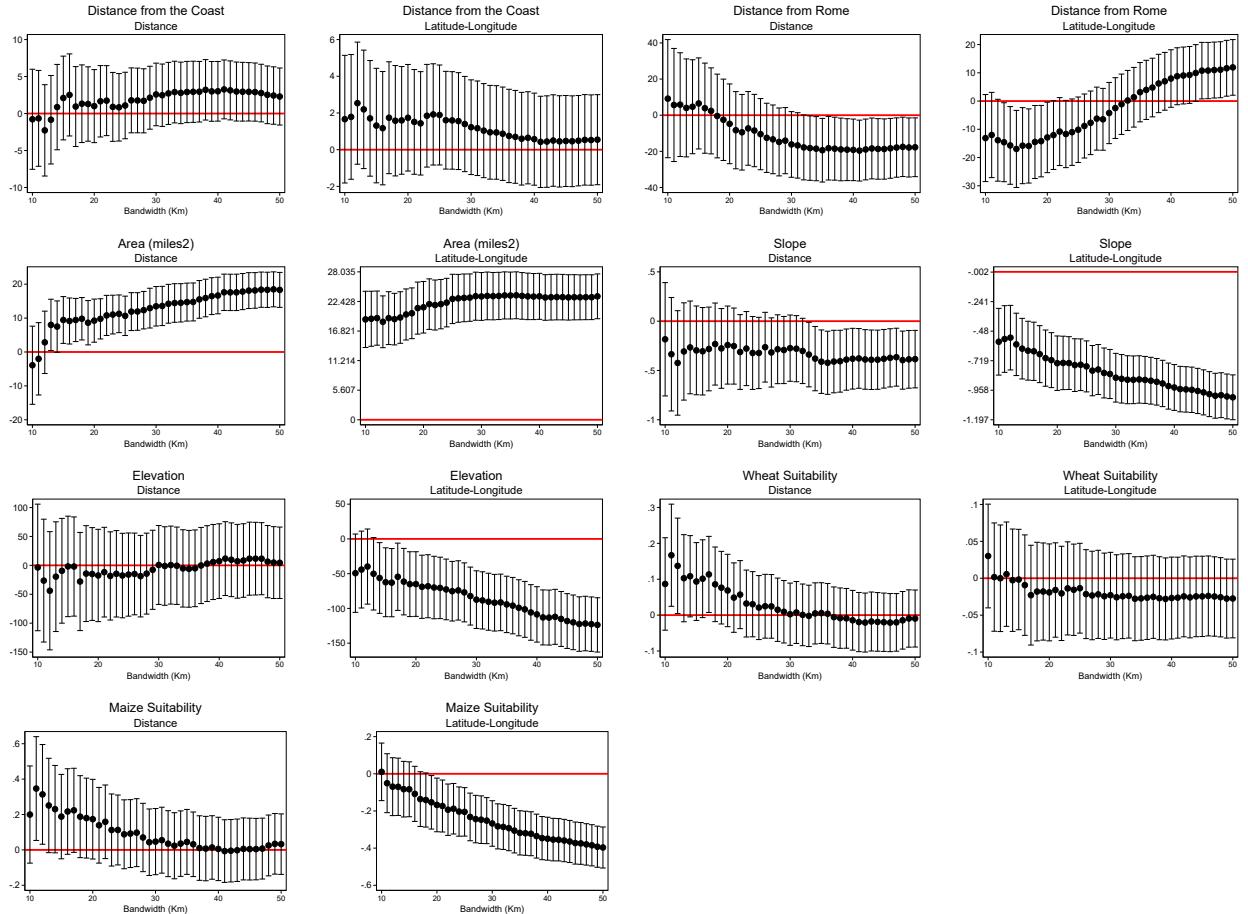
D Additional Results

D.1 Southern Italy

Figure D.1: Balance – Southern Italy

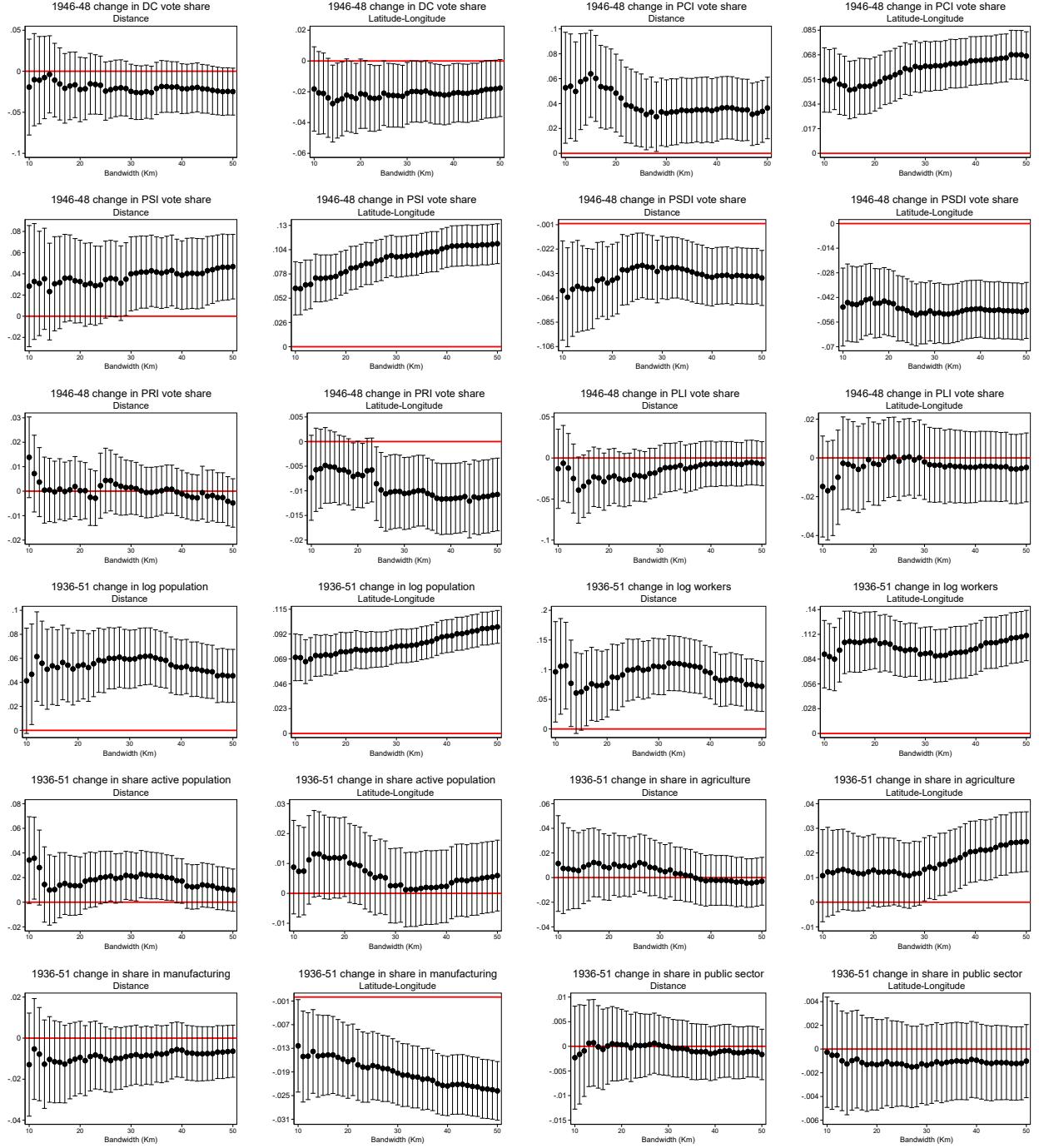






Notes: The graphs report the coefficients of separate regressions of the RDD specification in Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. The subtitle *Distance* indicates the RDD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RDD regressions controlling for a quadratic polynomial in latitude and longitude. See Appendix B for a detailed description of each of these variables and their sources. Units of observation are towns. The sample consists of all towns close to the reform borders of Fucino, Opera Combattenti, Sila, and Puglia-Lucania.

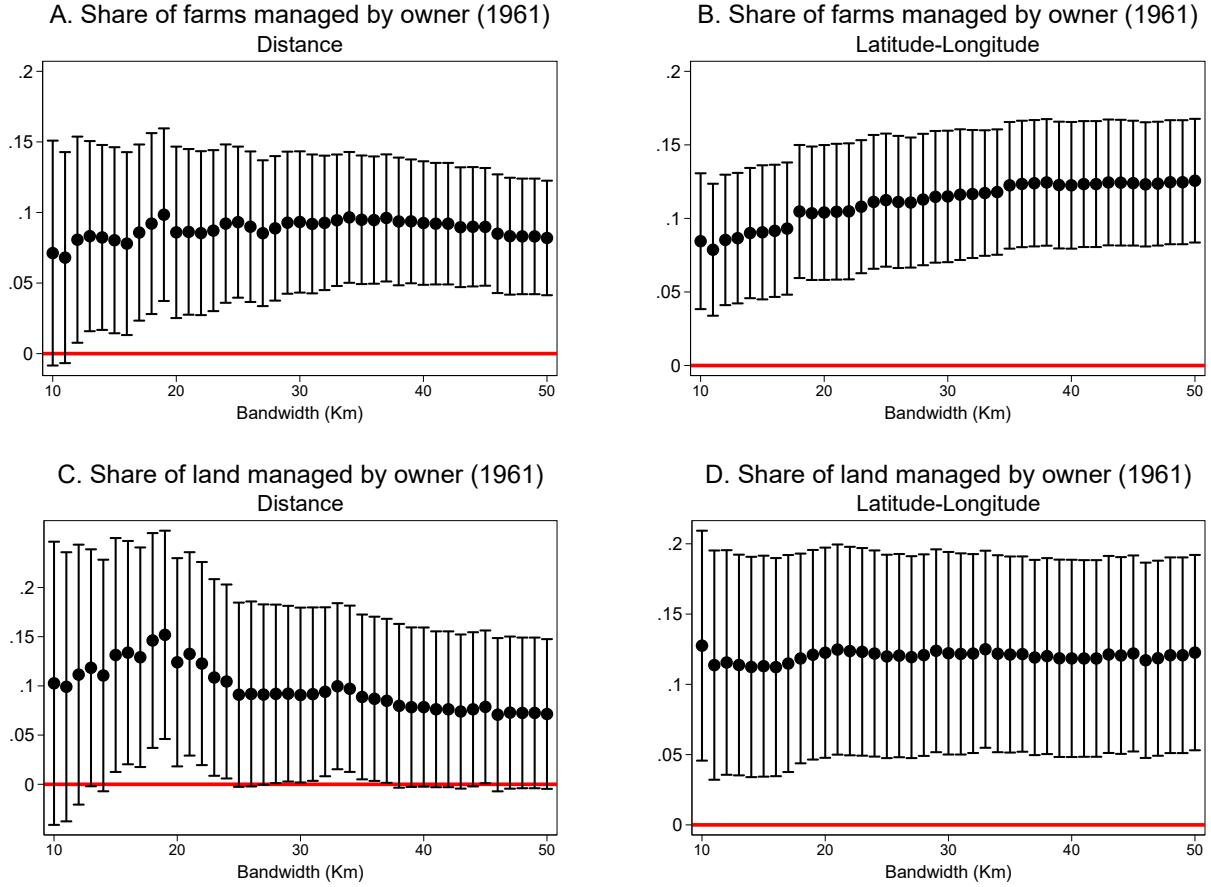
Figure D.2: Pre-trends – Southern Italy



Notes: The graphs report the coefficients of separate regressions of the RDD specification in Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. Dependent variables are changes in the outcome variable between 1946 and 1948 for electoral variables and between 1936 and 1951 for economic and demographic variables. The subtitle *Distance* indicates the RDD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RDD regressions controlling for a quadratic polynomial in latitude and longitude. See Appendix B for a detailed description of each of these variables and their sources. Units of observation are towns. The sample consists of all towns close to the reform borders of Fucino, Opera Combattenti, Sila, and Puglia-Lucania.

D.2 The impact of the reform on farm management. Controlling for outcomes in 1929.

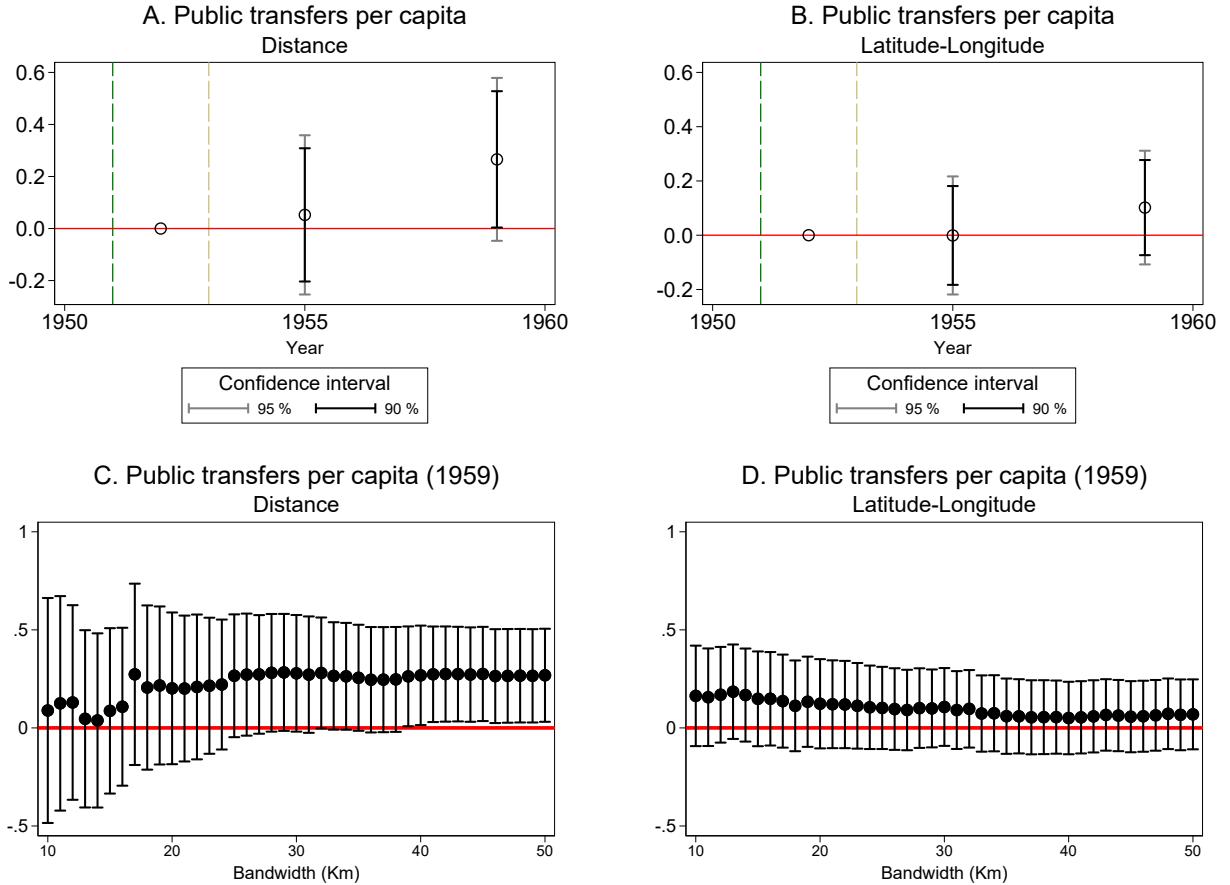
Figure D.4: The impact of the reform on farm management.



Notes: The graphs reports β of RDD regression Equation (2) and 95% confidence intervals, for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. Panels A and B: dependent variable is share of *farms* managed by the farms owner in 1961. Panels C and D: dependent variable is the share of *land* managed by the farm owner in 1961. All regressions control for the dependent variable measured in 1929. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Results are similar when controlling for the dependent variable observed in 1929. Source of 1961 farm management is the 1961 Agricultural Census; source of 1929 variables is ISTAT (1936). Units of observation are towns. We estimate heteroschedasticity robust standard errors and plot 95% confidence intervals as bars around the coefficients.

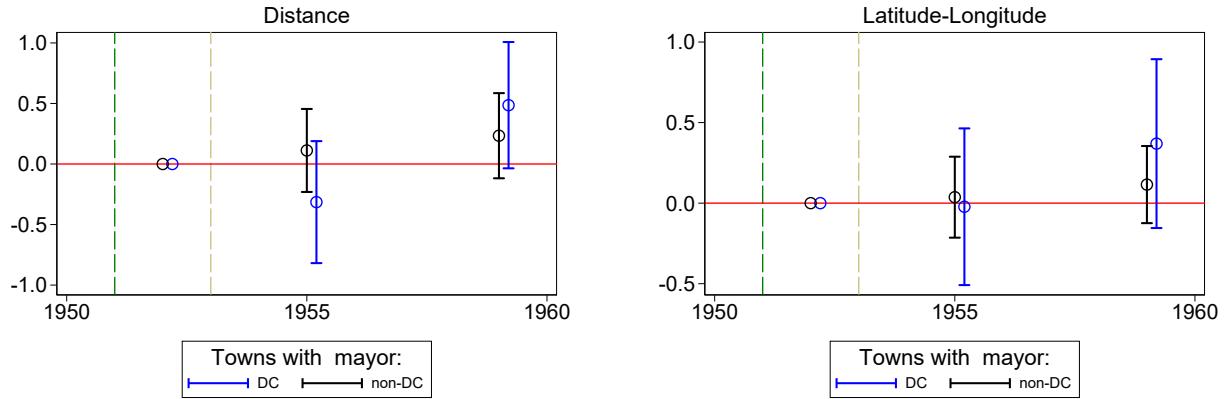
D.3 The impact of the reform on pork-barrel spending

Figure D.5: The impact of the reform on pork spending.



Notes: Dependent variable is the inverse sin of public transfers (from central to local government) per capita, a proxy for pork barrel spending. Panels A and B display coefficients β_t for each year from the panel RDD Equation (1). The omitted category is the β of 1952 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The green vertical dashed lines mark the 1950 land reform, the yellow ones mark the end of the land redistribution (1953). Panels C and D report the coefficients for year 1959, for each bandwidth between 10 and 50 km in 1 km intervals. from the reform borders of Delta Padano and Maremma. All regressions control for year \times reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure D.6: The impact of the reform on pork spending. The role of aligned mayors.



Notes: The Figure displays coefficients β_t from the panel RDD Equation (2) for two groups of towns: those with DC mayors in 1946 (in blue) and the others (in black). The dependent variable is the inverse sine transformation of per capita fiscal transfers from the central government to the municipal governments (available only for 1952, 1955 and 1959). Panel A controls for a linear polynomial in distance, Panel B controls for a quadratic polynomial in latitude and longitude. Both regressions control for year \times reform area \times 1946 mayor affiliation and town fixed effects. The omitted categories are the β of 1952 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The green vertical lines mark the 1950 land reform and the grey vertical line marks the year in which the first post-reform Parliament took office. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

D.4 Effect of the Reform on PCI, Other Parties and Turnout

The 1950 Italian land reform was designed to counteract the rise of Communism in the countryside. This appendix provides support to historical records discussed in Section 2 by looking at the effect of the reform on PCI vote share. In addition, we show that the reform did not have any major impact on turnout and on the vote share of other political parties.

Preliminary Graphical Evidence. Appendix Figure D.7 presents graphical evidence by plotting vote shares against distance to the border. PCI vote share was similar across the border in 1948 (Panel A) and shows parallel pre-trends between 1946 and 1948 (Panel B). The reform determined a large drop in the support for PCI: relative to pre-reform elections (1946-48), in 1953-1992 treated towns at the border reduce PCI support by 3 p.p. Appendix Figure D.8 shows these effects on a map of Italy, confirming that border towns inside of the reform area swung away from PCI.

Panel RD Estimates. These effects appear immediately after the reform, and are stable until the 1990s. Appendix Figure D.9-Panels A-B plot the effect of the reform on PCI vote share over the entire period (β_t in Equation (1)) in the two specifications. The 1946 coefficient confirms the absence of differential trends pre-reform (see also Figure 2). The treatment coefficient in the 1953 election suggests that in treated towns PCI vote share decreased by between 2 and 4 p.p. during the first election after the land reform, from a control mean of 33%: a decrease of between 6 and 12 percent. This effect is larger but noisier in the distance specification. Finally, PCI loss remains large and quite stable until 1987, even as they become noisier in the 80s. When we pool together all post-reform elections until 1992 in Appendix Figure D.9-Panels C-D, we find that the size of the effect is stable across bandwidths. However, the effects are smaller and noisier in narrower bandwidths in the specification controlling for distance. These results confirm that the land redistribution was successful in stemming the rise of communism in the countryside.

Pre-Fascism elections. Long-term pre-trends suggest that these effects may underestimate the true effect of the reform on PCI support. Appendix Figure D.10 reports treatment coefficients from a panel RDD regression that includes the 1919, 1921, and 1924 elections (the last ones before the Fascist dictatorship), as well as the 1946 and the 1948 ones. We look at two left-wing parties: the Italian Socialist Party (PSI) and the Italian Communist Party (PCI). PSI was the largest left-wing party until 1947: it won relative majorities in the elections of 1919 and 1921 and had one of its leaders, Giacomo Matteotti, killed by fascist hit men in 1924.⁵⁰ PCI was relatively small before the war: founded in 1921, it collected

⁵⁰PSI lost ground to PCI after 1947, when it split into 2 parties. One of these parties ran together with

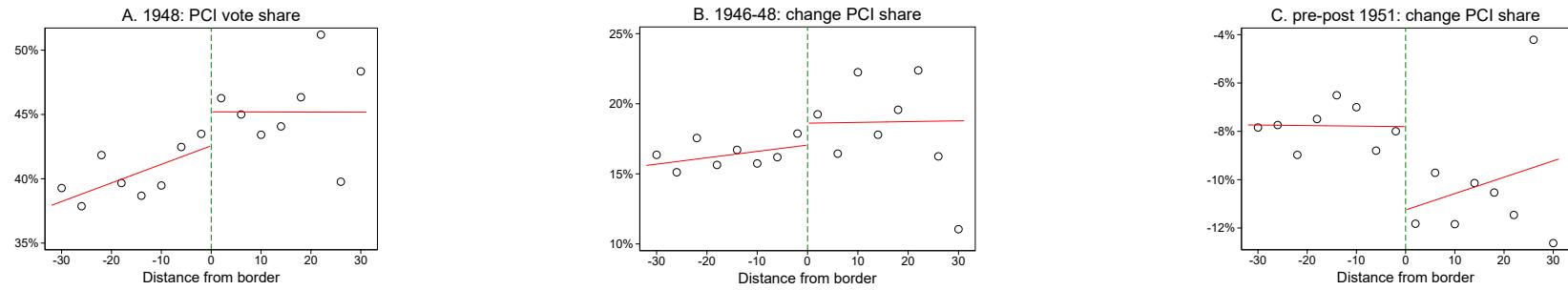
4.6% of votes that year and 3.7% in 1924. We report pre-fascism elections results for the two specifications at 25 km, for both parties (Appendix Figure D.10). PSI vote shares exhibit parallel pre-trends in the specification controlling for a linear polynomial in distance, while the coefficients for 1921 and 1924 elections are negative and significant in the coordinate specification. PCI vote shares seem to grow faster in treatment towns, although pre-trends coefficients are not significantly different from zero in the specification controlling for a linear polynomial in distance. Growing support for left-wing parties pre-reform may bias the PCI treatment effect downward.

Effect on other parties and turnout. In Appendix Figure D.11 we pool together all post-reform elections until 1992 and, using both our preferred specifications, we find that the reform did not have a significant effect on the vote share of other parties. This null effect is robust across different bandwidths. The only exception is a negative effect on the vote share of MSI (a very small neo-fascist party) when using the specification controlling for a quadratic polynomial in latitude and longitude. We conclude that around 85% of DC gains were at the expense of the Communist Party, while the remaining may have been at the expense of MSI. Additionally, in Appendix Figure D.12, we verify that the reform did not affect turnout.⁵¹

PCI in 1948. After that year, PSI never received more than 15% of votes. When we look at the effect of the reform on PSI vote shares in 1946-92 we find no significant effect (see Appendix D.11).

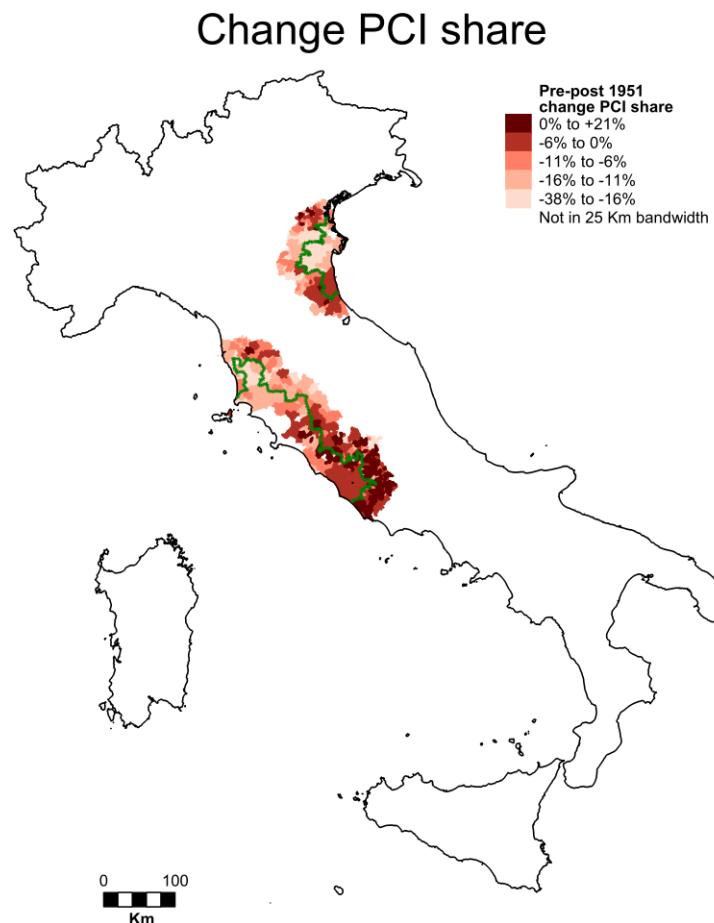
⁵¹1992 elections are an exception especially when using the specification controlling for distance from the border. However, the effect is very small given the high average turnout (95% in control towns across all years in the regression).

Figure D.7: Balance, pre-trends and effect of reform on PCI vote share: graphical evidence



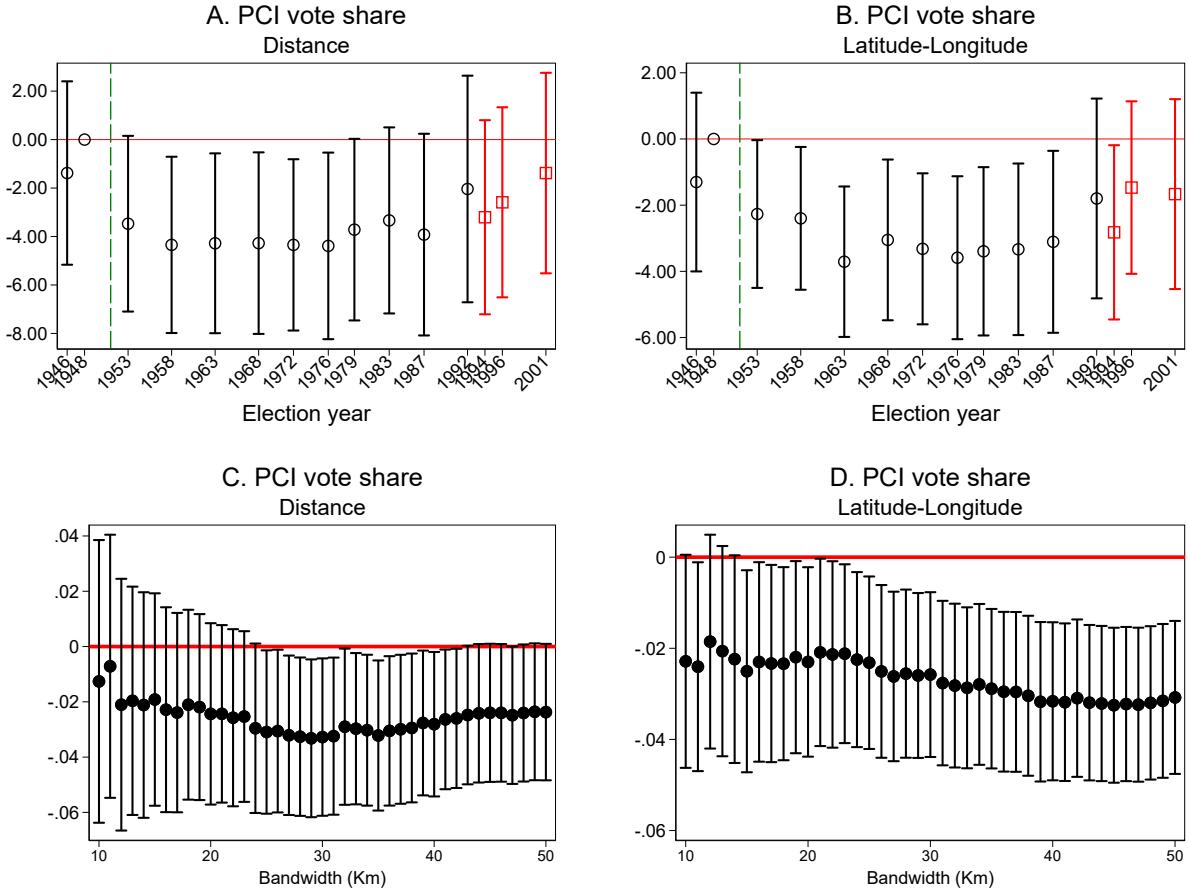
Notes: The Figure presents graphical evidence on the panel RDD. On the y-axes we plot electoral outcomes; on the x-axes the distance to the border. In each Panel, we bin data in 4 km intervals. Treated towns have positive distance and control towns have negative distance. The red lines report linear fits from regressions of the outcome on the distance from the border (separately for the two sides of the discontinuity). Panel A: dependent variable is Communist Party (PCI) vote share in 1948 (the last election before the land reform). Panel B: dependent variable is change in PCI vote share between the 1946 and 1948 (the two elections before the land reform). Panel C: dependent variable is change in PCI vote share between pre- (1946-48) and post-reform elections (1953-92). For PCI we use the vote share of the Popular Democratic Front (FDP) in 1948 and the vote share for the Democratic Party of the Left (PDS) in 1992. Electoral data are from Corbetta and Piretti (2009). The sample consists of all towns within 32 km from the reform borders of Delta Padano and Maremma.

Figure D.8: Electoral effect of the reform: visualization of impact on a map



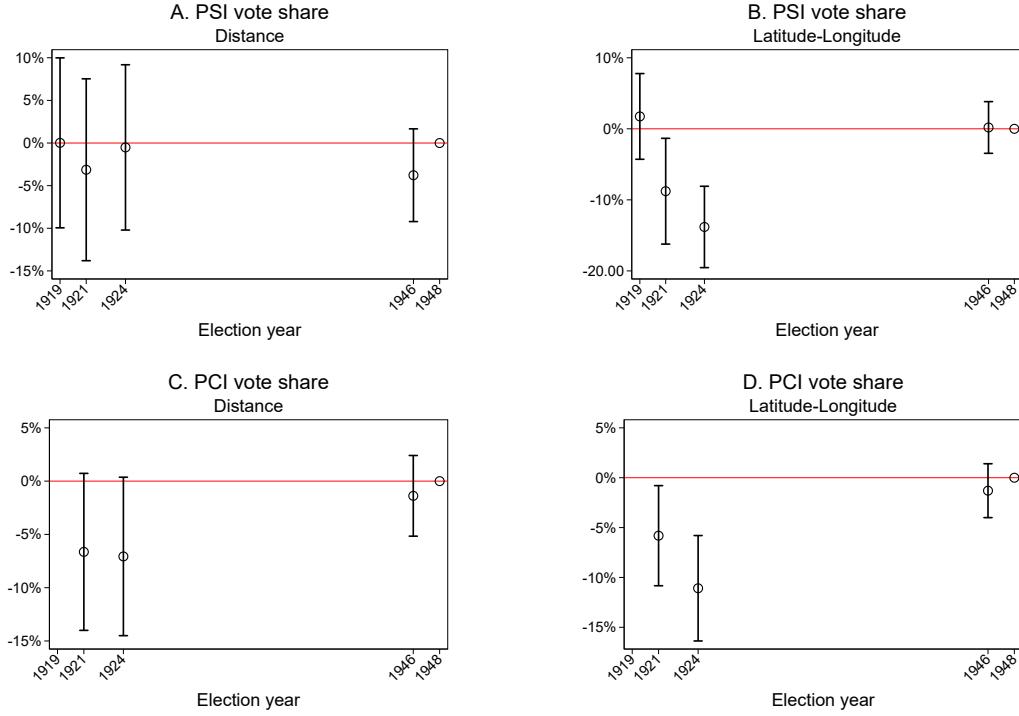
Notes: Change in PCI vote share between pre- (1946-48) and post-reform elections (1953-92) in the towns within 25 km from the reform borders of Delta Padano and Maremma.

Figure D.9: The electoral impact of the land reform – Communist Party (PCI)



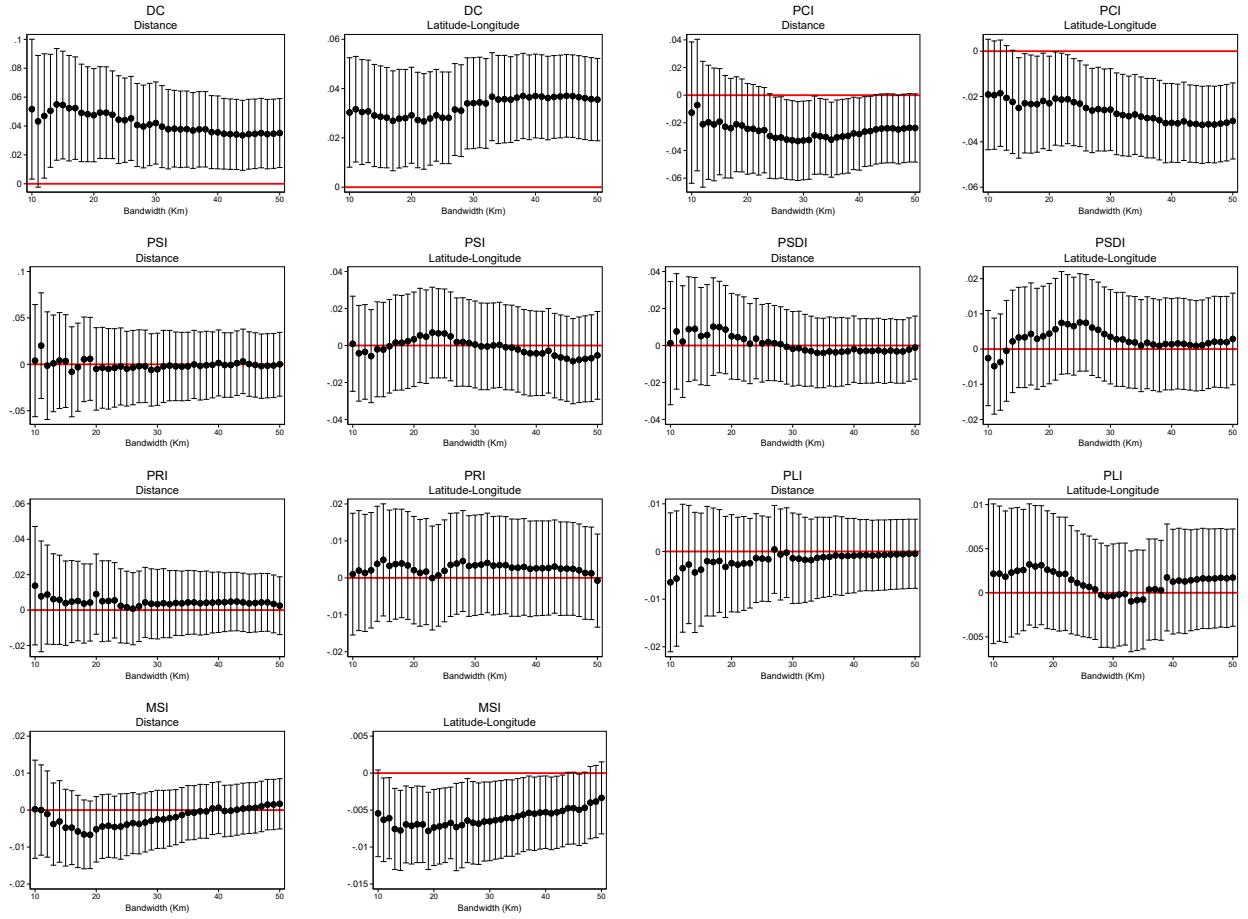
Notes: Dependent variable is Communist Party (PCI) vote share. Panels A and B display coefficients β_t for each year from the panel RDD Equation (1). The omitted category is the β of 1948 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. Panels C and D report pooled treatment effects for all electoral years between 1953 and 1992. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for year \times reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. For PCI we use the vote share of the Popular Democratic Front (FDP) in 1948 and the vote share for the Democratic Party of the Left (PDS) in 1992. In the post-1992 elections we consider PCI the following parties: Democratic Party of the Left (1992); Democratic Party of the Left and Communist Refoundation Party (1994-96); Democrats of the Left, Communist Refoundation Party and Communist Party (2001). Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure D.10: 1919-1948 Pre-Trends PCI and PSI



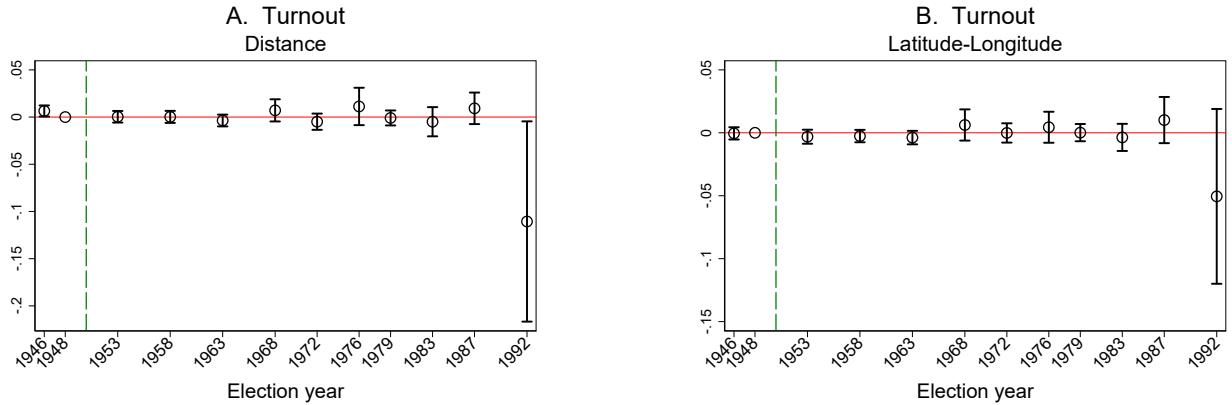
Notes: Dependent variable is Socialist Party (PSI) vote share (Panel A and B) and Communist Party (PCI) vote share (Panel C and D). All panels display coefficients β_t for each year before the reform from the panel RDD Equation (1). The omitted category is the β of 1948 and the sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. All regressions control for year \times reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. We use the vote share of the Italian Popular Party (PPI) in the years 1919-24. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure D.11: The impact of the land reform on all major Italian parties



Notes: The graphs report the pooled treatment effects for all electoral years between 1953 and 1992 ((1)). We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. The dependent variables are the vote shares of the main Italian political parties in this period. The subtitle *Distance* indicates the RDD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RDD regressions controlling for a quadratic polynomial in latitude and longitude. All regressions control for year \times reform area and town fixed effects. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

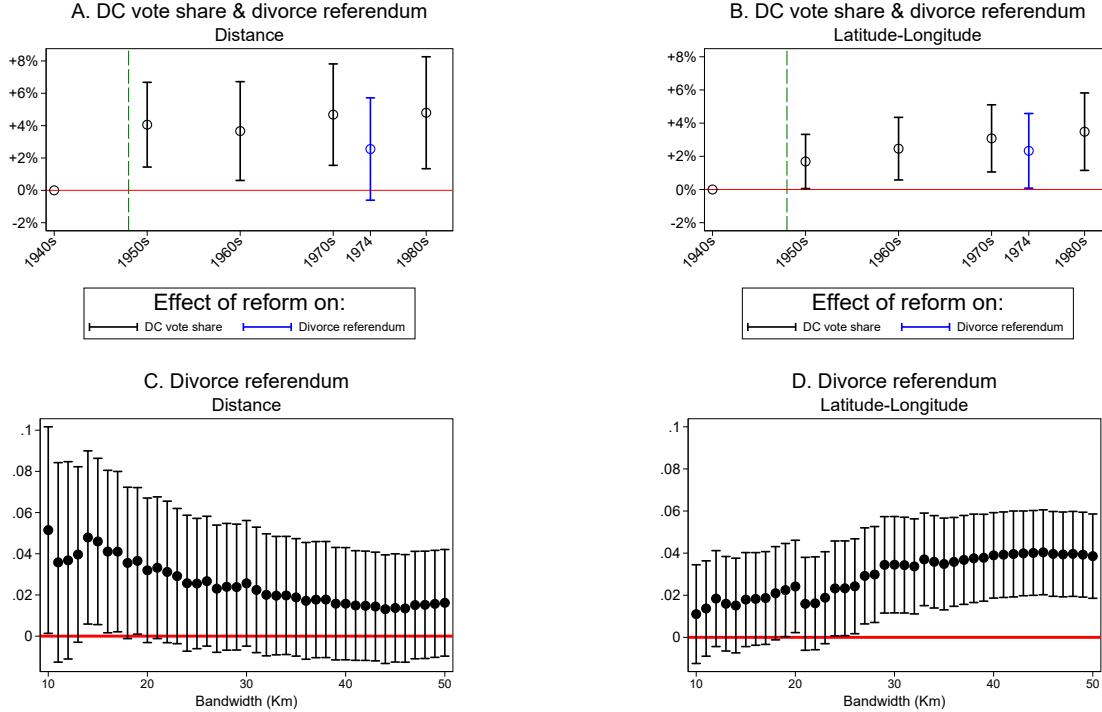
Figure D.12: Turnout: Panel RDD Coefficients



Notes: The Figure displays coefficients β_t from the panel RDD Equation (1), which controls for year \times reform area and town fixed effects. The omitted category is the β of 1948. Dependent variable is votes cast divided by number of eligible voters. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. Units of observation are town-years. The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

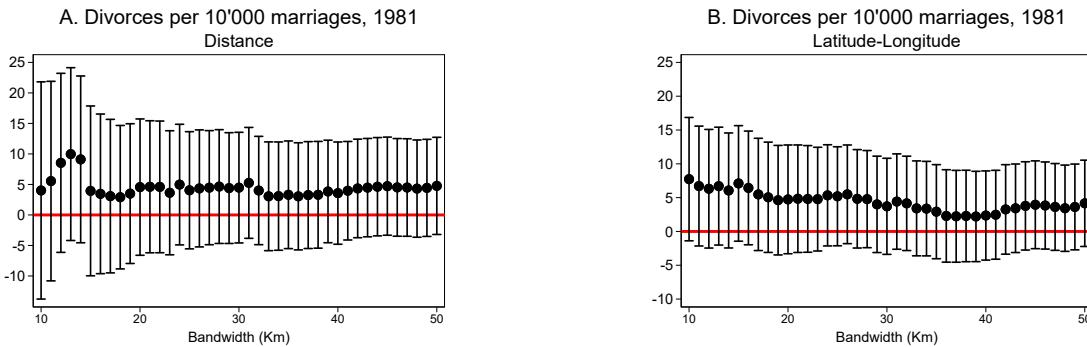
D.5 Divorce Referendum

Figure D.13: 1974 Referendum on the Repeal of Divorce



Notes: Panel A and B display coefficients β_t from the panel RDD Equation (1), which controls for year \times reform area and town fixed effects. We estimate β for separate decades as well as for the 1974 divorce referendum. The omitted category is the β of the elections of 1946 and 1948. We include the 1992 election in the 1980s decade. The dependent variable is Christian Democrat (DC) vote share in every year except 1974; the source is Corbetta and Piretti (2009). In these years, we plot the β in black. In 1974 dependent variable is share of "yes" votes in the divorce referendum; the source is Ministry of Interior (1977). In this year, we plot the coefficient in blue. The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. Panel C and D report treatment effects for the 1974 divorce referendum. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. Panel A and C control for a linear polynomial in distance. Panel B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure D.14: Divorces per 10'000 marriages 1981



Notes: The graphs reports coefficients β_t of the RDD Equation (2). We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for reform area fixed effects. Dependent variable is the number of divorces per 10'000 marriages. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.

D.6 Migration

Table D.1: Impact of reform on the number of eligible voters and DC votes. Bandwidth: 25 km.

	Distance		Latitude-Longitude	
	(1) log eligible voters	(2) log DC votes	(3) log eligible voters	(4) log DC votes
Treatment × 1950s	-0.040 [0.025]	0.094 [0.058]	-0.010 [0.013]	0.072** [0.033]
Treatment × 1960s	-0.105* [0.060]	0.035 [0.076]	-0.078** [0.032]	0.043 [0.044]
Treatment × 1970s	-0.156 [0.095]	0.011 [0.102]	-0.134*** [0.051]	0.010 [0.060]
Treatment × 1980s	-0.178 [0.125]	-0.004 [0.130]	-0.153** [0.065]	-0.000 [0.072]
R^2	0.111	0.222	0.355	0.373
Mean Y Control Group	7.95	6.79	7.95	6.79
Observations	5838	5838	5838	5838

Notes: Panel A and B display coefficients β_t from the panel RDD Equation (1), controlling for year \times reform area and town fixed effects. We pool together the coefficients of all years in the same decade. The omitted categories are the β s of 1946 and 1948. Election year 1992 is included in the 80s. The sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. Dependent variables are log number of eligible voters (Column 1 and 3) and log number of DC votes (Column 2 and 4). Columns 1 and 2 control for a linear polynomial in distance. Column 3 and 4 control for a quadratic polynomial in latitude and longitude. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. Standard errors clustered at the town level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table D.2: Impact of reform on active population and sectoral shares. Bandwidth: 25 Km.

	Distance						Latitude-Longitude					
	(1) Active pop.	(2) Active men	(3) Active women	(4) Agriculture	(5) Manufacturing	(6) Services	(7) Active pop.	(8) Active men	(9) Active women	(10) Agriculture	(11) Manufacturing	(12) Services
Treatment \times 1936	0.007 [0.017]		0.017 [0.016]	-0.011 [0.011]	-0.456 [0.410]	-0.017 [0.011]			0.008 [0.016]	-0.014 [0.009]	-0.912 [0.685]	
Treatment \times 1961	-0.022 [0.017]	-0.011 [0.012]	-0.032 [0.031]	-0.026 [0.019]	0.006 [0.009]	-0.005 [0.005]	-0.046*** [0.010]	-0.018** [0.008]	-0.072*** [0.018]	-0.037*** [0.012]	-0.010 [0.007]	-0.002 [0.004]
Treatment \times 1971	-0.021 [0.017]	-0.016 [0.016]	-0.025 [0.030]	-0.031 [0.025]	0.005 [0.016]	0.020** [0.009]	-0.041*** [0.011]	-0.015 [0.011]	-0.062*** [0.019]	-0.049*** [0.017]	-0.010 [0.012]	0.011 [0.008]
Treatment \times 1981	-0.008 [0.019]	-0.014 [0.016]	0.003 [0.032]	-0.012 [0.029]	-0.010 [0.020]	0.012 [0.012]	-0.038*** [0.011]	-0.013 [0.012]	-0.054*** [0.020]	-0.035* [0.020]	-0.022 [0.015]	0.005 [0.010]
Treatment \times 1991	-0.003 [0.019]	-0.020 [0.013]	0.017 [0.033]	0.008 [0.032]	-0.027 [0.021]	0.026** [0.011]	-0.040*** [0.013]	-0.019* [0.011]	-0.055** [0.021]	-0.026 [0.022]	-0.011 [0.016]	0.004 [0.009]
Mean Y Control Group	0.56	0.85	0.31	0.41	0.22	0.21	0.56	0.85	0.31	0.41	0.22	0.21
Number of Towns	490	490	490	490	490	490	490	490	490	490	490	490
Observations	2939	2449	2449	2939	2939	2939	2939	2449	2449	2939	2939	2939

Notes: The Table reports coefficients β_t from the panel RDD Equation (1), which controls for year \times reform area and town fixed effects.

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Dependent variables are: share of people in the labor force (Column 1 and 7), share of men in the labor force (Column 2 and 8), share of women in the labor force (3 and 9), share of workers employed in agriculture (Column 4 and 10), manufacturing (5 and 11) and services (6 and 12).

Columns 1 to 6 control for a linear polynomial in distance. Columns 7 to 12 control for a quadratic polynomial in latitude and longitude. The omitted category is the β of 1951. Source is the decadal population censuses. Units of observation are town-years. The sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level in parentheses. *p<0.1,

p<0.05, *p<0.01.

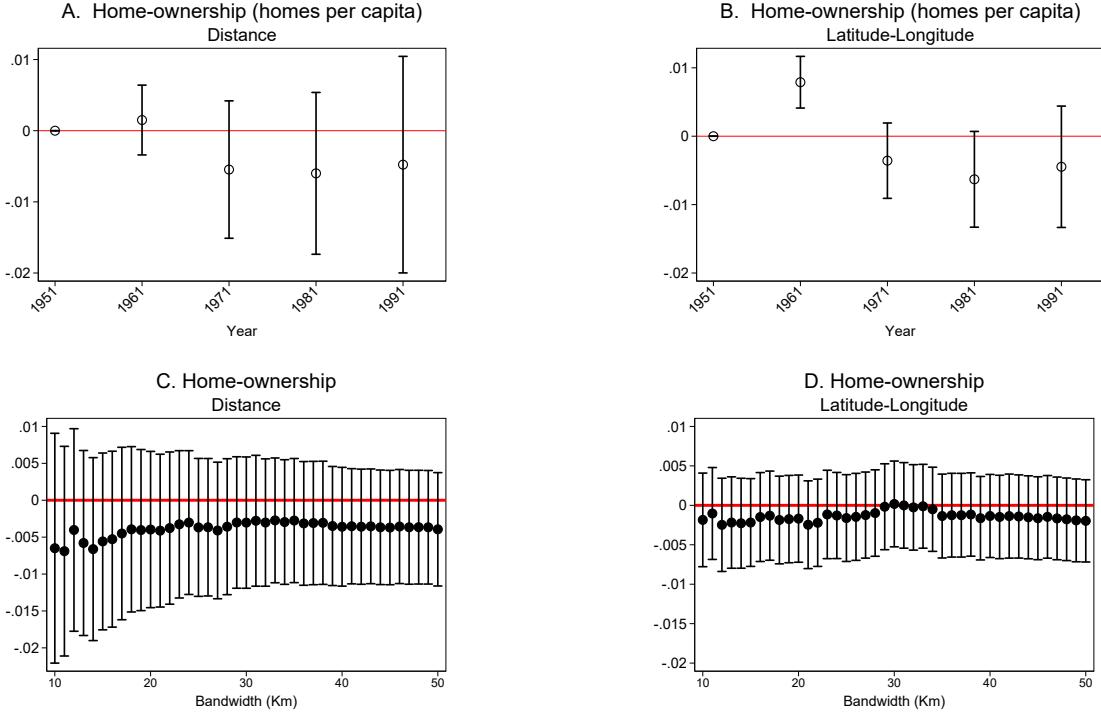
Table D.3: Impact of reform in the north on population structure. Bandwidth: 25 Km.

	Distance					Latitude-Longitude				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share population aged				Share males	Share population aged				Share males
	0-20	21-45	46-65	>65		0-20	21-45	46-65	>65	
Treatment × 1961	-0.003 [0.004]	-0.003 [0.004]	0.003 [0.004]	0.004 [0.003]	-0.001 [0.002]	-0.005* [0.003]	-0.001 [0.003]	0.004 [0.002]	0.002 [0.002]	-0.001 [0.001]
Treatment × 1971	-0.001 [0.007]	0.002 [0.005]	0.005 [0.006]	0.005 [0.005]	-0.003 [0.002]	-0.009** [0.004]	0.002 [0.003]	0.005 [0.004]	0.004 [0.003]	-0.003* [0.002]
Treatment × 1981	-0.006 [0.008]	-0.005 [0.008]	0.001 [0.006]	0.010 [0.009]	-0.004 [0.002]	-0.014** [0.006]	-0.002 [0.004]	0.007 [0.005]	0.009* [0.005]	-0.004** [0.002]
Treatment × 1991	-0.002 [0.007]	-0.009 [0.009]	0.002 [0.005]	0.012 [0.010]	-0.004 [0.003]	-0.016*** [0.005]	-0.004 [0.005]	0.011** [0.004]	0.011* [0.006]	-0.004** [0.002]
Mean Y Control Group	0.26	0.33	0.22	0.13	0.50	0.26	0.33	0.22	0.13	0.50
Number of Towns	490	490	490	490	490	490	490	490	490	490
Observations	2450	2450	2450	2450	2450	2450	2450	2450	2450	2450

Notes: The Table reports coefficients β_t from the panel RDD Equation (1), which controls for year × reform area and town fixed effects. Dependent variables are: share of people within specified age groups (Columns 1 to 4 and 6 to 9) and share of males in the population (Column 5 and 10). Columns 1 to 5 control for a linear polynomial in distance. Columns 6 to 10 control for a quadratic polynomial in latitude and longitude. The omitted category is the β of 1951. Source is the decadal population censuses. Units of observation are town-years. The sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

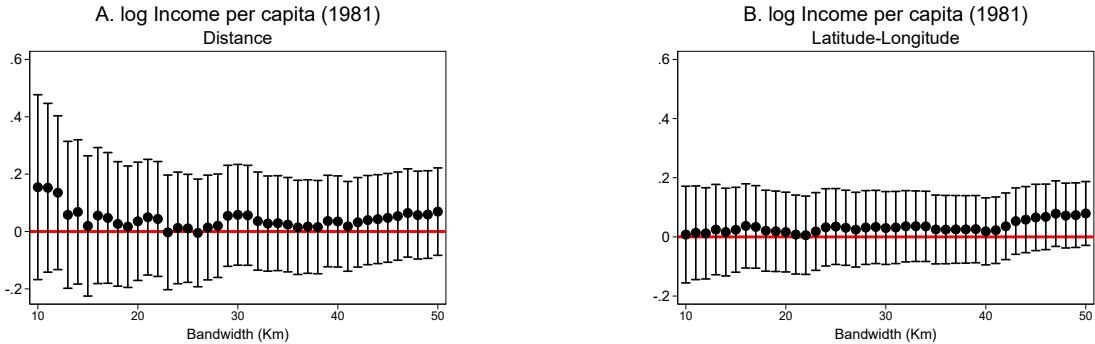
D.7 Economic Conservatorism

Figure D.15: The impact of the reform on home ownership.



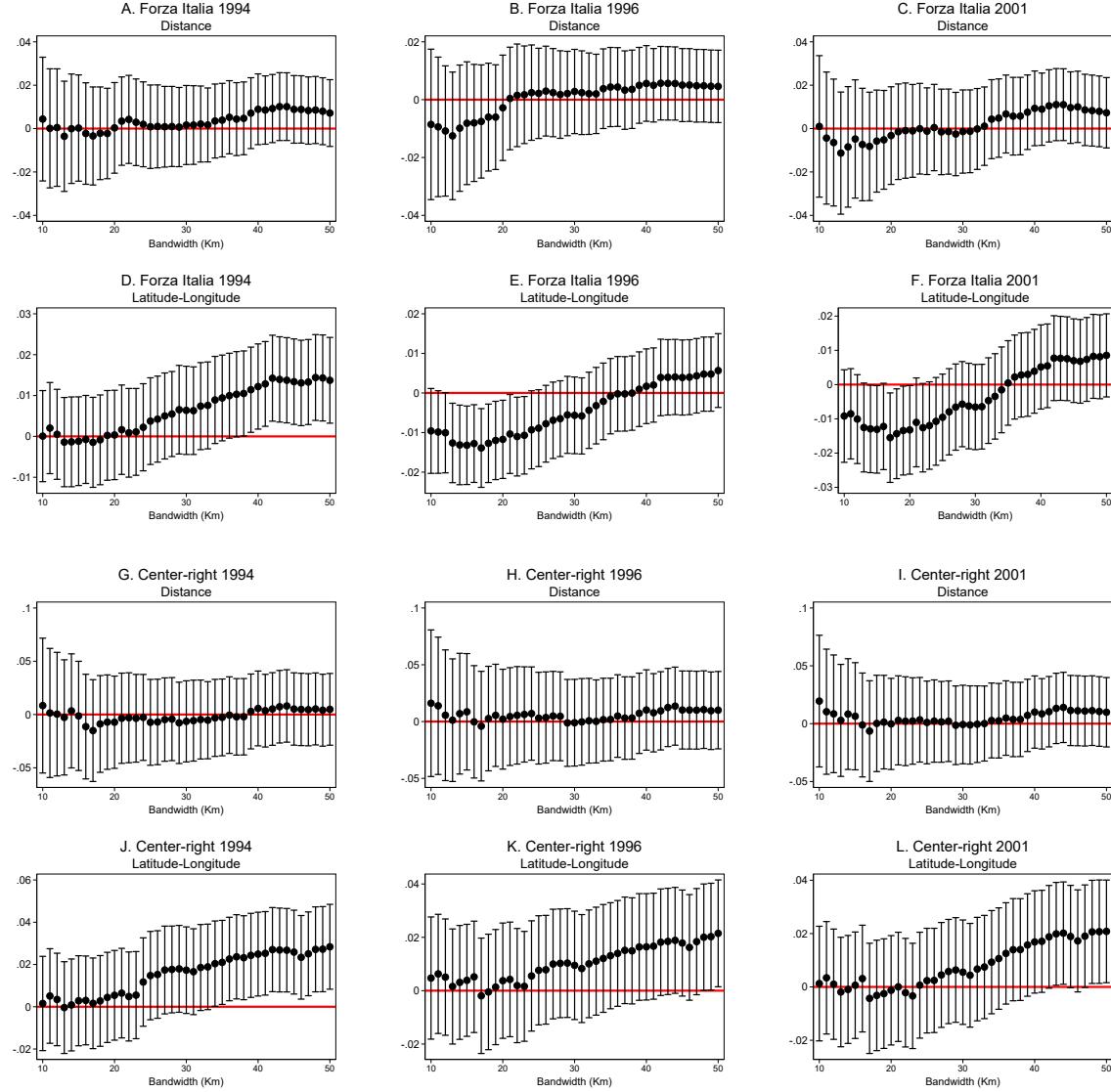
Notes: Panel A and B display coefficients β_t from the panel RDD Equation (1), which controls for year \times reform area and town fixed effects. We estimate β for separate census years. The omitted category is the β of the elections of 1951. The dependent variable is the number of property houses per capita. The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. Panel C and D report pooled treatment effects for all census years between 1961 and 1991. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. Panel A and C control for a linear polynomial in distance. Panel B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure D.16: The impact of the reform on income.



Notes: The graphs reports coefficients β_t of the RDD Equation (2). We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for reform area fixed effects. Dependent variable is the log income per capita in 1981. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.

Figure D.17: The impact of the reform on Forza Italia/center-right vote share after 1992.

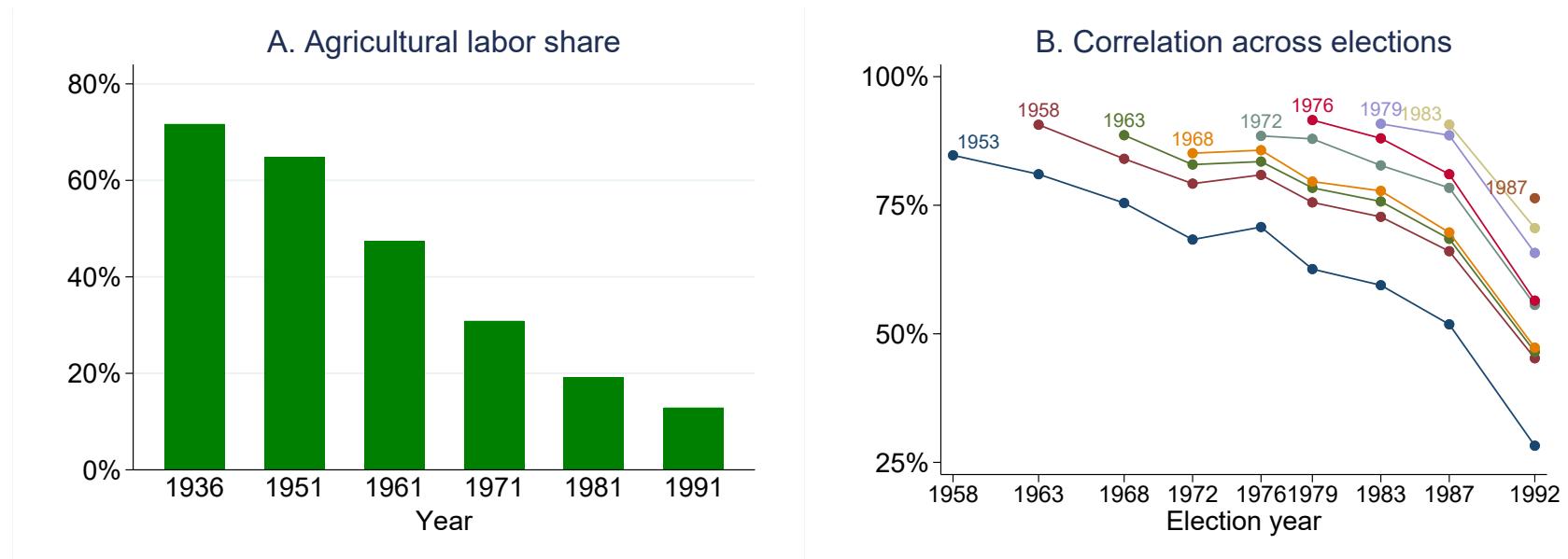


Notes: The graphs reports coefficients β_t of the RDD Equation (2). We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for reform area fixed effects. Dependent variables are: the vote share of Forza Italia (Berlusconi's party) in 1994 (Panel A and D), 1996 (Panel B and E) and 2001 (Panel C and F) and the vote share of major right-wing parties in 1994 (Panel G and J), 1996 (Panel H and K) and 2001 (Panel I and L). Panels A, B, C, G, H, and I control for a linear polynomial in distance. Panels D, E, F, J, K, and L control for a quadratic polynomial in latitude and longitude. Results controlling for Christian Democrat vote share in 1948 are very similar and available upon request. We include among the major post-1992 right-wing parties Forza Italia, Alleanza Nazionale, Lega Nord (all years) and Christian Democratic Center and the Christian Democratic Union (in 2001). Electoral data are from Corbetta and Piretti (2009). Units of observation are towns. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.

D.8 Changing society

Figure D.18: Share of Agricultural Workers and Correlation across Elections

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Notes: Panel A: share of workers employed in agriculture between 1936 and 1991. Source: decadal population censuses. Panel B: pairwise correlation of Christian Democrat (DC) vote share across election years. Each point corresponds to the pairwise correlation of town-level DC vote share in two separate elections. Correlation is on the y-axis; one of the election years on the x-axis the other is marked on top of the lines. The lines connects correlations of the same election year. The sample consists of all Italian towns.

D.9 Firms' growth

Table D.4: Impact of reform in the north on the number of workers per firm plant.

	Distance						Latitude-Longitude					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Number of plants per capita			Number of workers per plant			Number of plants per capita			Number of workers per plant		
	< 25km	< 10km	< 50km	< 25km	< 10km	< 50km	< 25km	< 10km	< 50km	< 25km	< 10km	< 50km
Treatment × 1961	-0.001 [0.003]	-0.000 [0.005]	-0.003 [0.003]	0.311 [0.245]	0.740* [0.419]	0.075 [0.198]	-0.002 [0.003]	-0.002 [0.003]	-0.002 [0.003]	-0.019 [0.195]	0.193 [0.232]	-0.125 [0.155]
Treatment × 1971	-0.001 [0.003]	0.002 [0.006]	-0.004 [0.004]	-0.056 [0.276]	0.061 [0.423]	-0.254 [0.223]	-0.001 [0.004]	-0.001 [0.003]	-0.002 [0.003]	-0.642*** [0.248]	-0.303 [0.277]	-0.690*** [0.212]
Treatment × 1981	0.001 [0.004]	0.003 [0.007]	-0.002 [0.004]	-0.267 [0.337]	0.177 [0.506]	-0.428 [0.261]	-0.002 [0.004]	-0.003 [0.003]	-0.004 [0.004]	-0.622** [0.270]	-0.296 [0.309]	-0.481** [0.227]
Treatment × 1991	-0.001 [0.004]	-0.004 [0.007]	-0.004 [0.004]	-0.349 [0.309]	0.096 [0.485]	-0.516* [0.269]	-0.006 [0.004]	-0.006* [0.003]	-0.007** [0.004]	-0.717*** [0.256]	-0.456* [0.272]	-0.575*** [0.207]
Mean Y Control Group	0.04	0.04	0.05	2.95	2.79	3.17	0.04	0.04	0.05	2.95	2.79	3.17
Number of Towns	490	222	863	490	222	863	490	222	863	490	222	863
Observations	2443	1107	4308	2443	1107	4308	2443	1107	4308	2443	1107	4308

Notes: The Table reports coefficients β_t from the panel RDD Equation (1), which controls for year \times reform area and town fixed effects. For consistency across years, the sectors included in the regressions are: manufacturing, extraction, construction, commerce, trasport, utilities, social services, banking and insurance. Columns 1-3 and 7-9 dependent variable is the number of plants per capita. Columns 4-6 and 10-12: dependent variable is the number of workers per plant. The omitted category is the β of 1951. Columns 1-6 control for a linear polynomial in distance. Columns 7-12 control for a quadratic polynomial in latitude and longitude. Source are the decadal economic and population censuses. Units of observation are town-years. The samples consists of all towns within 25 km (Column 1, 4, 7, 10), 10 km (Column 2, 5, 8, 11) and 50 km (Column 3, 6, 9, 12) to the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level in parentheses. *p<0.1, **p<0.05, ***p<0.01.