

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

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First Draft: March 2019

This Draft: June 2021

## Abstract

Many democracies around the world feature pervasive clientelist practices. Inequality is often considered a key determinant of these practices. By reducing inequality, redistributive policies may therefore undermine clientelism. However, by inducing gratitude and reciprocity among beneficiaries, redistribution may also initiate clientelist exchange. We study the long-term effects of 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 benefits.

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

JEL Classification: P16, N44, Q15, D72.

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\*We received valuable comments from Daron Acemoglu, Ciccio Amodio, Pietro Biroli, Enrico Cantoni, Matteo Cervellati, Decio Coviello, Rafael Di Tella, Stefano Gagliarducci, Luigi Guiso, Michael Kremer, Guilherme Lichand, Monica Martinez-Bravo, Ben Marx, Claudio Michelacci, Joel Mokyr, Elias Papaioannou, Thomas Piketty, Giacomo Ponzetto, Joachim Voth and participants at presentations held at the 2019 RIDGE Political Economy Workshop, the 2019 Barcelona Summer Forum, the UZH Workshop in Political Economy and Development, the 2019 ZEW Public Finance Conference, the 2018 Swiss Development Economics Network conference, and at EIEF, IADB, IMT, McGill, Milano Labor Lunch Seminars, Northwestern, PSE, SSE, Sussex, Tor Vergata, U Bologna, U Bozen, U Maryland, UPF, U Wien and U Zurich. Lorenzo Maria Casale, Lorena Mita and Giulia Tozzi provided excellent research assistance. We also thank: Massimiliano Baragona (Italian Ministry of the Interior), Salvatore Mura (*Archivio Segni*), Eleonora Cesareo (ALSIA archive), Gabriella Bonini (*Archivio Emilio Sereni*), the Senate Library “Giovanni Spadolini,” *Archivio DC* and *Archivio di Stato*. Michela Giorcelli and Nicola Bianchi shared data on *Piano Marshall*: thanks! Bruno Caprettini acknowledges financial support from the Swiss National Science Foundation through the SNF Ambizione (grant PZ00P1\_173998). The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

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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, more recently, West Bengal (Left Front). How these systems emerge and persist is a central question in political economy. Inequality is often considered an important determinant of clientelism (e.g. Robinson and Verdier, 2013). By reducing inequality, redistribution policies may therefore undermine these systems. However, 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 clientelist systems.

In this paper, we study how voting and clientelism respond to a major redistribution policy, the 1950 Italian land reform. The reform expropriated large landowners and redistributed their land to poor rural workers. The Christian Democratic (*DC*) government implemented the land reform and only targeted well-defined reform areas. This feature allows us to adopt a panel spatial regression discontinuity design (RDD) and study differential changes in outcomes between treated and control towns close to the reform areas’ borders.

DC engaged in widespread clientelism throughout its government (1946-1992); we provide evidence that redistribution policies contributed to the emergence of such a pervasive clientelist system. As in other clientelist systems around the world, DC relied on networks of political brokers to mobilize voters (Allum, 1974) and exploited public sector employment to maintain consensus through patronage (Chubb, 1982). DC politicians also engaged in other forms of distributive politics such as pork-barrel spending (Golden and Picci, 2008). For all these reasons, DC rule is often taken as archetypical of clientelist systems (see e.g. Hicken, 2011; Piattoni, 2001). Results from our quasi-experimental design show that the land reform increased the prevalence of clientelist practices in treated towns, including political brokers, patronage and targeted benefits.

Within this context, the land redistribution generated electoral gains for DC, which lasted for four decades after the reform. While many arguments may contribute to these electoral effects—including retrospective voting and voter’s gratitude—the long-lasting clientelist practices may explain the persistence of the electoral impact in spite of profound economic and social changes in the second half of the XX century (e.g. many of the original reform beneficiaries moved out of agriculture and died by the end of our sample).

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

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<sup>1</sup>We 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. lack of manipulation and parallel pre-trends at the border) are more likely to hold there. Section 1 elaborates.

voting patterns and clientelist practices were balanced in levels and were on similar pre-trends in treatment and control towns around the reform border.<sup>2</sup> 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 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.

Next, the panel spatial RDD shows that DC experiences long-lasting electoral gains from the reform. In the first election after the reform, treated towns increase support for DC by four percentage points (control mean: 35%). DC gains are mirrored by the electoral losses of the Communist Party (*PCI*). The electoral effect of the reform then remains remarkably stable for 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. This provides evidence that land redistribution in 1950 transformed reform towns into DC strongholds for four decades, and it motivates us to study the sources of this persistent electoral effect.

The land allocation process likely contributed to the initial electoral effects: this creates similarities and differences with other episodes of clientelism. Archival evidence suggests that political considerations affected the selection of land beneficiaries. The way DC used access to land to promote clientelist exchange is reminiscent of rural clientelism in Chile (Baland and Robinson, 2008) and India (Anderson et al., 2015), where landed élites exploit land control to influence rural voters. In Mexico, Larreguy et al. (2018) find that assigning land property rights decreases the vote share of incumbent parties in local elections, arguably because it reduces the control over clients who occupy land illegally. In contrast to these experiences, when DC transferred land ownership to rural workers, it initiated a clientelist exchange. In the following decades DC maintained control over beneficiaries without controlling directly the land, and relying instead on other common clientelist practices that may have perpetuated farmers' vulnerability (Bobonis et al., 2017).<sup>3</sup>

Voters' held long-term memory of the benefits of the land reform: we find evidence of vivid recollections of the reform many years later (e.g. Nigrelli and Bonini, 2017; Zucco et al., 2011; Bonini, 2012). The land allocation and the gratitude for the reform may have thus constituted the initial ingredients for the sustained clientelist exchange that followed, in line with the recent literature on clientelism and reciprocity (Finan and Schechter, 2012).

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<sup>2</sup>Before the reform we observe vote shares, public sector employment (patronage: both levels and pre-trends) and public transfers (pork: levels), while we have no information on *Coldiretti* (brokers).

<sup>3</sup>Important 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.

Several pieces of evidence indicate that DC clientelism became stronger in treated towns after the land reform. 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 (Crainz, 1982; Lanza, 1991). Using two newly digitized datasets, we find that in treated towns *Coldiretti* had stronger support among farmers. Thanks to a 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. Greater presence of DC brokers in treated towns may have facilitated the clientelist exchange with the party.

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., 2001). These effects are particularly large in the 1980s, when DC clientelist system became rampant (Allum, 1997). Thus, as treated towns reliably supported DC election after election, DC continued to dispense benefits to these municipalities, well after the direct effects of the land reform were over. This provides additional support to the existence of political exchange between reform towns and DC governments.

Additionally, after the reform, but not during it, treated towns received more “pork” in the form of discretionary transfers from the central government. While pork spending is not always associated with clientelism (cf. Golden and Min, 2013), in the case of Italy it represents one of the well-known ingredients of DC political exchange (Golden and Picci, 2008). This result thus reinforces the idea that treated towns were favored by DC politicians once they aligned with the party. Taken together, 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). Thus, a policy that reduced inequality in the 1950s laid the foundation for a long-lasting clientelist system, arguably because it generated reciprocity among its beneficiaries.

These results are 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, and inference approaches. We also examine alternative explanations that may have contributed to the

persistent political gains of DC, including migration, voters' beliefs, economic growth and development. The data provide little support for these other channels.

Credibly identifying the political impact of redistribution policies is challenging and there is little work over their *long-run* electoral effects. Existing empirical work mostly looks at electoral impact of various fiscal transfer programs over few election cycles (see, e.g. Bechtel and Hainmueller (2011) on disaster relief and Manacorda et al., 2011, Zucco Jr, 2013 on conditional cash transfers). 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).<sup>4</sup> 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.

We show that large-scale land redistribution induced gratitude among beneficiaries and represented the original ingredient that initiated a system of clientelist exchange between the major ruling party and voters. Our results, based on the long-term impact of a policy reform, are in line with Finan and Schechter (2012), who use survey data on vote-buying and an experiment-based measure of reciprocity to look at the relation between reciprocity and clientelism. The clientelist exchange we document emerged in spite of the fact that the reform reduced inequality.

## 1 Background

### 1.1 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

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<sup>4</sup>An 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.

to prevent a Communist coalition between rural and urban workers (Segni, 1952).<sup>5</sup>

**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 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 text of 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 consultants, who advised on the type of investments necessary in different parts of the country. Detailed reports used to prepare this map confirm the technical nature of the process (Negri, 1949; Canevari, 1949; Servizio Statistica UNSE, 1949).

The *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.<sup>6</sup> 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 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.<sup>7</sup>

Some of the reform borders differed significantly from the original *legge di riforma agraria* (Ruini, 1951; Ministry of Agriculture’s Spokesman, 1951)<sup>8</sup>. 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

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<sup>5</sup>Cf. Acemoglu and Robinson (2000) for a model where *élites* extend the franchise to avoid a revolution.

<sup>6</sup>The *legge stralcio* was enacted as law 249 of 28<sup>th</sup> 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 12<sup>th</sup> May 1950, and it was later superseded by the *legge stralcio*.

<sup>7</sup>The areas were: Delta Padano (North East), Maremma (Center-West), Fúicino (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.

<sup>8</sup>The 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).

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 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 in the original plan: out of the proposed 1.4 million hectares in table 3, 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.<sup>9</sup>

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 3 support this preliminary conclusion. In addition, to mitigate concerns of manipulation of the reform borders due to land occupations or other reasons, Section 4.4 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).

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<sup>9</sup>These 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 suggest 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).

Land owners received compensation for the propriety lost in the form of 25-years fixed-rate government bonds yielding 5% a year.<sup>10</sup> 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 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% (Baldocchi, 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 of 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 (Baldocchi, 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 it 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

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<sup>10</sup>Landowners opposed form and amount of compensation (Capua, 1950; Pecoraro, 1950).

included land reclamation, irrigation, roads and housing for the new landowners. Reform 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.

## 1.2 Fifty years of DC rule

**DC Governments.** 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). DC clientelist system emerged after WWII, as Fascists governed Italy in 1922-43, and DC leaders had no connection with Fascism. 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 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 land owners), but also its actual penetration among small landowners. Back-of-the-envelope calculations discussed in Section 4.3 indicate that farmers who received land through the reform were 71 percent more likely to support *Coldiretti* than pre-existing small land owners 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.<sup>11</sup> 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, farmers had to interact with local

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<sup>11</sup>Farmers without access to *Cassa Mutue* had to pay doctors out of pocket or, when too poor, had basic medical expenses covered by the municipality.

*Coldiretti* leaders to access healthcare and the association used this influence to mobilize voters during elections (Luzzi, 2004; Lanza, 1991). In Section 4.3 we examine *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 4.3 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 4.3 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* run 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 4.2.

## 2 Data Sources

We combine several town-level datasets, including a number of 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 list of 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’Unitá*).

***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, which 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 income 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.<sup>12</sup> We normalize transfers by 1951 census population.

***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 Giorelli (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 2001 are from decadal population and economic censuses, including the newly digitized population census of 1936 and population and economic censuses of 1951 and 1961. Information on income 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 distance to the coast, to Rome and to World War II “Gothic line” based on the 1951 map of towns. Potential yield of wheat and maize is from FAO (2015) and elevation and slope from USGS (2005).

### 3 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.

#### 3.1 Panel Spatial Regression Discontinuity

Evaluating the impact of redistribution policies, including their electoral impact, typically faces major identification challenges. Politicians may target areas where they have stronger support or where they expect the returns from redistribution to be higher: this would lead to an upward bias of the estimates. Alternatively, they may target areas where they have lower or fading political support, leading to downward bias.

The design of the Italian land redistribution program offers an opportunity to overcome these 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

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<sup>12</sup>Budget information for later years is recorded only by province and by category of town size.

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).

In our preferred specification, we restrict the sample to towns that are located within 25 km from the reform border. In addition, we perform robustness specifications at alternative bandwidths of 10 km and 50 km.<sup>13</sup> Figure 1-Panel B shows 25 km buffers inside (dark red) and outside (orange) the reform areas we consider in the analysis.<sup>14</sup> These buffers include 490 towns. Our empirical strategy is based on the following panel RDD equation:

$$y_{irt} = \sum_t \alpha_t^0 \cdot d_i + \sum_t \alpha_t^1 \cdot d_i \times T_i + \sum_t \beta_t \cdot T_i + \eta_i + \eta_{rt} + u_{irt} \quad (1)$$

where  $y_{irt}$  is an outcome in town  $i$ , reform area  $r$ , election year  $t$ ;  $d_i$  is the distance of town  $i$  to the closest reform border (our running variable) and  $T_i$  is a dummy equal to one for towns included in the reform. The parameters  $\alpha_t^0$  and  $\alpha_t^1$  are election year-specific coefficients on the distance from the border and its interaction with the treatment. Our parameters of interest are the  $\beta_t$  year-specific treatment coefficients. The model also includes town fixed effects,  $\eta_i$ , and reform area  $\times$  year fixed effects:  $\eta_{rt}$ . We cluster standard errors by town. Section 4.4 presents alternative specifications and inference approaches.

Our empirical strategy identifies the causal effect of the land reform under three assumptions. First, parallel trends *at the reform border*. Second, no other contemporary policy should affect treated and control towns differentially at the border. Third, 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 provide evidence in support of these three assumptions in Section 3.2.

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 a number of endogenous variables that would compromise identification (e.g., the number of expropriable farms in each town may be correlated with many other town-level characteristics).

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<sup>13</sup>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.

<sup>14</sup>Section 1 suggests that our empirical strategy is not suitable in the South. Appendix Table D.1, shows that key identification assumptions fail in the South. In particular, pre-trends in vote shares at the border are not parallel.

## 3.2 Testing the Identification Assumptions

In this section, we test the identification assumptions of our empirical strategy. In Section 3.2.1, we look at balance at the border for a number of covariates. While our strategy does not require balance in levels (because town fixed effects capture time-invariant differences), showing that towns on the two sides of the border were similar at the time of the reform provides initial support to our approach. Section 3.2.2 examines pre-trends at the border. Section 3.2.3 discusses other contemporary policies. We address spillovers on control towns in Section 3.2.4 and then more extensively in Appendix C.8. Section 3.2.5 takes stock.

### 3.2.1 Balance at the Border

To test the balance of observables at the border, we estimate the following RDD model:

$$y_{ir} = \alpha^0 d_i + \alpha^1 \cdot d_i \times T_i + \beta \cdot T_i + \eta_r + \epsilon_{ir} \quad (2)$$

**Land Distribution: 1948.** We first estimate Equation (2) using the share of expropriable estates as dependent variable. For this purpose, we digitized administrative data from Medici (1948) and used the criteria that identified which estates were eligible for expropriation to define variables of interest.<sup>15</sup>

Table 1-Panel A shows that the share of expropriable estates is continuous at the border: in our preferred specification with 25 km bandwidth, the treatment coefficient is 0.002 (s.e.=0.01), from a control mean of 0.028. Results with 10 km and 50 km bandwidths are similar.

**Vote Shares: 1946 and 1948.** Table 1-Panel B presents result on vote shares for DC and PCI in the 1946 and 1948 elections.<sup>16</sup> Baseline vote shares are continuous at the border. The average DC vote share in control towns was 0.31 in 1946 and 0.43 in 1948. The RDD coefficients on DC vote shares are -0.025 in 1946 (s.e.=0.025) and -0.028 in 1948 (s.e.=0.028). Figure 2-Panel A presents a bin scatter with graphical evidence of continuity at the border. Similarly, the average PCI vote share in control towns was 0.24 in 1946 and 0.41 in 1948. The RDD coefficients on PCI are 0.021 in 1946 (s.e.=0.031) and 0.035 in 1948 (s.e.=0.034).

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<sup>15</sup>As explained in Section 2, 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). Appendix Table C.1-Panel A shows balance using alternative thresholds to define high-value land estates.

<sup>16</sup>In 1948 PCI run together with the Socialist Party (PSI) and other smaller parties in the Popular Democratic Front (FDP). Because we cannot separate votes for PCI and PSI, in 1948 we look at FDP vote shares. Correlation between vote shares of FDP and PCI is 0.85 (0.8) between 1946-48 (1948-53).

Figure 2-Panel D shows the bin scatter for PCI vote share and provides graphical evidence of continuity at the border. Appendix Table C.1-Panel B, which presents results from the analysis of the newly-digitized database of town mayors, also shows that mayor’s political affiliation at the time of the reform is balanced at the border. Finally, Table C.1-Panel C, shows that the reform border is uncorrelated to the “Gothic line,” a line separating US and Germans troops during WWII that had persistent effect on voting (Fontana et al., 2018).

**Other Variables.** Table 1-Panel C presents results on a number of geographic and economic variables at the time of the reform. All of these variables are continuous at the border. We highlight two results. First, potential yields of the two major crops of these areas (wheat and maize) and the share of workers in the agricultural sector are balanced at the border, suggesting that productivity or the number of potential beneficiaries did not drive the definition of the border. Second, both 1951 public sector employment and 1952 public transfers are similar at the border. This suggests that at the time of redistribution these clientelist practices had similar incidence in treated and control towns.<sup>17</sup>

Finally, we cannot check balance on the local strength of *Coldiretti* before the reform because, to the best of our knowledge, town-level data on *Coldiretti* membership do not exist.<sup>18</sup> Three other observations suggest balance of *Coldiretti* before the reform. First, the last two rows of Appendix Table C.1-Panel A show that small landowners are balanced in 1929. Thus, reform towns did not have more potential *Coldiretti* members before 1951. Second, the balance in DC vote shares in 1946-48 is reassuring: because *Coldiretti* was very effective at promoting DC candidates, balanced DC vote shares speak against greater activity of the association in treated towns before 1951. Finally, the strongest tool that *Coldiretti* had to influence its members were the *Casse Mutue* which were established only after the reform, in 1954. To summarize, differential presence of *Coldiretti* in treated towns before 1951 is both unlikely to exist and unlikely to explain the post-reform results.

**McCrary Test.** Appendix Figure C.1-Panel A presents the results of a McCrary test on the density of observations (i.e. towns) at the border. The figure shows a discontinuous drop in the number of towns inside the reform areas ( $t$ -statistic=-2.07). While this result may generate concerns of manipulation, we believe that the geometry of the land reform drives this pattern. Since reform areas are clusters of contiguous towns, and since on average these clusters are convex sets (see Figure 1), we conjecture that there will be a mechanical increase

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<sup>17</sup>Additionally, Appendix Table C.1-Panel D shows that treated and control towns at the border are similar in terms of average firm size, firms per capita and the Herfindhal-Hirschmann index of sectoral diversification.

<sup>18</sup>Lanza observes that only aggregate data on *Coldiretti* membership are available, and that these data are not reliable (Lanza, 1991 p.58, footnote 36). In Section 4.3 we measure the presence of *Coldiretti* at town level indirectly, using the results of the elections of *Casse Mutue*.

in the number of towns right outside the border. Appendix Figure C.1-Panel B provides an intuition of this argument. In the spirit of randomized inference, we validate this intuition by re-estimating the McCrary test on a number of fictitious reform areas. Appendix C.2 presents these simulations: the results support our conjecture, and 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.<sup>19</sup>

### 3.2.2 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.

**Vote Shares: 1946-1948.** We first present estimates of Equation (2) when the outcome variable is the change in vote shares of DC and PCI from the 1946 elections to the 1948 elections. Table 1-Panel D supports our empirical approach. We find parallel pre-trends between treatment and control towns at the border: the coefficient on the DC pre-trend is -0.03 (s.e.=0.02), from a control mean of 0.12.<sup>20</sup> Figure 2-Panel B presents a bin scatter and shows graphically the continuity at the border. The coefficient on PCI pre-trend is 0.04 (s.e.=0.03), from a control mean of 0.17. Figure 2-Panel E shows the bin scatter for this variable and confirms its continuity at the border.

**Pre-Fascism Elections.** One concern is that only two elections took place before the land reform and after World War II. Appendix Figure C.2 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 normalize the 1948 coefficient to zero). Parallel trends hold for the Italian Popular Party (PPI), the Catholic Party to which most DC founders belonged before the war (Appendix Figure C.2-Panel A). We also 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 his leaders, Giacomo Matteotti, killed by fascist hit men in 1924.<sup>21</sup> PCI was relatively small before the war: founded in 1921, it collected 4.6% of votes that year and 3.7% in 1924. PSI vote shares exhibit parallel pretrends

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<sup>19</sup>Another indication of the “average convexity” of the reform area is that within 25 km bandwidth, the total control area is more than 50% larger than the total treated area (roughly 18’000 vs 12’000 km<sup>2</sup>).

<sup>20</sup>Appendix Table C.1-Panel E shows parallel pre-trends for *all* other major parties.

<sup>21</sup>PSI lost ground to PCI after 1947, when it split into 2 parties. One of these parties ran together with 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 Table D.2).

(Appendix Figure C.2-Panel B). PCI vote shares seem to grow slightly faster in treatment towns, although pre-trends coefficients are not significant (Appendix Figure C.2-Panel C).

**Census Variables.** We digitized town-level data from the 1936 and 1951 population censuses. Table 1-Panel E presents estimates of Equation (2) on the 1936-51 changes of these variables. Overall, we observe parallel pre-trends both in population and employment variables. Importantly, public sector employment evolves similarly in treated and control towns before the reform, suggesting that patronage was not growing faster in reform towns. 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.

### 3.2.3 Contemporary Policies

Could 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 *Cassa del Mezzogiorno* (1950-84) and the *Piano Casa* (1949-63). In those years, Italian governments also signed the General Agreement on Tariffs and Trade (1950) and joined the European Coal and Steel Community (1951). Crucially for our identification, none of these policies targeted exactly the same areas included in the land reform. We demonstrate this in Appendix Table C.2, where we estimate Equation (2) using measures of intensity of these programs as dependent variables. We find that none of these policies affected discontinuously treated towns at the border. Moreover, because reform areas did not overlap for the most part with any other administrative unit, any shock that was specific to one of these units would affect both treated and control towns, and it would not compromise our identification.

### 3.2.4 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.<sup>22</sup>

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<sup>22</sup>The 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

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 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.8 presents the analysis in detail. Each of these tests find little evidence of resentment.

### 3.2.5 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 similar to those just outside. In short, we find no evidence of differential pre-existing exchange between voters and politicians at the border. Finally, we find no evidence that contemporary policies affected differentially treated and control towns at the border.

## 4 The Effects of the Reform

The section presents the main results of the paper. As a preliminary step, we show that the reform did affect land distribution. We then show the electoral effect of the reform, which persisted for four decades. Next, we document the emergence of clientelist practices in reform towns, focusing on political brokers and patronage via public sector employment. Finally, we address threats to identification and describe a number of robustness checks.

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beneficiaries were resident of treated towns (cf. Section 1). Section 5.1 discusses migration in depth.

## 4.1 The Effect of the Reform on Farm Ownership and Tenancy

As a preliminary result, we show that the land reform impacted land distribution in treated areas. The 1961 agricultural census reports how many farms and how many hectares are managed directly by the 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).

Table 2 shows the effect of the reform on the share of farms (Panel A) and on the share of land managed by the owner (Panel B), using the RDD model in Equation 2. Columns 1, 3 and 5 report estimates using 25 km, 10 km, and 50 km bandwidths, respectively. Treated towns have on average 10 to 11 percentage points (p.p.) more owner-operated farms, from a control average of about 70 percent. The effect is significant and stable across bandwidths. Similarly, in treated towns, the share of land in owner-operated farms is 11-13 p.p. higher than in control towns, from a control average of 41-47 percent. Both sets of results are robust when we control for the same variable in 1929 (columns 2, 4 and 6) which was balanced between treatment and control (see C.1-Panel A). Taken together, these results point to a strong and significant impact of the reform on farm ownership.

## 4.2 The Electoral Effect of the Reform

We now present the electoral effect of the land reform.

***Preliminary Graphical Evidence.*** We start with graphical evidence of the discontinuity at the border. Figure 2-Panel 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. The graph highlights that in 1953-1992 elections, relatively to control towns, DC experienced an increase of 4 p.p.<sup>23</sup>

Figure 2-Panel F repeats the exercise for PCI. On average, in elections occurring after the reform, treated towns reduced votes for PCI by about 3.5 p.p. relative to control towns. Appendix Figure C.3 show these effects on a map of Italy: they confirm that towns inside of the border supported more DC and less PCI after the reform.

***Panel RD Estimates.*** Next, we estimate Equation (1) for elections 1946-92. Figure 3-Panel A reports point estimates and 95% confidence intervals for  $\beta_t$ : the effect of the land reform on DC vote shares in every election. 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 coefficient in the 1946 election indicates that treated and control

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<sup>23</sup>As the graph shows, DC's vote share falls in 1953-92 compared to 1946-48.

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 4 p.p. during the first election after the land reform, from a control mean of 36.4%: an 11 percent increase. The effect is large and precisely estimated.

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 27<sup>th</sup> January 1953, and Reform Boards assigned land immediately after (Russo, 1955). Thus, when on the 7<sup>th</sup> 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 a number of 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 gain remains large and stable until 1992 (Figure 3-Panel A; in 1953-92, the average DC vote share in control towns ranges between 29% and 36%). During these years, 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 contrast with the short-term electoral impact of other redistribution policies (Bechtel and Hainmueller, 2011).

DC gains match almost one-to-one PCI losses (Figure 3-Panel B). Between 1953 and 1992, PCI received about 3.5 p.p. fewer votes in treated towns relative to control towns on the other side of the border (over this period, PCI vote share in control towns ranges between 29% and 40%). The reform did not affect votes for the other five major Italian parties (Appendix Table D.2) nor voters’ turnout (Appendix Figure D.1).<sup>24</sup>

Table 3-Panel A presents results from the panel RDD estimation where we pool together all the post-reform elections until 1992. Table 3-Panel B presents regression estimates by decade. Column 1 (4) looks at the effect of the reform on DC (PCI) vote share. The other columns show results with alternative bandwidths of 10 km (columns 2 and 5) and 50 km (columns 3 and 6). Results are qualitatively similar across specifications, though in the case

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<sup>24</sup>1992 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).

of PCI they are less precise with the 10 km bandwidth.

**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 voting for DC.<sup>25</sup> We find a persuasion rate of 0.64: out of three people who received land and were not already DC voters, two started voting for DC as a result of the reform. 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.2 shows that repeal received 2.6 p.p. more preferences in treated towns, from an average of 36% in control towns. The effect is quantitatively sizable and marginally significant (p-value=0.11). Appendix Table D.3 shows that the effect of the reform on referendum voting to repeal divorce is positive at alternative bandwidths, and stronger and more precisely estimated at 10 Km. Overall, these results suggest that treated towns did not vote with DC only out of economic interest.<sup>26</sup> Section 5.2 elaborates.

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<sup>25</sup>For 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 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:  $dc^T - dc^C = 0.04$ , the effect of the reform on DC vote share;  $dc^0 = 0.43$  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.

<sup>26</sup>Divorce was still rare in 1981 (0.25% of couples) and divorces are continuous at the border (Appendix

***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 was able to form a government. In Figure 3-Panel A we aggregate the votes of post-1993 DC parties and plot the treatment effect in blue. We do not find greater support for Christian Democrat parties in reform areas in 1994-2001. The dramatic change to the Italian political system after 1992 invites caution when interpreting these results.<sup>27</sup> Nevertheless, this result is consistent with the unraveling of the clientelist system, once DC lost access to the government and was unable to credibly commit to transfer public resources to its supporters.

***Memory.*** What drives DC long-term electoral gains? Long-term memory is one plausible channel: voters may remember some of the benefits of the reform for a long time and, as a result, continue supporting DC for several decades. Indeed, 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). While existing work documents the temporary nature of electoral benefits of cash transfers (e.g. disaster relief or conditional cash transfers), the effect of land transfers may be more persistent.<sup>28</sup> Memory combined with gratitude and reciprocity may thus have created the necessary pre-conditions for DC clientelist system to emerge. We now provide systematic evidence of the emergence of such a system.

### 4.3 The Effect of the Reform on Clientelist Practices

We study the impact of the reform on two distinctive features of a clientelist system: political brokers and patronage (cf. 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). We also provide evidence of increased pork-barrel spending. The increase in clientelist practices and pork-barrel may 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 reform beneficiaries moved out of agriculture and died by the end of our sample).

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Table D.4).

<sup>27</sup>For 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.

<sup>28</sup>Evidence from Germany (Bechtel and Hainmueller, 2011), Brazil (Zucco Jr, 2013), Mexico (Díaz-Cayeros, 2009), the United States (Achen and Bartels, 2004) as well as cross-country analysis (Duch and Stevenson, 2006) indicates that voters forget quickly the politicians who implemented successful redistribution policies.

**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. *Coldiretti* held significant sway among its members because it provided several important services including credit and access to markets (see Section 1).

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 votes. Lanza (1991) estimates that in 1955 and 1958 *Coldiretti* effectively controlled around 97% of *Casse Mutue*. To understand the role of *Coldiretti*, we digitized new town-level data on *Casse Mutue* budgets and board elections.<sup>29</sup>

Table 4 presents spatial RDD estimates. We find two results. First, Panels A and B show results on the 1965 *Casse Mutue* budgets. We observe an increase of 30-40% in revenues and expenses per inhabitant. These results suggest that, by increasing the number of small farm owners, the reform increased the penetration of *Casse Mutue*’s high-quality healthcare in treated towns. Because virtually all *Casse Mutue* were controlled by *Coldiretti*, treated towns were also arguably more exposed to the influence of DC political brokers. Second, Panel C shows results on the board elections 1955-70. Consistent with the previous results, the number of voters in the board elections grows substantially (by 1.4 voters per capita, or approximately 45%), suggesting again that more people relied on the *Casse Mutue*’s service provision in reform towns. Panel D shows that approximately 80% of the new voters chose *Coldiretti*, which controlled most *Casse Mutue*.<sup>30</sup>

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).<sup>31</sup> This result suggests that the reform strengthened the role of DC brokers in reform areas.

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<sup>29</sup>We also searched for *Coldiretti* town-level membership, but, as far as we know, it does not exist.

<sup>30</sup>The reform had no significant effect on *Coldiretti* share ( $\beta=0.03$ , s.e.=0.04), already high at baseline (73%).

<sup>31</sup>In 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*.

**Public Sector Employment.** Governments' patronage in public sector employment is common in many settings (Shefter, 1977, cf. Acemoglu et al., 2011 for a theoretical 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 where reform towns experienced an increase in public sector employment.

Table 5-Panel A presents the results. 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, though the coefficients are only significant in 1961 ( $\beta = 0.007$ , s.e.=0.004, p-value=0.05) and 1981 ( $\beta = 0.02$ , s.e.=0.01, p-value=0.04). 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 0.009 (s.e.=0.005, p-value=0.11). Results are robust at different bandwidths (cols. 2-3). 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).

**Fiscal Transfers.** Transfers from central to local governments are key for public good provision and political competition. The Italian government started financing local public goods through grants to municipalities in the late forties (Giarda, 2000). Theory suggests that such transfers can respond to political incentives (Grossman, 1994): an intuition confirmed for many countries (Levitt and Snyder Jr, 1995; Brollo and Nannicini, 2012), including Italy (Alesina et al., 1995). While distributive politics (pork) is not always associated with clientelism, it was commonly used by DC to maintain consensus (Golden and Picci, 2008).

Using the panel RDD of Equation (1), we test whether reform towns receive more discretionary fiscal transfers from the central government. For this purpose, we use newly digitized data on town-level fiscal transfers from municipal budgets. Every year, each Italian town must return to the Ministry of the Interior complete budget records. ISTAT published all town budgets for the years 1952, 1955 and 1959, and discontinued the publication afterwards.

In Section 3.2.1 we showed that treated and control towns had similar level of (log) municipal transfers per capita in 1952. Table 5-Panel B also shows that the reform had no impact on transfers in 1955, around the end of the land reform, and 2 years after the first post-reform Parliament had taken office. These results suggest that treated towns did not receive transfers differentially during the implementation of the reform. In contrast, in 1959, we find a sizable and marginally significant difference between treatment and control towns, ( $\beta=0.27$ , s.e.=0.12, p-value=0.097). Results are noisier at 10 km and robust at 50 km (cols.

2-3). This provides suggestive evidence that DC rewarded treated towns with pork after the land reform was completed.<sup>32</sup>

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

## 4.4 Robustness

We have already shown that our results are robust to alternative bandwidths (Table 3, 4 and 5). Here, we show that they are also robust to a battery of additional checks.

***Alternative Specifications.*** In Appendix Table C.3, we experiment with different specifications. Panel A reports results for DC vote share, Panel B for PCI vote shares, Panel C-F for *Coldiretti*, Panel G for public sector employment and Panel H for fiscal transfers. In col. 1 we drop provincial seats from the sample (including Rome); in col. 2 we control for a 2<sup>nd</sup> order polynomial in distance interacted with decades; in col. 3 we estimate a flexible polynomial in latitude and longitude interacted with decades (as in Dell, 2010), and in col. 4-6 we include three different fixed effects  $\times$  decade: ten segments (col. 4), electoral district (col. 5) and province (col. 6). Results from these specifications are robust and similar to our baseline, with the exception of the PCI vote with the 2<sup>nd</sup> order polynomial and the fiscal transfers with the 2<sup>nd</sup> order polynomial and with the latitude-longitude polynomial.

We also split the reform border into 10 segments and assign every town to one of these segments (Appendix Figure C.4). We then estimate the effect of the reform after dropping towns close to each of these 10 segments. Figure C.5 presents the results for DC (Panel A), PCI (Panel B), *Coldiretti* (Panels C-F), public sector employment (Panels G-J) and fiscal transfers (Panel K). The coefficient of public sector employment in the last decade (1991) becomes insignificant when we drop one segment, but with this exception no specific section of the reform border seems to drive the results.

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

Reform areas that redistributed land in Maremma and Delta Padano largely followed the definition of “Zone B” spelled out in the *legge di riforma agraria*. As we discuss in Section 1, the reform areas in Maremma and Delta Padano were drawn with the inputs of agricultural technical consultants, with no evidence of strategic manipulation. Later changes in the border

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<sup>32</sup>Contrary to a large literature (e.g. Broilo and Nannicini, 2012; Martinez-Bravo, 2014), we find no evidence that local politicians helped attract pork. Appendix Figure D.3 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).

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. Results for all our outcome variables are in Appendix Table C.4-Panels A-H and confirm all our baseline results. Historical records suggest that border changes in Maremma and Delta Padano were minor and not strategic. The IV in this section confirms that these changes do not drive our results.

**Placebo Borders.** Appendix Figure C.6 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.6-Panels A-H 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.6-Panels I-P 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 inside (e.g. fiscal transfers) or 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. If anything, correcting spatial correlation increases the significance of the results of our treatment variable.

## 5 Alternative Explanations of the Electoral Effects

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 4 we showed

that the reform created persistent electoral gains for DC and documented the emergence of clientelism in reform areas, which may have contributed to the persistence of the electoral effects. In this Section we explore alternative explanations for these persistent electoral effects: changes in voters’ beliefs; economic growth and development; and mechanical persistence arising from serial correlation in voting. We find limited support for these explanations.

## 5.1 Selective Migration

The reform may impact migration from or to reform areas. Here, we first analyze population levels and population composition data and then discuss a few points suggesting that migration is unlikely to drive results.

**Total Population.** The reform may affect political outcomes through permanent changes to the population. To explore this channel, we estimate (1) with the log of the number of eligible voters as dependent variable. Appendix Figure D.4-Panel A shows the estimated  $\beta_t$ . While none of these coefficients is significant, they suggest that treatment towns experienced higher out-migration in the decades following the reform. Importantly, the coefficient is relatively small (-3%) right after the reform and grows over time (approximately -20% in 1987).

**Population Composition.** We next test for differential changes in the composition of the population in treatment areas by looking at a number of characteristics of the population from decadal censuses.<sup>33</sup> Appendix Table D.5 presents the results. The reform had no significant impact on the share of workers employed in agriculture and manufacturing (columns 1 and 2), on the labor force participation (column 3), on the share of male in the population (column 4), and on the age structure of the population (columns 5-8).<sup>34</sup> This suggests that observed out-migration was homogenous across groups, and it did not draw more people from any specific sub-population (based on observables).

**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 overall pattern uncovered in Figure D.4-Panel A suggests that differential out-migration was relatively small right after the reform, and then it grew over the following

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<sup>33</sup>When 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.

<sup>34</sup>In 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 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).

decades. In contrast, Figure 3 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, this would be the case only if out-migration was PCI-intense in the 1950s and then balanced across parties later on (otherwise one could not explain the *stable* coefficient on DC vote share across decades in spite of growing out-migration). In addition, Figure D.4-Panel B reports the  $\beta_t$  of equation (1) when the dependent variable is the log of the *absolute number* of DC votes. Importantly, the graph shows that, at least in 1953 and 1958, the *absolute* number of DC votes increased by 6 to 7 log-points ( $p$ -value = 0.03 for 1953 and 0.16 for 1958), which suggests that a sizable fraction of voters began to vote DC after the reform. A simple back-of-the-envelope exercise suggests that the increase in the number of DC votes explains *at least* 76% of the DC vote share effect.<sup>35</sup>

Third, since the number of people who applied for land was substantially larger than the number of land parcels to redistribute, the vast majority of beneficiaries were previous resident of treatment towns (Section 1). For this reason, it is unlikely that the reform spurred in-migration of DC supporters from control towns. While we cannot observe individual political preferences, the lack of changes in population composition (by employment sector, age, or gender) is consistent with this interpretation.

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 effect. Treated towns experience greater outmigration by the 1980s but this is not accompanied by a reduction in support for DC nor by less patronage.

## 5.2 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).<sup>36</sup> This phenomenon may offer another plausible explanation to the persistent electoral

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<sup>35</sup>For this exercise, we assume, consistent with Figure D.1, 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, an increase of 7.1% in the number of DC votes corresponds to an increase in the DC vote share of about 3.06 p.p., or 76% of the vote share increase in 1953 (4.04 p.p.). This number is a lower bound since we assume away any out-migration (which would lead to a decrease in the denominator of the vote share).

<sup>36</sup>Wealth 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).

effect of the reform, because beneficiaries of the land redistribution became richer than control farmers who did not receive land. Both the original beneficiaries and their children may then be more conservative than families on the other side of the border simply because they had more wealth. However, 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 two additional pieces of evidence that speak against this interpretation.

***Wealth and Income.*** First, the premise of the economic conservatism argument is that voters are richer in treated towns. We test this possibility in two ways. First, we look at relative wealth of towns at the border using the home-ownership rate, the most common form of wealth among Italian households (Rossi, 2019).

Appendix Table D.6-Panel A reports results for Equation (1) with home-ownership as dependent variable. The treatment coefficients in the five decades after the reform are small and never significant. Second, we look at income per capita in 1981, the earliest year for which this variable is available. Panel B reports estimates of Equation (2) with income per capita as dependent variable and shows that the reform did not make treated towns richer in 1981. Together, these results speak against greater wealth or income of treated towns.

***Support for Forza Italia and Other Right-wing Parties.*** Second, 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 whether DC implemented them or not, we would expect greater support for Berlusconi. 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 Table D.7-Panel A reports the treatment coefficient on *Forza Italia* vote share in the sample of towns 25 km from the border. Treatment coefficients are small and insignificant in each election year. Moreover, controlling for DC vote share in 1948 has no effect on point estimates (columns 2, 4 and 6). When we look at the entire center-right coalition, we obtain similar results (Appendix Table D.7-Panel B). This evidence is thus inconsistent with persistent right-wing attitudes in treated towns.

***Discussion.*** To sum up, economic conservatism is unlikely to explain the persistence of

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

### 5.3 Economic Growth and Development Patterns

The land reform may also have political consequences through its impact on economic growth and development.<sup>37</sup> Appendix Table D.6 showed that, contrary to other land reform programs, the reform did not affect wealth (home ownership 1961-2001) or income (in 1981). In this section, we consider a number of other development outcomes.

***Agricultural Productivity.*** First, we consider the role of agricultural productivity. While, to the best of our knowledge, town-level data 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 twentieth century (Appendix Figure D.5-Panel A). This is not what we find, as the effect of the reform is stable until 1992.

***Structural Transformation.*** Second, Appendix Table D.5 showed that the reform did not foster structural transformation: the  $\beta_t$  of agricultural labor share is small, non-significant and its sign flips across decades. Overall, while the estimates are somewhat imprecise, the secular decline of agriculture appears to be similar in treated and control towns.

***Firms Growth.*** Third, we consider firms' growth, using information from the economic census (including the newly digitized 1951 and 1961). We focus on three variables: number of plants per capita, number of workers per plant and an Herfindahl-Hirschman index of industry diversification. Appendix Table C.1 showed that these outcomes are balanced in 1951. Appendix Table D.8 reports estimates of the panel spatial RDD Equation (1) and shows that the reform had no consistent impact on any of these outcomes: the coefficients are small and mostly non-significant; the sign of the coefficients flips across decades.

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<sup>37</sup>Much 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).

*Discussion.* To summarize, analysis in this section suggests that the electoral impact of the reform is unlikely to be driven by its potential effects on economic growth and development.

## 5.4 Mechanical Persistence

Finally, 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, such mechanical persistence would predict that the effect should fade over time. Appendix Figure D.5-Panel B presents correlation in vote shares across each pair of elections in our sample, and 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%.

## 6 Conclusions

How do large redistribution policies affect clientelism? An influential strand of theoretical literature (e.g. Robinson and Verdier, 2013) suggests that greater inequality promotes clientelism and thus redistribution may reduce its incidence. At the same time, an emerging literature shows that reciprocity is crucial to sustain clientelist exchange (Finan and Schechter, 2012). If redistribution generates gratitude among beneficiaries, it may promote clientelism through this channel. In this paper, we study this question in the context of the Italian land reform, which redistributed land within well-defined reform areas. We identify the long-term causal effect of the reform with a panel spatial RDD, and compare treated towns just inside the reform border with control towns just outside of it.

We find that the party that promoted the reform, DC, gains 4 p.p. in treated towns after the reform. These electoral gains persist for over 40 years, and disappear only when DC loses access to the national government in 1994. The reform created a clientelist system characterized by political brokers, patronage and targeted benefits, well-known hallmarks of DC rule during this period (Chubb, 1982; Lanza, 1991; Golden and Picci, 2008). The analysis finds little support for several alternative explanations of the persistent political gains of DC, including migration patterns, changes in voters' beliefs, economic growth and development. Our results imply that by creating gratitude among beneficiaries, large-scale redistribution may create the pre-conditions for clientelist exchange to thrive.

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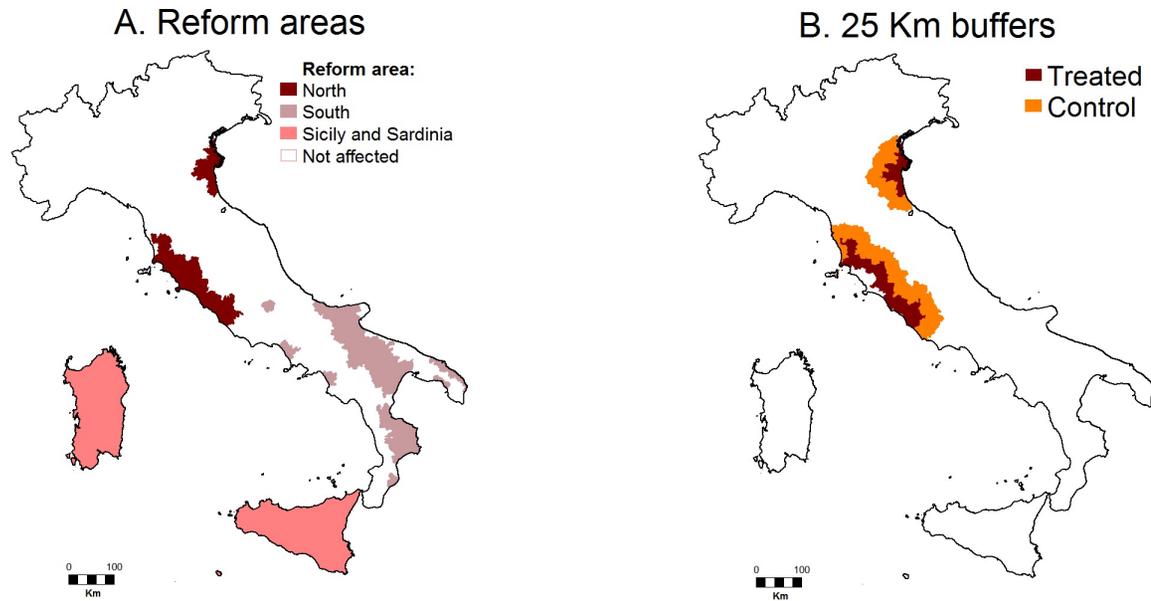
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# Figures

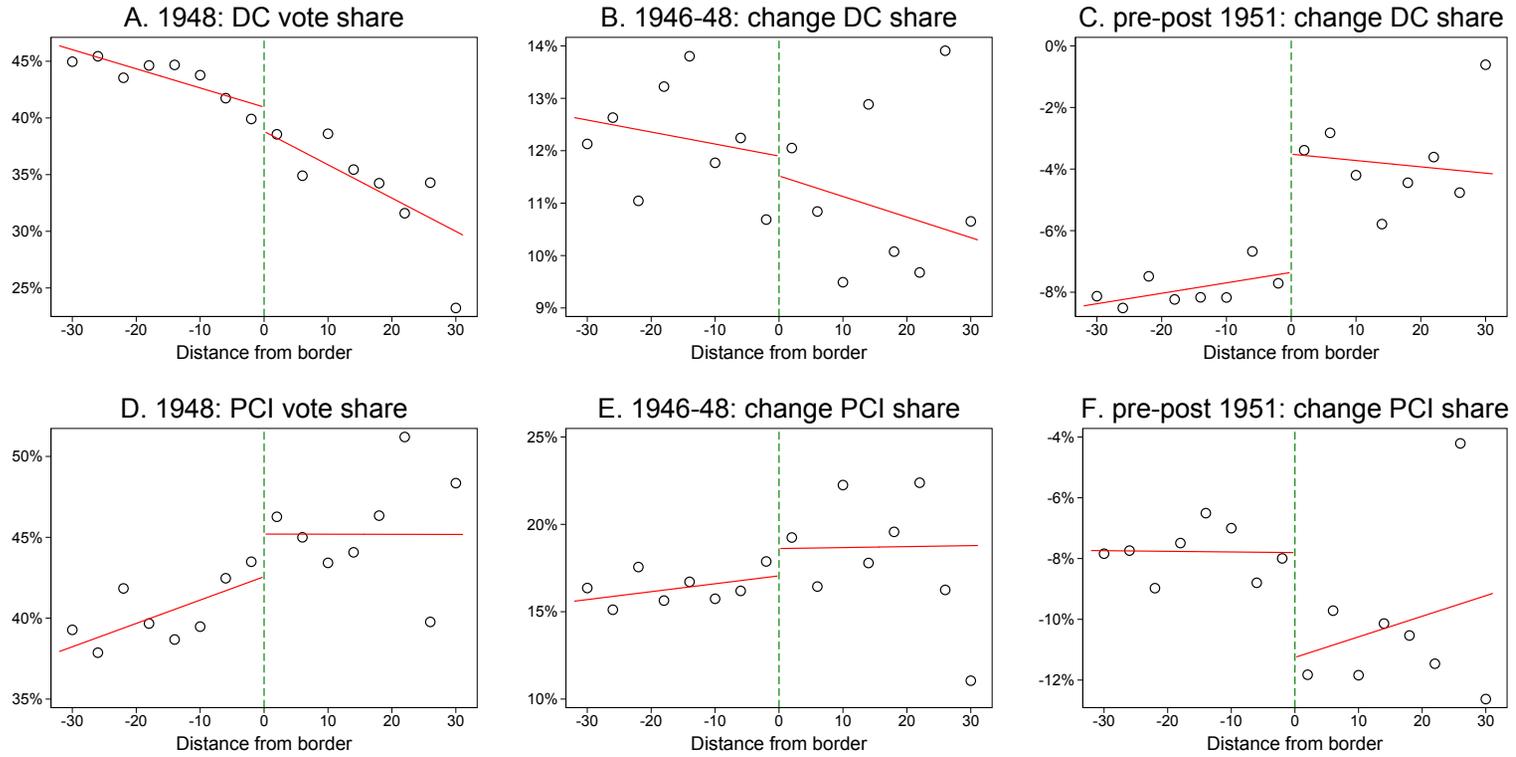
Figure 1: Reform Areas and Buffers



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*Notes:* Panel A: land reform areas as defined in the 1951 Law. 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. Panel B: 25 km buffers inside and outside the border of Delta Padano and Maremma (preferred bandwidth).

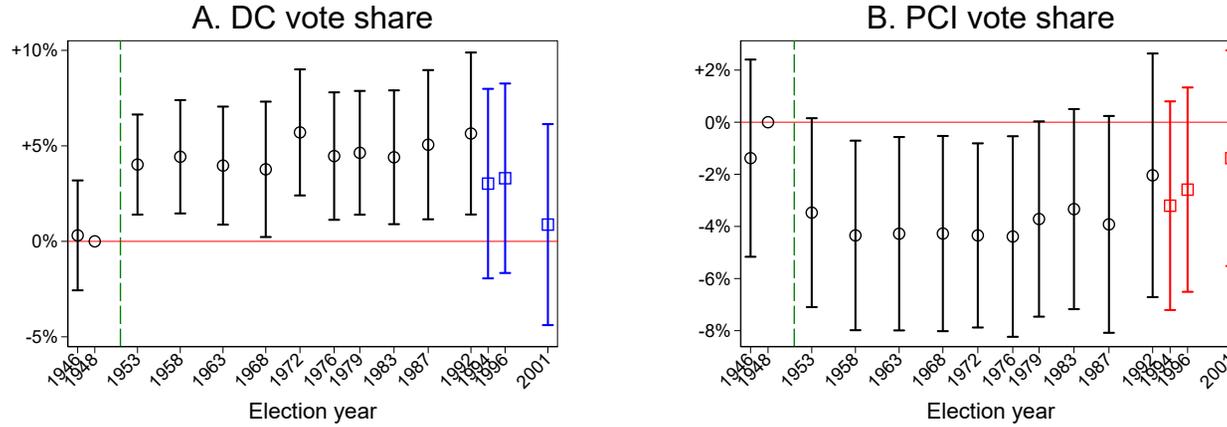
**Figure 2:** Balance, pre-trends and effect of reform: graphical evidence



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*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). Panels D-F: repeat the analysis for the Communist Party (PCI). 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 3:** The Electoral Impact of the Reform: Panel Regression Discontinuity Results



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*Notes:* The Panels display coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. The omitted category is the  $\beta$  of 1948. Panel A: dependent variable is Christian Democrat (DC) vote share. 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). Panel B: dependent variable is Communists (PCI) vote share. 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. The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The vertical lines mark the 1951 land reform. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

# Tables

	<i>Preferred Bandwidth</i> < 25km (N=490)			<i>Alternative Bandwidths</i>					
	Control mean	$\beta$	[s.e]	Control mean	$\beta$	[s.e]	Control mean	$\beta$	[s.e]
<b>A: Balance Land Distribution 1948</b>									
Share of Expropriable Estates 1948	0.028	0.002	[0.010]	0.029	-0.037	[0.024]	0.025	-0.010	[0.010]
<b>B: Balance Vote Shares 1946 &amp; 1948</b>									
Christian Democrats (DC) 1946	0.310	-0.025	[0.025]	0.295	-0.012	[0.040]	0.330	-0.010	[0.022]
Christian Democrats (DC) 1948	0.431	-0.028	[0.028]	0.411	0.019	[0.042]	0.454	-0.015	[0.024]
Communists (PCI) 1946	0.243	0.021	[0.031]	0.259	0.002	[0.052]	0.235	0.009	[0.026]
Communists (PCI) 1948	0.408	0.035	[0.034]	0.425	-0.010	[0.053]	0.387	0.019	[0.029]
<b>C: Balance Geography and Census 1951</b>									
Distance from the Coast	44.12	0.969	[2.761]	37.04	5.531	[4.282]	49.64	-4.571**	[2.269]
Distance from Rome	184.3	13.63	[13.03]	165.1	-2.344	[20.90]	226.2	10.02	[10.43]
Area (miles <sup>2</sup> )	18.03	0.337	[6.293]	21.19	-5.384	[6.735]	18.58	3.879	[5.362]
Slope	1.530	-0.020	[0.167]	1.345	0.226	[0.236]	1.575	-0.116	[0.143]
Elevation	225.4	27.67	[30.28]	203.3	27.26	[42.02]	224.9	29.30	[24.82]
Wheat Suitability	4.432	-0.046	[0.054]	4.506	-0.009	[0.085]	4.378	-0.009	[0.043]
Maize Suitability	6.193	-0.187	[0.138]	6.107	0.026	[0.223]	6.392	-0.177	[0.112]
Log Population	8.360	-0.226	[0.161]	8.438	-0.449*	[0.240]	8.454	-0.065	[0.144]
Share Active Population	0.530	-0.009	[0.013]	0.540	-0.022	[0.018]	0.523	-0.003	[0.010]
Share Agricultural Workers	0.645	0.005	[0.034]	0.669	0.025	[0.049]	0.627	0.030	[0.027]
Share Manufacturing Workers	0.144	0.019	[0.021]	0.122	-0.013	[0.029]	0.155	-0.002	[0.016]
Share Public Sector Workers	0.052	-0.010	[0.007]	0.055	-0.011	[0.008]	0.049	-0.008	[0.005]
Public transfers per capita (1952)	0.393	0.045	[0.254]	0.332	-0.149	[0.137]	0.250	-0.114	[0.103]
<b>D: Pre-Trends Vote Shares 1948-46</b>									
Christian Democrats (DC)	0.122	-0.003	[0.015]	0.116	0.031	[0.024]	0.123	-0.005	[0.012]
Communists (PCI)	0.165	0.014	[0.019]	0.166	-0.012	[0.033]	0.152	0.010	[0.016]
<b>E: Pre-Trends Census 1951-36</b>									
Log Population	0.075	-0.021	[0.023]	0.097	-0.030	[0.030]	0.065	0.008	[0.017]
Log Workers	0.053	-0.024	[0.031]	0.081	-0.083*	[0.049]	0.038	0.012	[0.025]
Share Active Population	0.080	-0.006	[0.013]	0.083	-0.028	[0.021]	0.077	0.001	[0.010]
Share Agricultural Workers	-0.068	-0.016	[0.015]	-0.067	-0.016	[0.022]	-0.082	-0.019	[0.012]
Share Manufacturing Workers	-0.026	0.011	[0.010]	-0.025	0.003	[0.014]	-0.023	0.018**	[0.008]
Share Public Sector Workers	0.025	-0.005	[0.004]	0.028	-0.004	[0.007]	0.024	-0.002	[0.004]

**Table 1: Balance and Pre-Trends at the Border.**

*Notes:* The columns beneath  $\beta$  report the coefficient of separate regressions of the RDD specification in Equation (2). Dependent variables are specified on the first column, and their average in control towns is reported in the columns beneath “Control mean”. Refer to 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. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). In Panel A, the sample is approximately 17% smaller due to missing data in Medici (1948). The columns beneath “[s.e.]” report heteroschedastic robust standard errors. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

	<i>Preferred Bandwidth</i> <i>&lt; 25km</i>		<i>Alternative Bandwidths</i> <i>&lt; 10km</i> <i>&lt; 50km</i>			
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Share of <u>Farms</u> Managed by the Farm Owner</b>						
Treatment	0.100*** [0.032]	0.100*** [0.032]	0.112** [0.052]	0.112** [0.052]	0.101*** [0.025]	0.101*** [0.025]
Mean Y Control Group	0.722	0.722	0.713	0.713	0.716	0.716
1929 share of farms managed by farm owner	No	Yes	No	Yes	No	Yes
<b>B. Share of <u>Land</u> Managed by the Farm Owner</b>						
Treatment	0.125*** [0.038]	0.125*** [0.038]	0.133** [0.062]	0.133** [0.062]	0.112*** [0.031]	0.112*** [0.031]
Mean Y Control Group	0.440	0.440	0.410	0.410	0.467	0.467
1929 share of land managed by farm owner	No	Yes	No	Yes	No	Yes
Observations	489	489	222	222	859	859

**Table 2: The impact of the reform on farm management.**

*Notes:* The Table reports coefficients  $\beta$  of separate regressions of the RDD specification in Equation (2). Panel A: dependent variable is share of *farms* managed by the farms owner in 1961. Panel B: dependent variable is the share of *land* managed by the farm owner in 1961. Columns 2, 4 and 6 control 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. The sample consists of all towns close to the reform borders of Delta Padano and Maremma. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Heteroschedastic robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

	Christian Democrats			Communist Party		
	<i>Preferred Bandwidth &lt; 25km</i>	<i>Alternative Bandwidths &lt; 10km &lt; 50km</i>		<i>Preferred Bandwidth &lt; 25km</i>	<i>Alternative Bandwidths &lt; 10km &lt; 50km</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A: Pooled Results (sample: 1946-1992)</b>						
Treatment × Post (1953-92)	0.044*** [0.015]	0.052** [0.025]	0.035 *** [0.012]	-0.031** [0.015]	-0.013 [0.026]	-0.024* [0.013]
<b>B: Results by Decade (sample: 1946-2001)</b>						
Treatment × 1950s	0.041*** [0.013]	0.045* [0.024]	0.031*** [0.011]	-0.032** [0.013]	-0.005 [0.022]	-0.020* [0.011]
Treatment × 1960s	0.037** [0.016]	0.053* [0.027]	0.028** [0.013]	-0.036** [0.015]	-0.019 [0.026]	-0.032** [0.013]
Treatment × 1970s	0.047*** [0.016]	0.061** [0.025]	0.037*** [0.013]	-0.034** [0.016]	-0.018 [0.028]	-0.028** [0.014]
Treatment × 1980s	0.048*** [0.018]	0.046 [0.028]	0.041*** [0.015]	-0.024 [0.019]	-0.008 [0.032]	-0.017 [0.015]
Treatment × 1990s	0.022 [0.023]	-0.001 [0.035]	0.005 [0.020]	-0.017 [0.018]	-0.012 [0.031]	-0.011 [0.015]
Mean Y Control Group	0.36	0.34	0.38	0.33	0.34	0.31
Number of Towns	490	222	863	490	222	863
Observations	7308	3317	12822	7308	3317	12822

**Table 3: The electoral impact of the land reform.**

*Notes:* The table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year × reform area and town fixed effects. Panel A reports a single coefficient for treated towns in the post-reform years (1953-92); the sample includes 1946-92 elections. Panel B reports separate  $\beta_t$  for each decade after the reform until the 1990s; the sample includes 1946-2001 elections. Because the DC rule ended in 1994, we assign 1992 to the 1980s and 2001 to the 1990s. In both panels the omitted category is the  $\beta$  of the elections of 1946 and 1948. Columns 1-3: dependent variable is Christian Democrat (DC) vote share. 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). Columns 4-6: dependent variable is Communist (PCI) vote share. For PCI we use the vote share of the Popular Democratic Front (FDP) in 1948. 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). Units of observation are town-years. The sample consists of all towns close to the reform borders of Delta Padano and Maremma. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Standard errors clustered at the town level in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

	<i>Preferred</i> <i>bandwidth</i> <i>&lt; 25km</i>		<i>Alternative</i> <i>bandwidths</i> <i>&lt; 10km</i> <i>&lt; 50km</i>			
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Healthcare budgets (<i>Casse Mutue</i>): State-subsidized but controlled by <i>Coldiretti</i></b>						
<b>A. Income per capita</b>						
Treatment	120.716**	120.401**	114.533	112.690	130.309***	134.273***
	[55.278]	[55.547]	[79.954]	[80.672]	[50.149]	[50.555]
Mean Y Control Group	437.79	437.79	414.26	414.26	499.05	499.05
Observations	488	488	221	221	861	861
<b>B. Expenditure per capita</b>						
Treatment	119.382**	117.862**	127.802*	131.751*	122.096***	124.524***
	[49.953]	[49.408]	[72.072]	[71.573]	[44.804]	[44.850]
Mean Y Control Group	316.00	316.00	311.89	311.89	350.88	350.88
Observations	488	488	221	221	861	861
<u><i>Casse Mutue</i> elections</u>						
<b>C. Votes per capita</b>						
Treatment	0.014**	0.013**	0.018	0.017	0.017***	0.016***
	[0.006]	[0.006]	[0.011]	[0.010]	[0.005]	[0.005]
Mean Y Control Group	0.03	0.03	0.03	0.03	0.03	0.03
Number of Towns	397	397	176	176	712	712
Observations	1451	1451	676	676	2594	2594
<b>D. <i>Coldiretti</i> votes per capita</b>						
Treatment	0.011**	0.011**	0.016*	0.015*	0.013***	0.013***
	[0.005]	[0.005]	[0.009]	[0.008]	[0.004]	[0.004]
Mean Y Control Group	0.02	0.02	0.02	0.02	0.03	0.03
Number of Towns	396	396	175	175	710	710
1929 share of farms managed by owner		Y		Y		Y
Observations	1419	1419	666	666	2519	2519

**Table 4: The impact of the reform on *Casse Mutue* elections and budgets.**

*Notes:* The Table reports  $\beta$  of RDD regressions Equation (2). Panel A and B use data from 1965 *Casse Mutue* budgets (Fed. Naz. Casse Mutue, 1966). Units of observation are towns. Panel C and D use data from 1955-70 *Casse Mutue* elections (All. Naz. dei Contadini, 1970) and have a few missing observations. We pool all years and add year  $\times$  reform area fixed effects. Units of observation are town-years. Cols. 2, 4 and 6 control for 1929 share of farm-owned farms. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Standard errors heteroschedastic robust (Panel A and B) and clustered at the town level (Panel C and D) in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

	<i>Preferred Bandwidth &lt; 25km</i>	<i>Alternative Bandwidths &lt; 10km</i>	<i>Alternative Bandwidths &lt; 50km</i>
	(1)	(2)	(3)
<b>A. Public sector employment (patronage): 1936-1991</b>			
Treatment × 1936	0.005 [0.004]	0.006 [0.007]	0.002 [0.004]
Treatment × 1961	0.007* [0.004]	0.011* [0.006]	0.009*** [0.003]
Treatment × 1971	0.005 [0.006]	0.011 [0.011]	0.007 [0.005]
Treatment × 1981	0.021*** [0.007]	0.025** [0.012]	0.021*** [0.006]
Treatment × 1991	0.012 [0.010]	0.018 [0.014]	0.013 [0.008]
Mean Y Control Group	0.05	0.06	0.05
Number of Towns	490	222	863
Observations	2939	1331	5177
<b>B. Municipal transfers per capita (pork): 1952, 1955, 1959</b>			
Treatment × 1955	0.052 [0.156]	-0.216 [0.289]	0.054 [0.122]
Treatment × 1959	0.266* [0.160]	0.089 [0.293]	0.268** [0.121]
Mean Y Control Group	1.14	1.15	1.11
Number of Towns	483	220	840
Observations	1438	659	2488

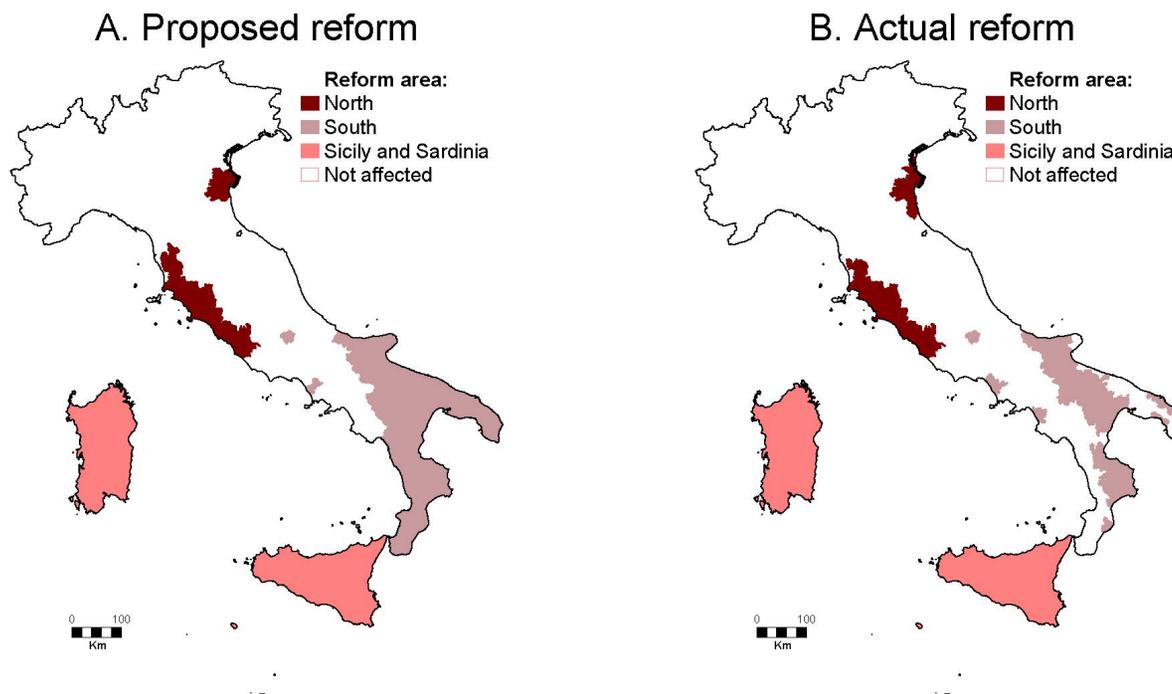
**Table 5: The impact of the reform on patronage and pork spending.**

*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year × reform area and town fixed effects. Panel A: omitted category is 1951 and dependent variable is the share of public sector employment (patronage). Panel B: omitted category is 1952 and dependent variable is log of per capita fiscal transfers from the central government to the municipal governments (pork spending). The sample consists of all towns close to the reform borders of Delta Padano and Maremma. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Standard errors clustered at the town level in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

# Appendix (For Online Publication)

## A The 1950 Italian Land Reform

Figure A.1: Actual and proposed 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

46

CAMERA DEI DEPUTATI 23/11/50

Caro Segni,  
 mi dicono che non  
 parlerai? Perché?  
 Mi fusti per un  
 minuto  
 Condulotti per  
 loro libri -  
 Non parlerò perché in effetti  
 non ho le idee; dopo un quarto  
 d'ora dovrà smettere.  
 Però, non ti mandavo che  
 una parola valentissima: Se non  
sono soddisfatto sulla sua  
"fondiaria" (fu la condizione  
del nord che nessuno toccava  
de I, non sono soddisfatto

(Panel A) Messages between Segni and Sampietro

E/34

DEMOCRAZIA CRISTIANA  
 COMITATO PROVINCIALE DI SIENA Siena, 2 Febbraio 1951  
 Telef. 20.581

La Giunta Provvisoria del Commissariato Provinciale di Siena, venuta a conoscenza che la legge Stralcio sulla Riforma Fondiaria, approvata recentemente dal Consiglio dei Ministri, comprende i soli tre Comuni della nostra Provincia di Abbadia S. Salvatore, Castiglion d'Orcia, Piancastagnolo, limitando così il precedente programma già annunciato, preparato e propagandato anche sulla stampa, secondo il quale la zona di applicazione della legge stessa si estendeva a undici Comuni di questa Provincia;

che questa limitazione, non si sa da quale motivo determinata, può far ritenere che sia dovuta a particolari influenze di elementi interessati, si da produrre una sfavorevolissima impressione sui lavoratori e braccianti agricoli che continueranno a nutrire sospetti e diffidenza verso il nostro Partito e con gravi ripercussioni politiche nella nostra Provincia dominata dal bolscevismo organizzato a sfruttare episodi di questo genere, ciò premesso

E L E V A

la sua vibrata protesta per le decisioni assunte dal Consiglio dei Ministri e declina ogni responsabilità per le conseguenze di carattere politico che potranno derivare dai fatti sopra denunciati.

*on. Elia*



(Panel B) Letter to Segni from DC Siena Committee

REPUBBLICA ITALIANA 2/n  
 Al Presidente  
 del Consiglio dei Ministri

Per Segni

Rodino mi dice che ogni  
 volta ribollono i signori  
 contro estensione compres-  
 so (Cavarzere  
 Confidonia ecc.)

Comunque ho promesso  
 incontro Segn. Segni  
 Rodino.

(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. Rodino [a marquis, president of the landowner’s association *Confida*] 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 Rodino.” 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.

Roma, / febbraio 1951

Carissimo Presidente,

ho avuto la lettera che mi hai inviato, letta quella inviata a Rinaldi, e, dal punto di vista tecnico, la risposta è qui allegata (stessa da Bandini, sentiti altri tecnici).

Non vi è dubbio che i territori di cui parla la lettera sono suscettibili di trasformazione fondiaria o agraria e ricadono nella legge; sulla sponda del Delta (Coppara, Jolanda di Savoia ecc.) sono ora iniziati a cura e spese dello Stato, opere pubbliche di irrigazione, che importano una trasformazione sostanziale; ancor più influente e decisiva è l’opera di appoderamento da fare, che riguarda tutti i territori del Delta inseriti nel comprensorio delimitato dal Comitato dei Ministri e anche oltre il comprensorio.

Tutto questo comprensorio è inoltre percorso dal Po di Volano e dal Po Morto di Primaro, diventato oggi canale di irrigazione, e quindi geograficamente fa parte del Delta Padano, che è anche più esteso, in quanto vi sono compresi altri comuni della provincia di Ferrara e di Rovigo, e qualche tratto della Bassa Modenese, non contenuti nel comprensorio stralcio.

(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, Livorno, 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**

SCAGLIONI DI REDDITO IMPONIBILE TOTALE		Imponibile medio per Ha.									
		Lire									
		1000 e oltre	900	800	700	600	500	400	300	200	100 e meno
Lire											
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	85
»	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 (in percentage).

Figure A.4: Original applications

Mod. D. A. T. **U. n. 3.60** **White Paper**

**Rosso!!!** **B** *non agricolo  
aziendale  
coop. Edile*

<b>F/M</b>	N. di categoria	<b>7333</b>
Categoria III		N. di registrazione

**22** **13**

ENTE PER LO SVILUPPO DELL'IRRIGAZIONE  
E LA TRASFORMAZIONE FONDARIA IN PUGLIA E LUCANIA

**SEZIONE SPECIALE PER LA RIFORMA FONDARIA**

Via Dante Alighieri N. 40 - B A R I - Via Dante Alighieri N. 40

*Luigi Noto  
di Pannofiore*

**OGGETTO: Domanda di assegnazione di terreni.**

Il sottoscritto *Nambrogo Nambro* di *Comino*  
e di *Della Luersa* nato a *Nambro* e residente  
in *Nambro* Via *Enrico* N. \_\_\_\_\_ avendo la qualifica  
principale di (2) *Bracciatto* 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 quota  
di terreno ai sensi degli articoli 16 e 17 della Legge 12 maggio 1950, n. 230 e dell'art. 21 della Legge 21 ottobre  
1950, n. 841.

A tale scopo, presa conoscenza delle notizie contenute nella presente scheda, dichiara che esse corrispondono a verità per cui ne assume piena responsabilità.

*Nambro*, li *6-5-1952* FIRMA DEL RICHIEDENTE

**13 FEB 1951**

ALLA SEZ. SPEC. RIF. FOND.  
Servizio Segr. ed Aff. Gen.  
**B A R I**

**MONELLI Rinaia**  
Via Filangieri 13  
**BRINDISI**

Riferimento nota n. 3459/223/Segr. A.G. del  
7/1/1951 di pari oggetto, si comunica che da accertamenti eseguiti la **MONELLI** è risultata non possedere alcun bene di fortuna.

Si fa inoltre presente che la di lei famiglia non compresa nello stato di famiglia passerebbe a nozze a giorni.

È risultato che la predetta famiglia è politicamente orientata per l'estrema sinistra.

*Si resterà in attesa e si è in attesa*

IL DIRETTORE DEL CENTRO  
(Dr. Carlo Corno)

*13*

Notes: Example of rejected applications. On the left: on the top-left corner “Rosso” identifies the applicant as “red” (i.e. communist). On the right: last sentence on the report reads: “It turns out that the above family is politically close to the extreme left”. Source: ALSIA archive. We thank Eleonora Cesareo for sharing this material with us.

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,<sup>38</sup> with a shapefile of 2001 Italian towns<sup>39</sup>. 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.<sup>40</sup> We compute the distance between the town centroid and each reform area border and assign each town 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 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.

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<sup>38</sup>We find these lists on <http://www.elesh.it>.

<sup>39</sup>From ISTAT. ISTAT provides a shapefile for 1991 towns, but ELES website does not have a 1991 list of towns.

<sup>40</sup>Nicastro, Sambiasè 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 50km 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.

***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 income 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 transfers from the central government divided by 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 dummy.** We delimit the area affected by the *Cassa del Mezzogiorno* as described in the law that created it in 1950 (L.646/1950). A small number of municipalities in the control group of our North sample are included in this area. We construct a dummy equal to 1 for municipalities included.

**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 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, 2001) and economic censuses (1951, 1961, 1971, 1981, 1991, 2001). 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: 1936-2001.*** 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), Table 2.2 (2001). The variable is the natural logarithm of total population. We made no adjustment for zeros as there were none.

***Share of active population: 1936-2001.*** 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), Table 2.5 (2001). 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: 1936-2001.*** 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), Table 2.7 (2001). 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: 1936-2001.*** 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), Table 2.7 (2001). 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: 1936-2001.*** 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), Table 2.7 (2001). The variable is number of workers employed in manufacturing divided total active population. In 1981 public sector is economic sector 9.A. In 2001 public sector combines workers in public administration and other public employees.

***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, transport, 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, transport, 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.

***Herfindahl–Hirschman Index.*** The town-level Herfindahl–Hirschman index is constructed using the town-level number of workers employed in manufacturing, extraction, construction, commerce, transport, utilities, social services, banking and insurance and computing the concentration index between these sectors using the Stata package `hhi5` written by Lian.

***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: 1951-2001.*** 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), Table 2.2 (2001). The variable is number of males divided by total population.

***Share of people in age groups (<21, 21-45; 46-65; >65): 1951-2001.*** 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), Table 2.2 (2001). The variable is population in

specific age groups divided by total population.

**Home ownership: 1951-2001.** 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), Table 2.12 (2001). 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.

**Distance to the Gothic line.** We draw the "Gothic line" using the map from Oland's map contained in North Apennines: The US Army Campaigns of World War II (1995). We compute the distance between every town's 1951 centroid and the line in ArcGIS.

**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  $462.5 \times 462.5$  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  $9.25 \times 9.25$  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  $9.25 \times 9.25$  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 Additional Balance and Pre-Trends

Table C.1: Additional Balance and Pretrends: 1946-1948.

	Preferred Bandwidth < 25km			Alternative Bandwidths					
	Control Mean	$\beta$	[s.e]	Control Mean	$\beta$	[s.e]	Control Mean	$\beta$	[s.e]
<b>A: Agricultural Variables</b>									
Share of estates worth in 1948 >£200,000	0.001	-0.000	[0.001]	0.001	-0.002	[0.002]	0.001	-0.001	[0.001]
Share of estates worth in 1948 >£100,000	0.004	-0.001	[0.002]	0.004	-0.007	[0.005]	0.003	-0.003	[0.002]
Share of estates worth in 1948 >£40,000	0.013	0.001	[0.006]	0.014	-0.021	[0.015]	0.011	-0.005	[0.006]
Share of estates worth in 1948 >£20,000	0.028	0.002	[0.010]	0.029	-0.037	[0.024]	0.025	-0.010	[0.010]
Share 1929 farms managed by farm owner	0.435	-0.054	[0.064]	0.433	0.028	[0.101]	0.446	-0.021	[0.051]
Share 1929 land managed by farm owner	0.532	-0.029	[0.057]	0.523	-0.013	[0.082]	0.539	-0.027	[0.045]
<b>B. Balance Mayor Elections 1946</b>									
DC	0.149	-0.016	[0.069]	0.155	0.079	[0.100]	0.181	0.007	[0.058]
PCI (alone)	0.041	0.002	[0.040]	0.019	0.041	[0.067]	0.044	-0.033	[0.032]
PCI (with allies)	0.428	0.038	[0.099]	0.465	0.187	[0.152]	0.439	0.004	[0.082]
PRI	0.041	0.005	[0.054]	0.032	0.070	[0.099]	0.042	0.023	[0.039]
<b>C. Balance Gothic Line and Vote Shares Other Parties 1946 &amp; 1948</b>									
Distance from WWII Gothic line	126.0	-15.41	[10.60]	129.8	-4.995	[16.26]	113.2	-14.43	[10.10]
Socialists (PSI) 1946	0.194	-0.003	[0.019]	0.189	-0.029	[0.030]	0.205	0.000	[0.016]
Socialists (PSI) 1948	0.408	0.035	[0.034]	0.425	-0.010	[0.053]	0.387	0.019	[0.029]
Social-Democrats (PSDI) 1946	0.194	-0.003	[0.019]	0.189	-0.029	[0.030]	0.205	0.000	[0.016]
Social-Democrats (PSDI) 1948	0.049	-0.003	[0.008]	0.051	0.009	[0.015]	0.058	0.005	[0.006]
Republicans (PRI) 1946	0.066	-0.011	[0.016]	0.074	-0.034	[0.028]	0.058	-0.012	[0.013]
Republicans (PRI) 1948	0.041	-0.021*	[0.012]	0.046	-0.030*	[0.017]	0.035	-0.022**	[0.009]
Liberals (PLI) 1946	0.026	0.001	[0.007]	0.025	0.010	[0.011]	0.026	-0.004	[0.005]
Liberals (PLI) 1948	0.013	0.000	[0.005]	0.016	-0.000	[0.007]	0.013	0.000	[0.004]
Post-Fascists (MSI) 1948	0.019	-0.001	[0.003]	0.015	-0.000	[0.005]	0.016	-0.005*	[0.003]
<b>D. Balance Industrial Census 1951</b>									
Workers per plant (average)	2.368	0.202	[0.296]	2.294	-0.342	[0.433]	2.448	0.217	[0.242]
Plants per capita	0.034	0.001	[0.003]	0.034	0.001	[0.006]	0.032	0.003	[0.003]
HH index of sectoral diversification	0.358	0.022	[0.015]	0.342	0.011	[0.025]	0.363	0.005	[0.012]
<b>E. Pre-Trends Vote Shares Other Parties 1948-46</b>									
Socialists (PSI)	0.215	0.038	[0.028]	0.236	0.020	[0.043]	0.182	0.019	[0.023]
Social-Democrats (PSDI)	-0.145	0.001	[0.019]	-0.138	0.038	[0.032]	-0.147	0.004	[0.016]
Republicans (PRI)	-0.025	-0.010	[0.009]	-0.028	0.004	[0.020]	-0.023	-0.010	[0.008]
Liberals (PLI)	-0.013	-0.001	[0.007]	-0.009	-0.011	[0.011]	-0.013	0.004	[0.006]

Notes: For a description of the dependent variables, refer to Appendix B. “Control mean” represents average in control towns.  $\beta$  columns report the coefficient of separate regressions of the RDD specification in Equation (2). The sample consists of all towns close to the reform borders of Delta Padano and Maremma. Estimates reported for three bandwidths (25, the preferred, 10 and 50 km). Panel A: the first four rows have a sample 17% smaller due to missing data in Medici (1948); the last two rows about 50% smaller due to missing data in ISTAT (1936). “[s.e.]” columns report heteroschedastic robust s.e. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

## C.2 Continuity of the Running Variable

This appendix presents the McCrary test and discusses why in our context a jump in the density does not need to be evidence of manipulation.

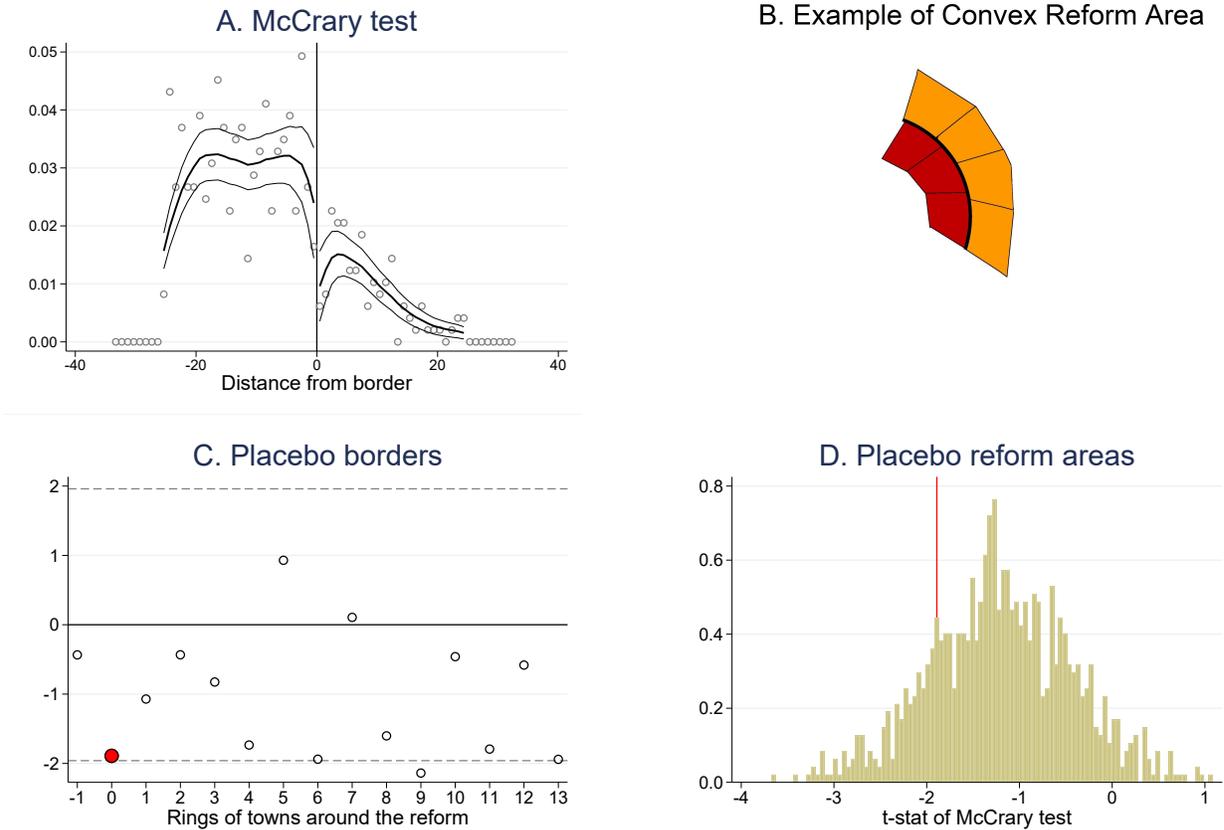
Appendix Figure C.1-Panel A presents the density approximation of the number of towns in the North (y-axis) as a function of the distance to the border (x-axis). The formal McCrary test has a  $t$ -statistic of -2.07 and rejects the null of no jump at the border. We believe that the jump in the density 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 km<sup>2</sup>). Because on average towns have the same size inside and outside the border (the RD estimate at 25 km for  $\log(\text{area})$  is 0.21 from a sample mean of 17.4;  $p = 0.21$ ), it must be the case that there are more towns outside than inside the reform area (see Appendix Figure C.1-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.1-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.1-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). The McCrary  $t$ -statistic for the towns in Maremma is -1.89, and many of the other fictitious borders have McCrary estimates close to this number.

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.1-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.

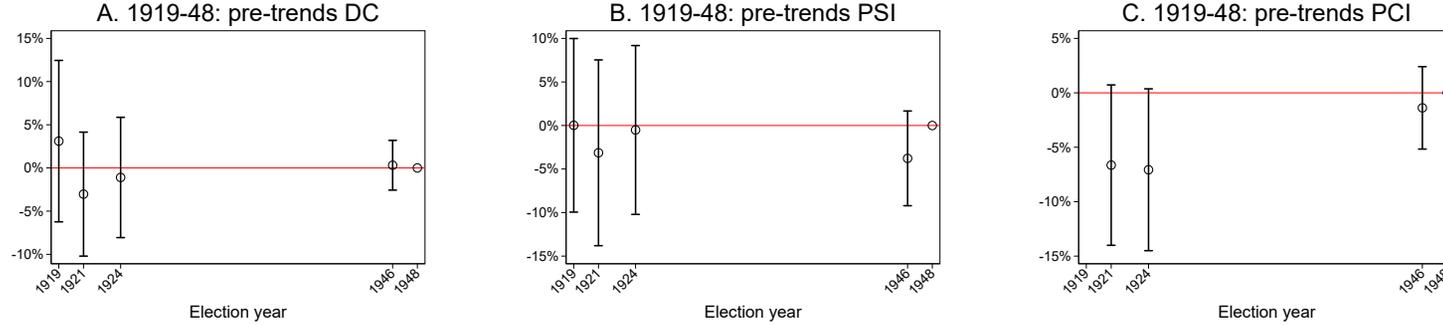
**Figure C.1:** McCrary test, Conjecture and Simulation Exercises



*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.2: Pre-Fascism Elections



*Notes:* The Panels display coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. The omitted category is the  $\beta$  of 1948. Panel A: dependent variable is DC vote share. For DC we use the vote share of the Italian Popular Party (PPI) in the years 1919-24. Panel B: dependent variable is Italian Socialist Party (PSI) vote share. For PSI we use the vote share of the Official Socialist Party (PSU) in 1921, of the United Socialist Party (PSU) in 1924 and of the Popular Democratic Front (FDP) in 1948. Panel C: dependent variable is PCI vote share. For PCI we use the vote share of the Popular Democratic Front (FDP) in 1948. 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.

## C.4 Contemporary Policies

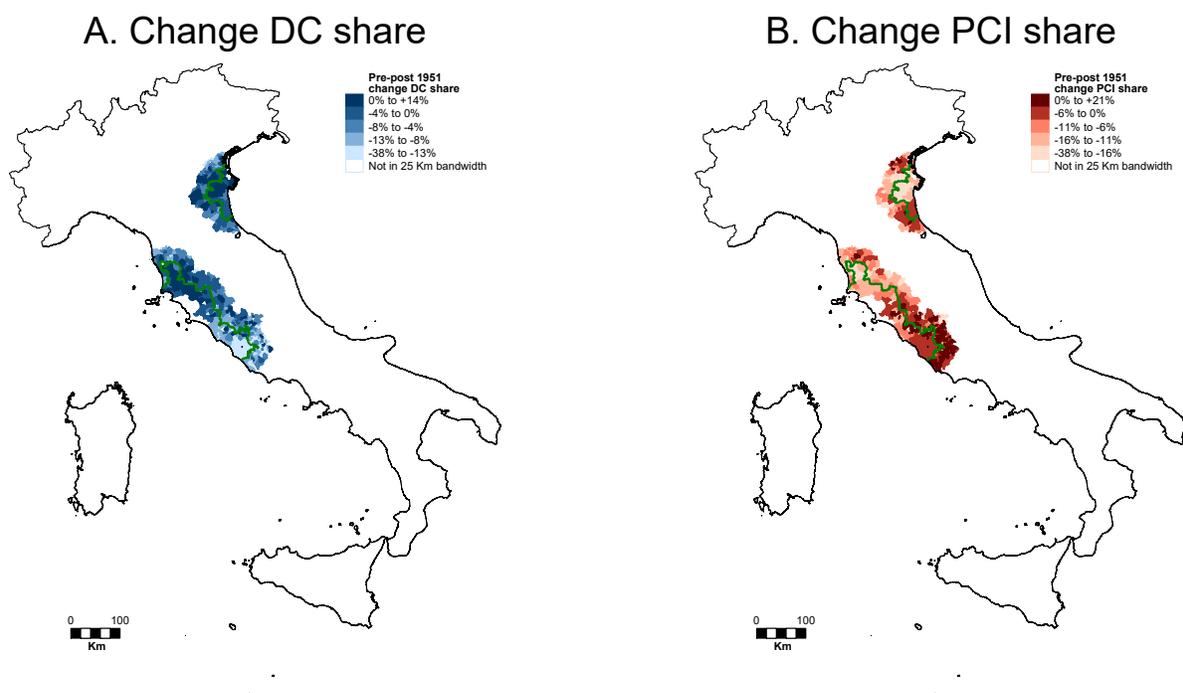
**Table C.2:** Contemporary Policies: 1947-1951

	<i>Preferred Bandwidth</i>			<i>Alternative Bandwidths</i>					
	< 25km (N=490)			< 10km (N=222)			< 50km (N=863)		
	Control	$\beta$	[s.e]	Control	$\beta$	[s.e]	Control	$\beta$	[s.e]
	mean			mean			mean		
Malaria eradication (1947-52)	0.50	0.029	[0.094]	0.53	-0.113	[0.153]	0.37	0.016	[0.074]
log Marshall Plan funds per capita (1948-52)	4.74	-0.205	[1.130]	3.75	0.092	[1.617]	5.85	0.040	[11.644]
Share of workers in GATT affected sectors (1948)	0.81	0.017	[0.020]	0.81	0.008	[0.028]	0.81	0.020	[0.016]
Piano Casa dummy (1949)	0.03	-0.038	[0.032]	0.03	-0.030	[0.047]	0.02	-0.021	[0.026]
Piano Casa houses per 10'000 inhabitants (1949)	0.99	-1.633	[1.259]	1.09	-2.096	[2.145]	0.91	-1.077	[0.991]
Cassa del Mezzogiorno dummy (1950)	0.020	0.020	[0.025]	0.000	0.000	-	0.04	-0.002	[0.029]
log firms in ECSC affected sectors (1951)	2.43	-0.179	[0.231]	2.57	-0.500	[0.361]	2.57	0.072	[0.200]
Share of workers in ECSC affected sectors (1951)	0.03	0.003	[0.017]	0.03	-0.030	[0.026]	0.03	0.013	[0.012]

*Notes:* For a description of the dependent variables, refer to Appendix B. “Control mean” represents average in control towns.  $\beta$  columns report the coefficient of separate regressions of the RDD specification in Equation (2). The sample consists of all towns close to the reform borders of Delta Padano and Maremma. Estimates reported for three bandwidths (25, the preferred, 10 and 50 km). Within 10 km from the reform border, no towns fell into the jurisdiction of *Cassa del Mezzogiorno*. “[s.e.]” columns report heteroschedastic robust s.e. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

## C.5 Visualization of Impact and Alternative Specifications

Figure C.3: Electoral effect of the reform: visualization of impact on a map



*Notes:* Change in DC vote share (Panel A) and change in PCI vote share (Panel B) 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.

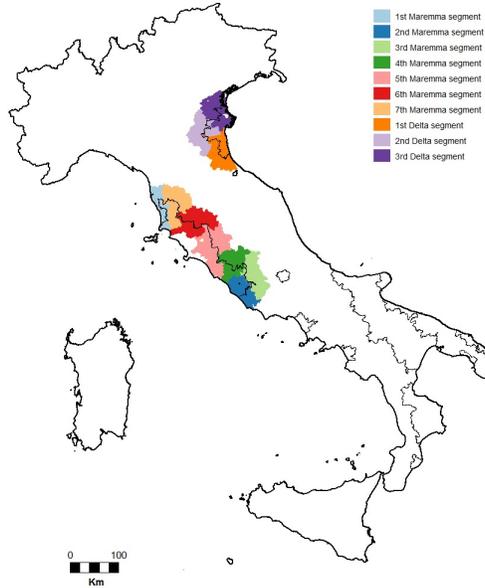
**Table C.3:** Robustness to alternative specifications.

	No prov. seats	2 <sup>nd</sup> pol.	Lat-long pol.	Segment FEs	Elec. dist. FEs	Prov. FE
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. DC vote share</b>						
Treatment × 1950	0.041*** [0.013]	0.048** [0.024]	0.020** [0.008]	0.031** [0.013]	0.038*** [0.013]	0.033** [0.015]
Treatment × 1960	0.036** [0.016]	0.061** [0.027]	0.029*** [0.009]	0.029* [0.016]	0.039** [0.015]	0.036** [0.016]
Treatment × 1970	0.047*** [0.016]	0.064** [0.025]	0.039*** [0.010]	0.036** [0.016]	0.052*** [0.016]	0.046*** [0.016]
Treatment × 1980	0.048*** [0.018]	0.047* [0.028]	0.047*** [0.012]	0.036** [0.018]	0.051*** [0.018]	0.045** [0.018]
<b>B. PCI vote share</b>						
Treatment × 1950	-0.033** [0.014]	0.004 [0.022]	-0.018** [0.007]	-0.022* [0.012]	-0.028** [0.012]	-0.022* [0.012]
Treatment × 1960	-0.036** [0.015]	-0.013 [0.024]	-0.028*** [0.009]	-0.027* [0.014]	-0.033** [0.014]	-0.031** [0.015]
Treatment × 1970	-0.035** [0.016]	-0.017 [0.027]	-0.031*** [0.010]	-0.028* [0.015]	-0.030** [0.015]	-0.030* [0.016]
Treatment × 1980	-0.024 [0.019]	-0.002 [0.031]	-0.028*** [0.012]	-0.022 [0.018]	-0.017 [0.018]	-0.019 [0.018]
<b>C. Casse Mutue income p.c.</b>						
Treatment	117.220** [55.307]	240.532*** [80.653]	143.625*** [34.311]	103.226* [55.713]	147.549*** [56.198]	124.183*** [55.303]
<b>D. Casse Mutue expenses p.c.</b>						
Treatment	116.118** [50.004]	232.458*** [73.101]	146.915*** [29.892]	100.840** [49.800]	124.428** [49.475]	98.217** [48.821]
<b>E. Casse Mutue votes p.c.</b>						
Treatment	0.014** [0.006]	0.027** [0.011]	0.013*** [0.003]	0.016*** [0.006]	0.017*** [0.006]	0.014** [0.006]
<b>F. Coldiretti votes p.c.</b>						
Treatment	0.011** [0.005]	0.022*** [0.008]	0.010*** [0.003]	0.012*** [0.005]	0.014*** [0.005]	0.011** [0.005]
<b>G. Public Sector Employment</b>						
Treatment × 1936	0.005 [0.004]	0.014* [0.008]	0.005 [0.004]	0.002 [0.004]	0.006 [0.004]	0.003 [0.004]
Treatment × 1961	0.007* [0.004]	0.011** [0.006]	0.011*** [0.003]	0.005 [0.003]	0.008** [0.004]	0.009** [0.004]
Treatment × 1971	0.004 [0.006]	0.020* [0.010]	0.016*** [0.004]	0.005 [0.006]	0.008 [0.006]	0.010 [0.006]
Treatment × 1981	0.021*** [0.007]	0.034*** [0.012]	0.026*** [0.005]	0.018*** [0.007]	0.025*** [0.007]	0.024*** [0.007]
Treatment × 1991	0.011 [0.010]	0.026* [0.015]	0.020*** [0.006]	0.010 [0.008]	0.017** [0.008]	0.019** [0.008]
<b>H. Fiscal Transfers p.c.</b>						
Treatment × 1955	0.043 [0.156]	-0.254 [0.282]	-0.011 [0.106]	0.119 [0.162]	-0.026 [0.149]	-0.010 [0.162]
Treatment × 1959	0.259 [0.161]	-0.027 [0.289]	0.088 [0.104]	0.259 [0.166]	0.219 [0.157]	0.206 [0.167]

*Notes:* The Table reports coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, B, G, H) and (2) (Panels C, D, E, F). Panels A, B, G, H: regressions control for reform area × year and town fixed effects. Panel C, D: regressions control for reform area fixed effects. Panels E, F: regressions control for reform area × year fixed effects. Panels A, B: The omitted category is the  $\beta$  of the elections of 1946 and 1948. Panel G: omitted category is the  $\beta$  of 1951. Panel H: omitted category is the  $\beta$  of 1952. Col. 1: drop 10 provincial seats (including Rome). Col. 2: control for 2<sup>nd</sup> order polynomial in distance interacted with decades on both side of the border. Col. 3: control for polynomial in latitude and longitude interacted with decades as in Dell (2010). Col. 4: control for ten segments × year fixed effects (see Figure C.4). Col. 5: control for electoral districts × year fixed effects. Col. 6: control for province × year fixed effects. Units of observation are town-years (Panels A, B, E, F, G, H) or towns (Panels C, D). 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, B, E, F, G, H) or heteroschedastic robust (Panels C, D) in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

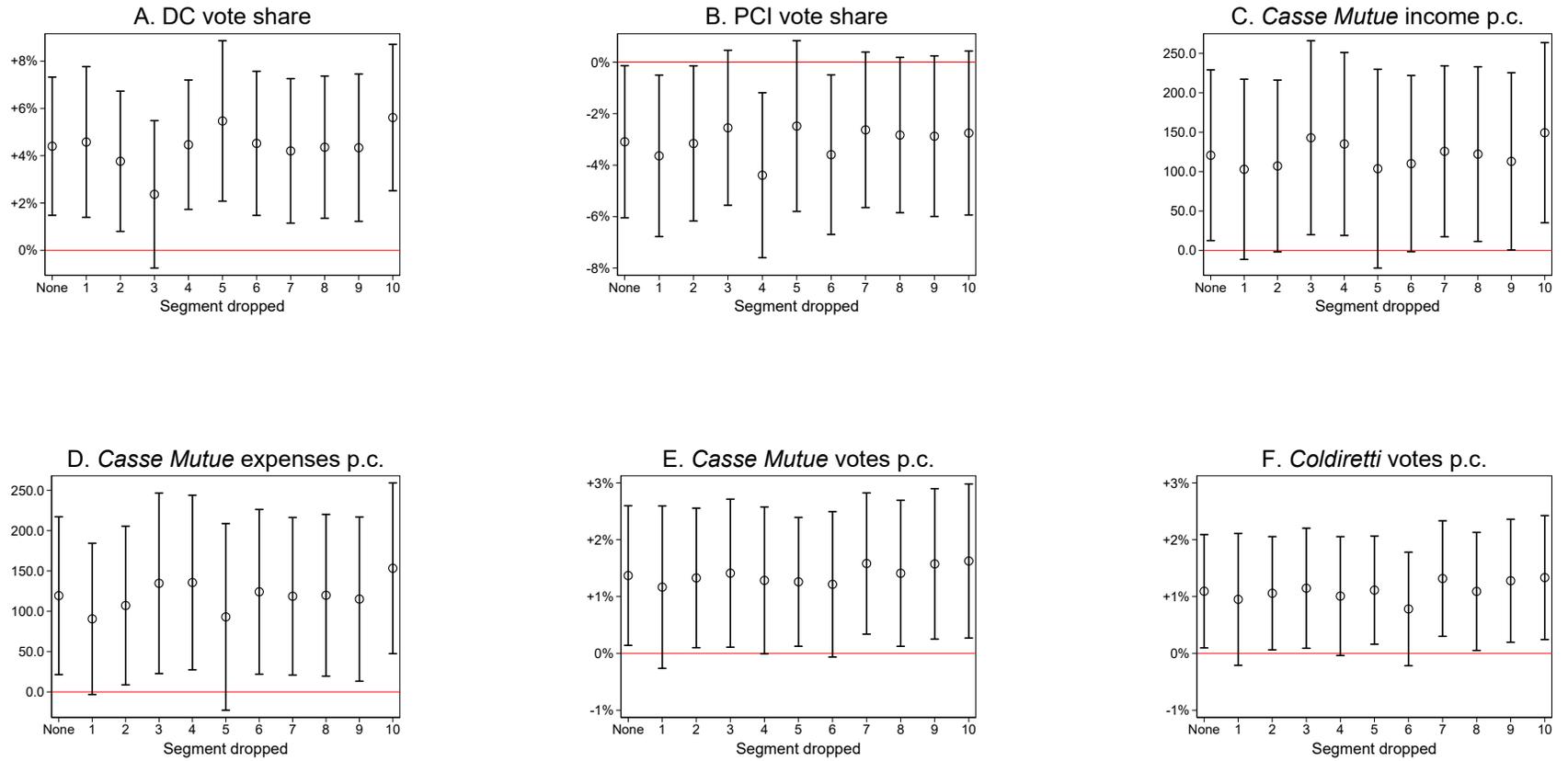
## C.6 Robustness to Dropping Portions of the Reform Border

Figure C.4: 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) in Figure C.5.

**Figure C.5:** Treatment coefficients when dropping portions of the sample





*Notes:* The Figure reports coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, B, G, H, I, J, K, L) and (2) (Panels C, D, E, F). Panels A, B, G, H, I, J, K, L: regressions control for reform area  $\times$  year and town fixed effects. Panel C, D: regressions control for reform area fixed effects. Panels E, F: regressions control for reform area  $\times$  year fixed effects. Panels A, B: 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 close to one of the 10 segments marked on Map C.4. Units of observation are town-years (Panels A, B, E, F, G, H, I, J, K, L) or towns (Panels C, D). The sample consists of all towns within 25 km of the reform borders of Delta Padano and Maremma. We estimate standard errors clustered at the town level (Panels A, B, E, F, G, H, I, J, K, L) or heteroschedastic robust (Panels C, D) and plot 95% confidence intervals as bars around the coefficients.

## C.7 Instrumental Variable

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

	<i>Preferred Bandwidth &lt; 25km</i>	<i>Alternative Bandwidths &lt; 10km      &lt; 50km</i>	
	(1)	(2)	(3)
<b>A. DC vote share</b>			
Treatment × 1950	0.044*** [0.012]	0.044*** [0.012]	0.044*** [0.012]
Treatment × 1960	0.044*** [0.015]	0.044*** [0.015]	0.044*** [0.015]
Treatment × 1970	0.039** [0.016]	0.039** [0.016]	0.039** [0.016]
Treatment × 1980	0.024 [0.024]	0.024 [0.024]	0.024 [0.024]
<b>B. PCI vote share</b>			
Treatment × 1950	-0.041*** [0.012]	-0.041*** [0.012]	-0.041*** [0.012]
Treatment × 1960	-0.053*** [0.015]	-0.053*** [0.015]	-0.053*** [0.015]
Treatment × 1970	-0.049*** [0.016]	-0.049*** [0.016]	-0.049*** [0.016]
Treatment × 1980	-0.080*** [0.021]	-0.080*** [0.021]	-0.080*** [0.021]
<b>C. Casse Mutue income p.c.</b>			
Treatment	85.006 [72.754]	114.222 [130.800]	97.562 [61.697]
<b>D. Casse Mutue expenses p.c.</b>			
Treatment	130.516** [64.440]	175.062 [118.754]	147.807*** [55.802]
<b>E. Casse Mutue votes p.c.</b>			
Treatment	0.020** [0.010]	0.022 [0.020]	0.033*** [0.009]
<b>F. Coldiretti votes p.c.</b>			
Treatment	0.017** [0.008]	0.024* [0.014]	0.028*** [0.007]
<b>G. Public Sector Employment</b>			
Treatment × 1936	0.011* [0.007]	-0.002 [0.013]	0.009* [0.005]
Treatment × 1961	0.007 [0.005]	-0.003 [0.009]	0.014*** [0.004]
Treatment × 1971	-0.004 [0.009]	-0.019 [0.016]	0.007 [0.007]
Treatment × 1981	0.021* [0.011]	0.014 [0.019]	0.030*** [0.008]
Treatment × 1991	-0.003 [0.015]	-0.010 [0.022]	0.014 [0.011]
<b>H. Fiscal Transfers p.c.</b>			
Treatment × 1955	0.082 [0.228]	0.124 [0.448]	-0.045 [0.169]
Treatment × 1959	0.332 [0.221]	0.492 [0.443]	0.232 [0.163]

*Notes:* The Table reports IV coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, B, G, H) and (2) (Panels C, D, E, F). 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, B, E, F, G, H) or towns (Panels C, D). 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). Standard errors clustered at the town level (Panels A, B, E, F, G, H) or heteroschedastic robust (Panels C, D) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

## C.8 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.<sup>41</sup>

**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 Col. 1 Table C.5: the coefficient is small and 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 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.<sup>42</sup>

$$\begin{aligned}
 y_{irt} = & \eta_i + \eta_{rt} + \beta \cdot Post_t \times T_i + \\
 & + \gamma \cdot Post_t \times \left(\frac{L_a}{L}\right)_i + \delta \cdot Post_t \times \left(\frac{L_a}{L}\right)_i \times T_i + \\
 & + \alpha^0 \cdot Post_t \times d_i + \delta^0 \cdot Post_t \times \left(\frac{L_a}{L}\right)_i \times d_i + \\
 & + \alpha^1 \cdot Post_t \times d_i \times T_i + \delta^1 \cdot Post_t \times \left(\frac{L_a}{L}\right)_i \times d_i \times T_i + u_{irt}
 \end{aligned} \tag{C.1}$$

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.<sup>43</sup> If resented potential beneficiaries in control towns punished DC, we would expect  $\gamma$  to be negative.

Table C.5-Column 2 reports the baseline result without heterogeneity. We compare pre-reform elections (1946, 48) with the first two elections after the reform (1953, 58). The DC vote share increases differentially by 4 percentage points in treated towns. Column 3 then shows that, in control towns, places with high and low share of agricultural workers showed

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<sup>41</sup>The 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).

<sup>42</sup>We include towns in a 25 km bandwidth. We performed the same exercise with a simpler difference-in-difference specification, which requires less power than the Panel RDD. Results are virtually identical and are available upon request.

<sup>43</sup>We 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.

similar support for DC after the reform (if anything, support for DC is higher in towns with more agricultural workers:  $\gamma=0.015$ ,  $s.e.=0.34$ ).<sup>44</sup> Similarly, for PCI none of the interactions is significant (Column 6). These estimates support our interpretation of the main results: the increase in support for DC in treated towns, as opposed to the reduction in control towns, drives the panel RDD estimates.

***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 4 reports the estimates of a modified version 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. Similarly, the negative and insignificant coefficient of the same interaction when the dependent variable is PCI vote share speaks against greater gains of the Communists in these towns (Column 7). 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.<sup>45</sup> 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-Column 1 reports 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 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 stable. Columns 5 through 8 show that also the impact of the reform on the Communist party is robust to dropping towns close to the border. These results suggest again that resentment against DC in control towns closely exposed to the reform does not drive our results.

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<sup>44</sup>In 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 ( $\delta=0.077$ ,  $s.e.=0.084$ ).

<sup>45</sup>This 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			Communists		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.016 [0.012]						
Post 1951 ×							
Treatment		0.040*** [0.013]	-0.013 [0.049]	0.048 [0.039]	-0.032** [0.013]	-0.096** [0.045]	0.013 [0.042]
Share agricultural workers			0.015 [0.034]			-0.025 [0.037]	
Share agricultural workers × Treatment			0.077 [0.084]			0.095 [0.070]	
Share of town limit on reform border				0.073 [0.051]			-0.030 [0.056]
Share of town limit on reform border × Treatment				0.014 [0.114]			-0.086 [0.096]
Mean Y Control Group	0.06	0.36	0.36	0.31	0.33	0.33	0.38
Observations	490	1929	1925	620	1929	1925	620

*Notes:* The Table reports coefficients from Equation (2) (col. 1) and (C.1) (cols. 2-7). Col. 1 controls for reform area fixed effects; cols. 2-7 control for year × 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 and 5: baseline (no heterogeneity). Column 3 and 6: RDD with heterogeneity in share of agricultural workers. Column 4 and 7: RDD with heterogeneity in share of town limits touching the reform border. Column 1: dependent variable is land invasions in 1951-52. Columns 2-4: dependent variable is Christian Democrat (DC) vote share. Columns 5-7: dependent variable is Communist (PCI) vote share. For PCI we use the vote share of the Popular Democratic Front (FDP) in 1948. Share of agricultural workers is from the population census. Units of observation are town-years. Columns 1-3 and 5-6: the sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. Column 4 and 7: 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.

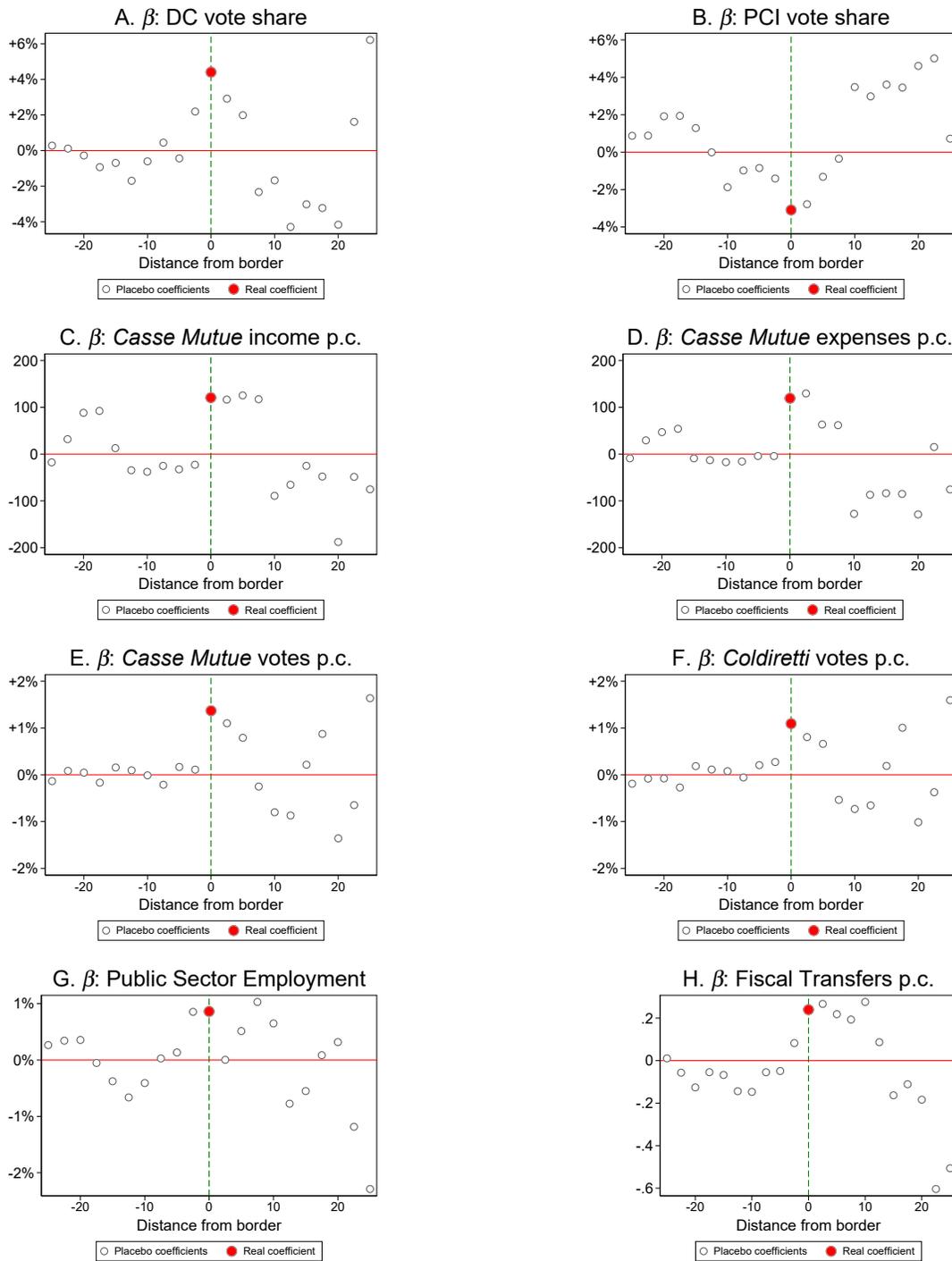
	Christian Democrats				Communist Party			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Donut: 1.5 km	Donut: 2 km	Donut: 2.5 km	All	Donut: 1.5 km	Donut: 2 km	Donut: 2.5 km
Treatment $\times$ 1950s	0.041*** [0.013]	0.033*** [0.012]	0.031** [0.012]	0.038*** [0.013]	-0.032** [0.013]	-0.049*** [0.014]	-0.048*** [0.014]	-0.046*** [0.015]
Treatment $\times$ 1960s	0.037** [0.016]	0.032** [0.015]	0.029** [0.015]	0.034** [0.016]	-0.036** [0.015]	-0.047*** [0.016]	-0.045*** [0.017]	-0.042** [0.018]
Treatment $\times$ 1970s	0.047*** [0.016]	0.046*** [0.017]	0.043** [0.017]	0.042** [0.018]	-0.034** [0.016]	-0.049*** [0.017]	-0.046*** [0.017]	-0.038** [0.017]
Treatment $\times$ 1980s	0.048*** [0.018]	0.052*** [0.018]	0.050*** [0.019]	0.053*** [0.020]	-0.024 [0.019]	-0.043** [0.019]	-0.040** [0.020]	-0.028 [0.019]
Mean Y Control Group	0.36	0.36	0.36	0.36	0.33	0.33	0.33	0.33
Number of Towns	490	471	461	444	490	471	461	444
Observations	5838	5615	5495	5291	5838	5615	5495	5291

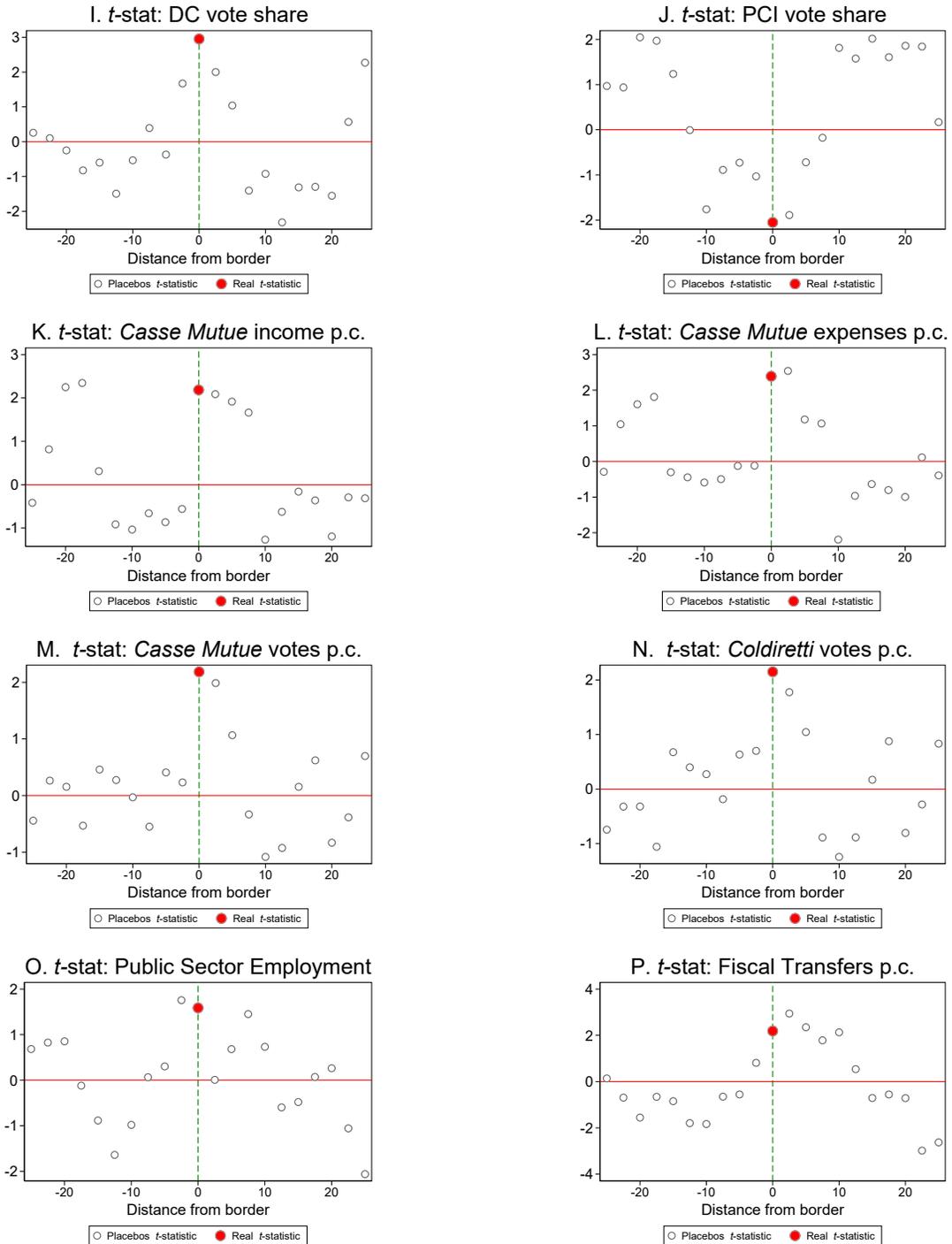
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*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. We include the 1992 election in the 1980s decade. The omitted category is the  $\beta$  of the elections of 1946 and 1948. Columns 1-4: dependent variable is Christian Democrat (DC) vote share. Columns 5-8: dependent variable is Communist (PCI) vote share. 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. 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.9 Placebo Borders

Figure C.6: Placebo Borders





*Notes:* The Panels report results of placebo regressions. 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-H: estimated  $\beta$ . Panels I-P: *t*-statistics calculated from standard errors clustered at the town level (Panels I, J, M-P) or heteroschedastic robust (Panels K, L). In every panels 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, E-J, M-P) or towns (Panels C, D, K, L).

## C.10 Spatial Standard Errors

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

	<i>Casse Mutue</i>						Public Sct.	Fiscal
	DC	PCI	income p.c.	expenditure p.c.	log votes	log <i>Coldiretti</i> votes	Employment	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment $\times$ Post	0.044***	-0.031**	120.716**	119.382**	0.014**	0.011**	0.009**	0.240**
Cluster: town	[0.015]	[0.015]	[55.278]	[49.953]	[0.006]	[0.005]	[0.005]	[0.110]
Conley s.e.: cutoff = 5 km	[0.015]	[0.015]	[55.058]	[50.464]	[0.006]	[0.005]	[0.005]	[0.110]
Conley s.e.: cutoff = 10 km	[0.015]	[0.016]	[55.647]	[51.070]	[0.006]	[0.005]	[0.006]	[0.106]
Conley s.e.: cutoff = 25 km	[0.017]	[0.017]	[57.302]	[53.533]	[0.007]	[0.006]	[0.006]	[0.106]
Conley s.e.: cutoff = 50 km	[0.019]	[0.018]	[52.178]	[53.417]	[0.007]	[0.005]	[0.007]	[0.101]
Conley s.e.: cutoff = 100 km	[0.021]	[0.014]	[42.217]	[45.776]	[0.007]	[0.005]	[0.007]	[0.093]
Mean Y Control Group	0.36	0.33	437.79	316.00	0.03	0.02	0.05	1.14
Observations	5838	5838	488	488	1451	1419	2939	1438

*Notes:* The Table reports on the first row coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Cols. 1, 2, 7, 8) and (2) (Cols. 3, 4, 5, 6). Cols. 1, 2, 7, 8: regressions control for reform area  $\times$  year and town fixed effects. Cols 3, 4: regressions control for reform area fixed effects. Cols. 5, 6: regressions control for reform area  $\times$  year fixed effects. Units of observation are town-years (Cols. 1, 2, 5, 6, 7, 8) or towns (Cols. 3, 4). The sample consists of all towns within 25 km of the reform borders of Delta Padano and Maremma. Row 2: standard errors clustered at the town level (Cols. 1, 2, 5, 6, 7, 8) or heteroschedastic robust (Cols. 3, 4) in parentheses. Rows 3-7: 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

Table D.1: Balance and Pre-Trends at the Border

	Preferred Bandwidth < 25km (N=1169)			Alternative Bandwidths					
	Control mean	$\beta$	[s.e.]	Control mean	$\beta$	[s.e.]	Control mean	$\beta$	[s.e.]
<b>A: Balance Land Distribution 1948</b>									
Share of Expropriable Estates 1948	0.001	0.004***	[0.001]	0.001	0.003	[0.002]	0.001	0.003*	[0.001]
<b>B: Balance Vote Shares 1946 &amp; 1948</b>									
Christian Democrats (DC) 1946	0.350	-0.036*	[0.020]	0.329	-0.036	[0.032]	0.351	-0.036**	[0.016]
Christian Democrats (DC) 1948	0.534	-0.061***	[0.018]	0.517	-0.056*	[0.029]	0.542	-0.061***	[0.014]
Communists (PCI) 1946	0.058	0.039**	[0.017]	0.064	0.040	[0.027]	0.053	0.056***	[0.014]
Communists (PCI) 1948	0.169	0.073***	[0.020]	0.176	0.093***	[0.034]	0.159	0.093***	[0.017]
<b>C: Balance Geography and Census 1951</b>									
Distance from the Coast	25.71	1.085	[2.294]	24.88	-0.777	[3.450]	27.53	2.298	[1.968]
Distance from Rome	307.4	-10.49	[10.04]	335.7	9.162	[16.68]	275.9	-17.75**	[8.298]
Area (miles <sup>2</sup> )	13.56	10.67***	[2.963]	14.51	-3.906	[5.880]	12.71	18.32***	[2.631]
Slope	3.121	-0.322*	[0.184]	2.673	-0.184	[0.293]	3.381	-0.385***	[0.149]
Elevation	446.2	-15.95	[36.71]	416.1	-3.596	[55.86]	482.9	4.453	[31.52]
Wheat Suitability	4.052	0.021	[0.045]	4.029	0.087	[0.066]	4.050	-0.009	[0.041]
Maize Suitability	3.669	0.088	[0.099]	3.488	0.200	[0.140]	3.846	0.033	[0.087]
Malaria (1932)	0.546	-0.000	[0.051]	0.576	0.071	[0.086]	0.508	0.016	[0.044]
Log Population	8.231	0.386***	[0.109]	8.245	0.166	[0.178]	8.161	0.393***	[0.090]
Share Active Population	0.564	-0.001	[0.013]	0.570	0.029	[0.020]	0.558	-0.010	[0.010]
Share Agricultural Workers	0.690	0.016	[0.022]	0.685	0.017	[0.035]	0.691	0.013	[0.019]
Share Manufacturing Workers	0.118	-0.029***	[0.010]	0.121	-0.030**	[0.017]	0.112	-0.034***	[0.008]
Share Public Sector Workers	0.040	0.006	[0.004]	0.039	0.008	[0.007]	0.041	0.005	[0.004]
<b>D: Pre-Trends Vote Shares 1948-46</b>									
Christian Democrats (DC)	0.185	-0.024	[0.018]	0.187	-0.019	[0.030]	0.191	-0.025*	[0.015]
Communists (PCI)	0.111	0.035**	[0.015]	0.112	0.053**	[0.023]	0.106	0.036***	[0.013]
<b>E: Pre-Trends Census 1951-36</b>									
Log Population	0.125	0.058***	[0.013]	0.131	0.041*	[0.022]	0.112	0.045***	[0.011]
Log Workers	0.171	0.100***	[0.026]	0.191	0.096**	[0.043]	0.138	0.072***	[0.022]
Share Active Population	0.138	0.020*	[0.011]	0.145	0.034*	[0.018]	0.126	0.010	[0.009]
Share Agricultural Workers	-0.039	0.010	[0.012]	-0.040	0.011	[0.020]	-0.054	-0.003	[0.010]
Share Manufacturing Workers	-0.051	-0.010	[0.008]	-0.055	-0.013	[0.013]	-0.043	-0.006	[0.006]
Share Public Sector Workers	0.016	0.000	[0.003]	0.014	-0.002	[0.005]	0.017	-0.002	[0.003]

*Notes:* The columns beneath  $\beta$  report the coefficient of separate regressions of the RDD specification in Equation (2). Dependent variables are specified on the first column, and their average in control towns is reported in the columns beneath “Control mean”. Refer to the Supplementary Materials 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, Puglia and Lucania and Sila. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). In Panel A, the sample is approximately 14% smaller due to missing data in Medici (1948). The columns beneath “[s.e.]” report heteroschedastic robust standard errors.

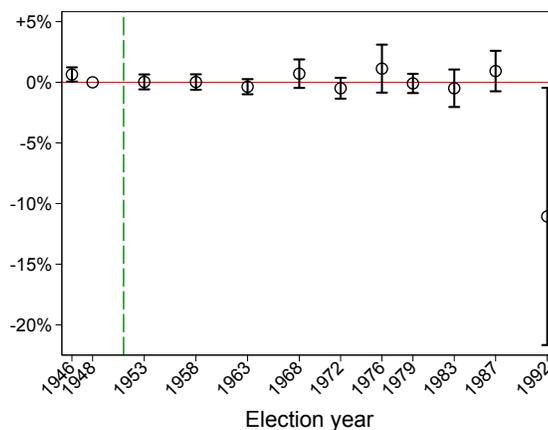
## D.2 Effect of the Reform on Other Parties and Turnout

**Table D.2:** The impact of the land reform on all major Italian parties

	Vote share						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	DC	PCI	PSI	PSDI	PRI	PLI	MSI
Treatment $\times$ Post	0.044***	-0.031**	-0.005	0.001	0.002	-0.001	-0.004
	[0.015]	[0.015]	[0.021]	[0.011]	[0.010]	[0.005]	[0.004]
Mean Y Control Group	0.36	0.33	0.14	0.06	0.03	0.02	0.04
Number of Towns	490	490	490	490	490	490	490
Observations	5838	5838	5831	5838	5838	5838	5356

*Notes:* The Table reports  $\beta$  for treated towns in the post-reform years (1953-92) from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. Dependent variables are vote share for: Christian Democrat (DC: Col. 1); Italian Communist Party (PCI: Col. 2); Italian Socialist Party (PSI: Col. 3); Italian Social-Democratic Party (PSDI: Col. 4); Italian Republican Party (PRI: Col. 5); Italian Liberal Party (PLI: Col. 6) and Italian Social Movement (MSI: Col. 7). Sometimes different parties run under a single name: this happens in 1946, 1968 (PSI and PSDI) and 1948 (PCI and PSI). In these cases we assign the vote share of the combined party to each of the two parties in the respective regressions. Units of observation are town-years. The sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. The sample in Column 7 excludes the 1946 elections, when the MSI did not run. Standard errors clustered at the town level in parentheses.

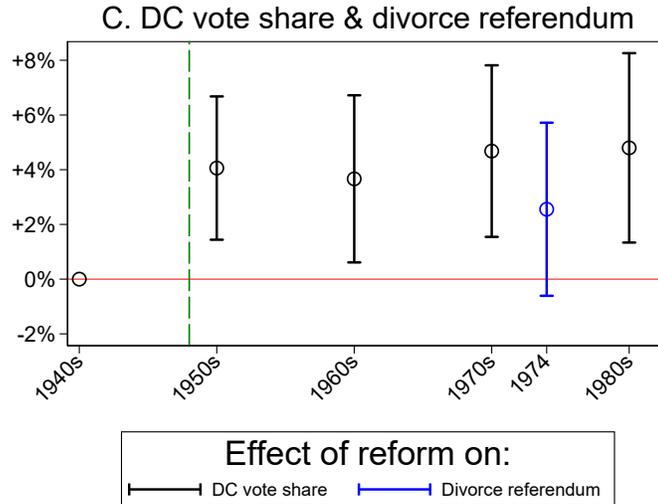
**Figure D.1:** Turnout: Panel RDD Coefficients



*Notes:* The Figure displays coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. The omitted category is the  $\beta$  of 1948. Dependent variable is votes cast divided by number of eligible voters. Units of observation are town-years. The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The vertical line marks the 1951 land reform. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

### D.3 Divorce Referendum

Figure D.2: 1974 Referendum on the Repeal of Divorce



*Notes:* The Figure displays coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. We estimate  $\beta$  for separate decades as well as for the 1974 divorce referendum. The omitted category is the  $\beta$  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  $\beta$  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. Units of observation are town-years. The sample consists of all towns within 25 Km from the reform borders of Delta Padano and Maremma. The vertical lines mark the 1951 land reform. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

**Table D.3:** Robustness to alternative bandwidths: Parliamentary elections (1946-1992) and divorce referendum (1974).

	$< 25km$	$< 10km$	$< 50km$
	(1)	(2)	(3)
Treatment $\times$ 1950s	0.041*** [0.013]	0.045* [0.024]	0.031*** [0.011]
Treatment $\times$ 1960s	0.037** [0.016]	0.053* [0.027]	0.028** [0.013]
Treatment $\times$ 1970s	0.047*** [0.016]	0.061** [0.025]	0.037*** [0.013]
Treatment $\times$ 1974	0.026 [0.016]	0.051** [0.026]	0.016 [0.013]
Treatment $\times$ 1980s	0.048*** [0.018]	0.046 [0.028]	0.041*** [0.015]
Mean Y Control Group	0.36	0.34	0.38
Number of Towns	490	222	863
Observations	6328	2873	11096

*Notes:* The Table reports coefficients  $\beta_t$  of the panel RDD Equation (1) with different bandwidths. In these regressions, we include the 1992 election in the 1980s decade. The omitted category is the  $\beta$  of the elections of 1946 and 1948. All regressions control reform area  $\times$  year and town fixed effects. Dependent variable is DC vote share in all years except 1974. In 1974 the dependent variable is the share of “Yes” votes at the divorce referendum. Units of observation are town-years. Sample consists of all towns within a given bandwidth of the reform borders of Delta Padano and Maremma: Column 1: bandwidth is 25 km; Column 2: bandwidth is 10 km; Column 3: bandwidth is 50 km. Standard errors clustered at the town level in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

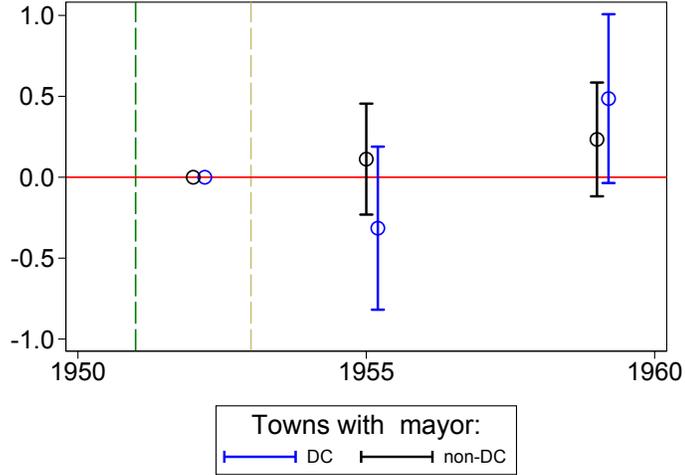
**Table D.4:** Dependent variable: divorced couples per 10'000 married couples, 1981

	25km	10km	50km
	(1)	(2)	(3)
Treatment	4.045 [4.411]	4.014 [7.027]	4.762 [3.846]
Mean Y Control Group	23.67	25.35	24.58
Observations	490	222	863

*Notes:* The Table reports coefficients of the RDD specification in Equation (2). Dependent variable is number of divorces per married couple in 1981. Source of 1981 divorced and married couple is 1981 population census. Units of observation are towns. The sample consists of all towns close to the reform borders of Delta Padano and Maremma. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Heteroschedastic robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## D.4 The Role of Aligned Mayors

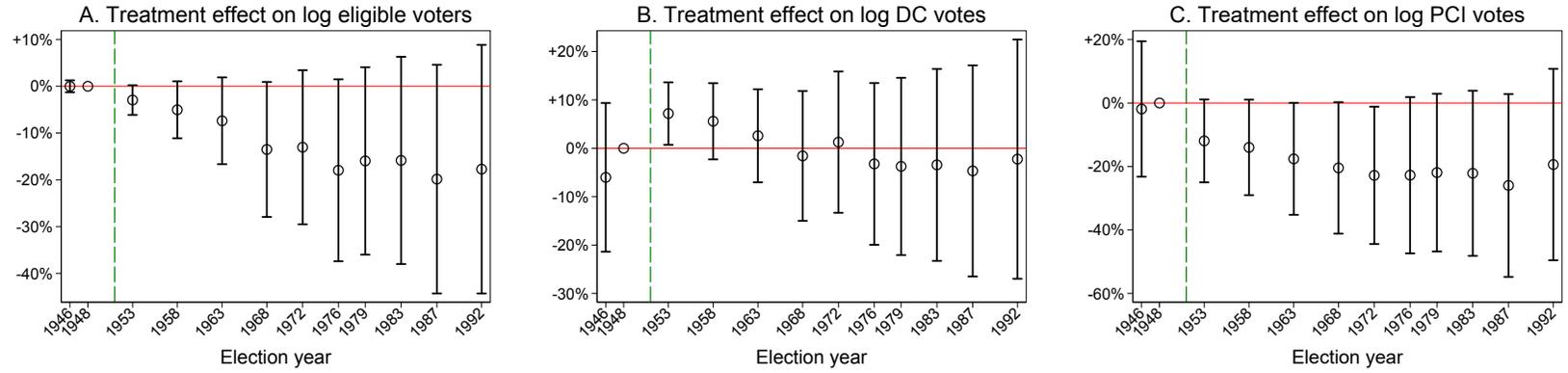
Figure D.3: The role of aligned mayors



*Notes:* The Figure display coefficients  $\beta_t$  from the panel RDD Equation (1) for two groups of towns: those with DC mayors in 1946 (in blue) and the others (in black). The regression controls for year  $\times$  reform area  $\times$  1946 mayor affiliation and town fixed effects. The omitted categories are the  $\beta$  of 1952 and the dependent variable is logarithm of per capita fiscal transfers from the central government to the municipal governments (available only for 1952, 1955 and 1959). The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The green vertical lines mark the 1951 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.5 Migration

**Figure D.4:** Absolute Number of Votes: Panel RDD Coefficients



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*Notes:* The Panels display coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. The omitted category is the  $\beta$  of 1948. Panel A: dependent variable is log of eligible voters. Panel B: dependent variable is log of Christian Democrat (DC) votes. Panel C: dependent variable is log of Communists (PCI) votes. 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). Units of observation are town-years. The sample consists of all towns within 25 km from the reform borders of Delta Padano and Maremma. The vertical lines mark the 1951 land reform. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

**Table D.5: Population Composition**

	Share workers in		Labor force	Share males	Share population aged			
	agriculture	manufacturing	participation		0-20	21-45	46-65	>65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment $\times$ 1961	-0.025 [0.019]	0.006 [0.009]	-0.020 [0.014]	-0.001 [0.002]	-0.003 [0.004]	-0.003 [0.004]	0.003 [0.004]	0.004 [0.003]
Treatment $\times$ 1971	-0.030 [0.025]	0.005 [0.016]	-0.021 [0.014]	-0.003 [0.002]	-0.001 [0.006]	0.002 [0.005]	0.005 [0.006]	0.005 [0.005]
Treatment $\times$ 1981	-0.011 [0.029]	-0.010 [0.020]	-0.064** [0.032]	-0.004 [0.002]	-0.006 [0.008]	-0.005 [0.008]	0.001 [0.006]	0.010 [0.009]
Treatment $\times$ 1991	0.008 [0.032]	-0.027 [0.021]	-0.050 [0.032]	-0.004 [0.003]	-0.002 [0.007]	-0.009 [0.009]	0.002 [0.005]	0.012 [0.010]
Treatment $\times$ 2001	0.008 [0.033]	-0.019 [0.021]	0.001 [0.015]	-0.004 [0.003]	-0.006 [0.007]	-0.013 [0.009]	0.003 [0.006]	0.016 [0.011]
Mean Y Control Group	0.30	0.23	0.60	0.50	0.25	0.33	0.23	0.14
Number of Towns	490	490	490	490	490	490	490	490
Observations	2939	2939	2939	2940	2940	2940	2940	2940

*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. Column 1 and 2: dependent variable is share of workers employed in agriculture and manufacturing. Column 3: dependent variable is share of people in the labor force. Column 4: dependent variable is share of males in the population. Columns 5-8: dependent variable is share of people within specified age groups. The omitted category is the  $\beta$  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.6 Economic Conservatism

**Table D.6:** The impact of the reform on income and home ownership.

	<i>Preferred bandwidth</i>	<i>Alternative bandwidths</i>	
	<i>&lt; 25km</i>	<i>&lt; 10km</i>	<i>&lt; 50km</i>
	(1)	(2)	(3)
<b>A. Home ownership: 1951-2001</b>			
Treatment × 1961	0.001 [0.003]	-0.002 [0.004]	0.001 [0.002]
Treatment × 1971	-0.005 [0.005]	-0.009 [0.009]	-0.004 [0.005]
Treatment × 1981	-0.006 [0.006]	-0.009 [0.011]	-0.006 [0.005]
Treatment × 1991	-0.005 [0.009]	-0.005 [0.014]	-0.007 [0.007]
Treatment × 2001	-0.003 [0.009]	-0.011 [0.015]	-0.008 [0.007]
Mean Y Control Group	0.20	0.19	0.19
Number of Towns	490	222	863
Observations	2940	1332	5178
<b>B. log income per capita: 1981</b>			
Treatment	0.011 [0.091]	0.155 [0.162]	0.069 [0.076]
Mean Y Control Group	8.46	8.46	8.45
Observations	490	222	862

*Notes:* Panel A: the Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year × reform area and town fixed effects. Dependent variable is per capita homes occupied by the owner. The omitted category is the  $\beta$  of the elections of 1951. Source is the decadal population censuses. Units of observation are town-years. Panel B: the Table reports coefficients  $\beta$  of separate regressions of the RDD specification in Equation (2). Dependent variable is log per capita income in 1981. The source of 1981 income is Marbach and Ciapparelli (1983) and the source of population is the population census. The sample consists of all towns close to the reform borders of Delta Padano and Maremma. We report estimates for the preferred bandwidth (25 km) and two alternative bandwidths (10 km and 50 km). Standard errors clustered at the town level (Panel A) and heteroschedastic robust (Panel B) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

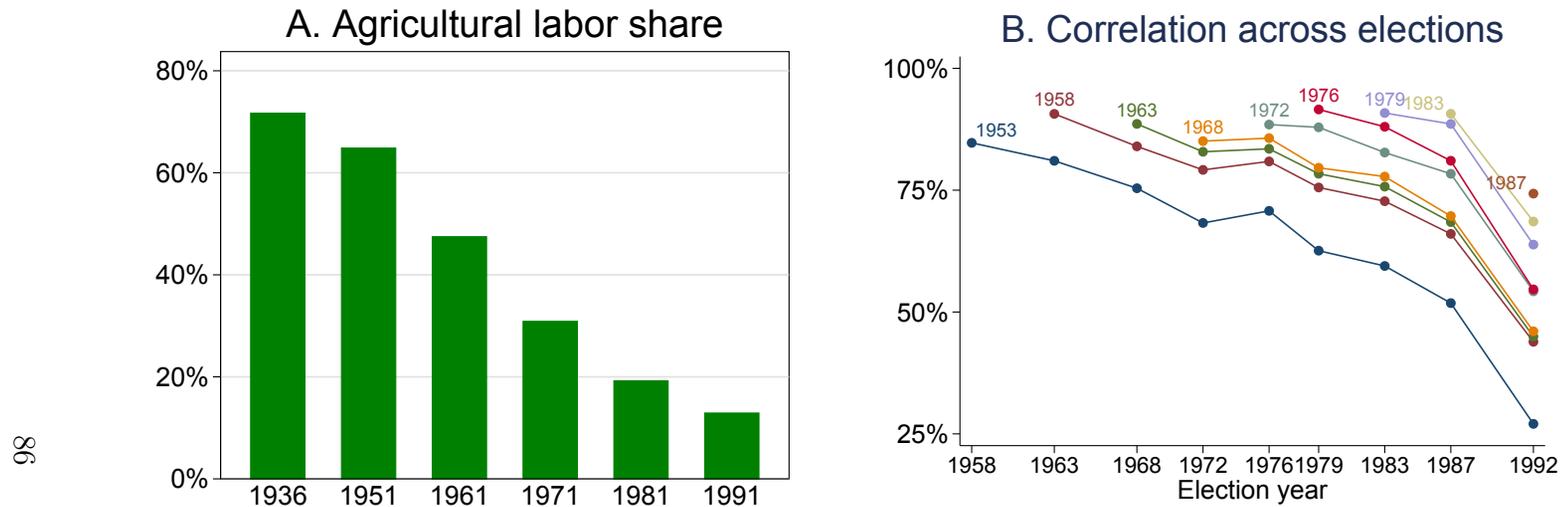
**Table D.7:** The impact of the reform on Forza Italia/center-right after 1992 vote share.

	1994		1996		2001	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Forza Italia vote share</b>						
Treatment	0.001 [0.010]	0.006 [0.009]	0.002 [0.008]	0.005 [0.008]	-0.001 [0.010]	0.004 [0.009]
Mean Y Control Group	0.16	0.16	0.14	0.14	0.23	0.23
Control DC 1948	No	Yes	No	Yes	No	Yes
Observations	490	482	490	482	490	482
<b>B. Center-right vote share</b>						
Treatment	-0.007 [0.021]	0.003 [0.016]	0.003 [0.021]	0.016 [0.014]	0.001 [0.018]	0.011 [0.014]
Mean Y Control Group	0.34	0.34	0.42	0.42	0.41	0.41
Control DC 1948	No	Yes	No	Yes	No	Yes
Observations	490	482	490	482	490	482

*Notes:* The Table reports coefficients  $\beta$  of separate regressions of the RDD specification in Equation (2). Columns 2, 4 and 6 control for Christian Democrat vote share in 1948. Panel A: Dependent variable is *Forza Italia* vote share (Berlusconi's party). Panel B: Dependent variable is vote share of 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. The sample consists of all towns within 25 km to the reform borders of Delta Padano and Maremma. The towns of Bieda (Viterbo province), Colle di Tora, Contigliano (Rieti), Rocca Santo Stefano (Rome), Rosolina (Rovigo), San Vincenzo (Livorno), Santa Luce Orciano (Pisa) and Stroncone (Terni) have missing data in the 1948 elections. Heteroschedastic robust standard errors in parentheses.

## D.7 Changing society

Figure D.5: Share of Agricultural Workers and Correlation across Elections



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.8 Firms Growth

**Table D.8:** Impact of reform on firms number, size and sectoral concentration.

	Number of plants per capita			Number of workers per plant			Herfindahl-Hirschman Index		
	< 25km	< 10km	< 50km	< 25km	< 10km	< 50km	< 25km	< 10km	< 50km
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment*1961	-0.001 [0.003]	-0.000 [0.005]	-0.003 [0.003]	0.314 [0.240]	0.744* [0.410]	0.078 [0.194]	-0.006 [0.018]	0.042 [0.031]	-0.023 [0.015]
Treatment*1971	-0.001 [0.003]	0.002 [0.006]	-0.004 [0.004]	-0.052 [0.270]	0.065 [0.414]	-0.251 [0.218]	-0.013 [0.030]	-0.001 [0.047]	-0.020 [0.025]
Treatment*1981	0.001 [0.004]	0.003 [0.007]	-0.002 [0.004]	-0.263 [0.330]	0.181 [0.496]	-0.425* [0.255]	-0.034 [0.021]	-0.011 [0.036]	-0.037** [0.016]
Treatment*1991	-0.001 [0.004]	-0.004 [0.007]	-0.004 [0.004]	-0.345 [0.303]	0.100 [0.475]	-0.512* [0.263]	-0.023 [0.019]	0.013 [0.033]	-0.020 [0.016]
Treatment*2001	-0.000 [0.004]	-0.002 [0.007]	-0.004 [0.004]	-0.260 [0.338]	0.208 [0.514]	-0.200 [0.277]	-0.027 [0.023]	0.005 [0.040]	-0.012 [0.018]
Mean Y Control Group	0.05	0.05	0.05	2.94	2.78	3.18	0.35	0.34	0.37
Number of Towns	490	222	863	490	222	863	490	222	863
Observations	2933	1329	5171	2933	1329	5171	2933	1329	5171

*Notes:* The Table reports coefficients  $\beta_t$  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, transport, utilities, social services, banking and insurance. Column 1, 2 and 3: dependent variable is the number of plants per capita. Column 4,5 and 6: dependent variable is the number of workers per plant. Column 7, 8 and 9: dependent variable is the Herfindahl-Hirschman Index that measures the concentration between these sectors using the STATA package `hhi5` written by Lian. The omitted category is the  $\beta$  of 1951. 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 km (Column 2, 5 and 8) and 50 km (Column 3, 6, 9) 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.