

# Harvesting Votes: The Electoral Effects of the Italian Land Reform <sup>\*</sup>

Bruno Caprettini      Lorenzo Casaburi      Miriam Venturini <sup>†</sup>

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## Abstract

Governments often implement large-scale redistribution policies to gain enduring political support. However, little is known on whether such policies generate sizable gains, whether these gains are persistent, and why. We study the political consequences of a major land reform in Italy. A panel spatial regression discontinuity design shows that the reform generated large electoral gains for the incumbent Christian Democratic party. The electoral effects persist over four decades. We explore several channels and find that clientelist brokering and patronage are plausible mechanisms for this persistence.

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

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<sup>†</sup>Caprettini (bruno.caprettini@gmail.com): University of St Gallen, Dufourstrasse 50, CH-9000 St. Gallen. Casaburi (lorenzo.casaburi@econ.uzh.ch) and Venturini (miriam.venturini@econ.uzh.ch): University of Zurich, Schonberggasse 1, CH-8001 Zurich.

# 1 Introduction

The political objective of many large-scale redistribution policies is to establish political support (Acemoglu, 2001). It is often maintained that successful redistribution can generate lasting political returns and translate into long tenures in office (cf. Kennedy, 1999, for Roosevelt’s US and Galli, 1993, for post-World War II Italy). However, surprisingly little evidence exists for such long-term political returns (Guriev and Papaioannou, 2021). Moreover, the mechanisms that allow redistribution to generate persistent political support remain largely unexplored.

In this paper, we study the long-term political impact of redistribution of a major asset: land. Land redistribution is a powerful tool to gain political support in agrarian societies: revolutionary governments of 1790s France, 1920s Russia, and 1940s China, among many others, passed ambitious plans of land redistribution during the first years in power. In democratic countries, governments often try to prevent extreme left-wing parties from taking power through land redistribution, as it happened in several Latin American countries after the 1958 Cuban revolution (Binswanger et al., 1995). We focus on a large-scale land reform that took place in Italy in the early 1950s. The Christian Democratic party (*DC*)—the majority government party in Italy between 1946 and 1992—designed and executed the reform, expropriating large landowners and redistributing their land to poor rural workers.

Importantly, the land reform only targeted well-defined reform areas where, contrary to other failed land reforms (e.g., Bazzi et al., 2020), led to substantial land redistribution. This feature allows us to adopt a panel spatial regression discontinuity design and study differential changes in outcomes between treated and control towns close to the reform areas’ borders (Dell, 2010).<sup>1</sup>

The panel spatial RDD result show that DC experienced immediate and long-lasting electoral gains from the reform. In the first elections after the reform, treated towns increased support for DC by two to four percentage points (control mean: 35%). These electoral gains persisted over the following four decades (1953-1992). Treated towns appear to support the DC policy agenda, as well as its candidates: in 1974, treated towns were more likely to follow DC directions in a highly divisive referendum on divorce. These results, which hold in unidimensional and multidimensional RDD specifications and in many robustness checks, provide evidence that the 1950 land redistribution transformed reform towns into DC strongholds for four decades.

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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 key assumption for the panel RDD analysis (i.e. parallel trends at the border) is more likely to hold there. Section 2 elaborates.

We then explore several potential mechanisms behind the long-lasting electoral impact of the reform. First, it is plausible that land redistribution induced gratitude among the beneficiaries (Finan and Schechter, 2012). Anecdotal evidence also suggests that some of the original beneficiaries had a vivid memory of the reform many years later (Zucco et al., 2011). However, gratitude is unlikely to explain long-term persistence, as overtime many beneficiaries died and memory of the reform may have faded. Consistent with this hypothesis, we show that where population was older at the time of redistribution, DC gains tended to diminish over time compared to places with younger population. This suggests that gratitude was not fully transmitted across generations, a result in line with existing literature that points to short-lived electoral effects of other redistribution policies (Bechtel and Hainmueller, 2011; Zucco Jr, 2013; Achen and Bartels, 2017).

Land reforms around the world include elements that limit the benefits for the recipients. This was true in the Italian case, too. First, given the large number of potential beneficiaries, land allocation was, at least in part, discretionary, with administrator close to the governing party reviewing applications for the land. Second, because recipients could not sell nor rent the land for thirty years, the reform created a class of immobile landholders. Third, while the land transfer was hardly reversible, access to fundamental services for the new landowners, including agricultural inputs, credit, and healthcare, was managed by organizations that acted as political brokers (cf. Stokes et al., 2013; Larreguy et al., 2016 and Allum, 1974 for Italy). Because discretionality and conditionality of benefits are well-known elements of clientelism around the world (Hicken, 2011), we next consider the effects of the reform on political brokering and on targeted benefits through public employment, two important components of DC clientelistic system (see e.g. Hicken, 2011; Piattoni, 2001; Allum, 1974).<sup>2</sup>

Political brokers are a crucial element of many clientelistic systems (Hicken, 2011). In the context of Italy, DC relied on the work of *Coldiretti*, an association of small farmers that like similar associations around the world helped the party mobilize voters during elections (Crainz, 1982; Lanza, 1991). We collect novel data on the penetration of these political brokers at the local level: RDD analysis of two distinct databases indicates that in treated towns these brokers enjoyed greater support and controlled larger economic resources. The land reform increased not only the potential member base of this organization (i.e. the number of small landowners), but it also deepened its penetration among small landowners. According to a back-of-the-envelope calculation, farmers who received land through the reform were

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<sup>2</sup>Targeted benefits to core constituencies are consistent with distributive politics (Cox and McCubbins, 1986; Dixit and Londregan, 1996), as well as with clientelism. While we cannot document whether in the land reform areas these targeted benefits were contingent on electoral support, many scholars have shown that patronage was an important element of DC consensus (Golden, 2003).

52% more likely to support the political brokers of the land reform party than pre-existing farm owners.<sup>3</sup> This suggests that in reform towns *Coldiretti's* growth went beyond the mechanical effects resulting from the increase in small farms and that instead the organization had greater appeal among the new farm owners created by the reform. The disproportional presence of political brokers induced by the reform in treated towns may have facilitated a system of repeated exchange between voters and politicians.

We also study a second form of targeted benefits: patronage in public employment. After the reform, treated towns employed a greater share of workers in the public sector, a common form of patronage in Italy (see, e.g., Alesina et al., 2001). While these effects are not always robust across specifications (e.g., they are stronger in the multidimensional specification than in the unidimensional one), they always indicate large and significant effects in the 1980s, when DC patronage became rampant (Allum, 1997). This behavior is in line with models of political redistribution where parties target their core constituencies (Dixit and Londregan, 1996).

The reform may also affect long-term electoral outcomes through its impact on economic development. We do find an effect on out-migration: in the decades following the reform, treated towns experienced a differential fall in population. Two features, however, suggest that migration patterns are unlikely to explain the electoral results. First, the changes in population are small in the first few years after the reform and grew overtime; in contrast, DC support increased sharply in the first election after the reform and remained remarkably stable over four decades.<sup>4</sup> Second, in the first elections after the reform, there was an increase in the *absolute number* of votes for DC, which cannot be explained by out-migration of opposition supporters. Immigration of DC supporters is also unlikely to explain the result, as the vast majority of land beneficiaries were poor rural workers already residing in reform towns. Instead, the increase in DC votes suggests that the reform persuaded voters in reform towns. We also document that the land reform had no significant impact on income, wealth, structural change, and firms' dynamics.

Finally, land redistribution may also have affected voting because land ownership changes voters' beliefs and attitudes. A large literature shows that wealth is an important determinant of preferences for redistribution (Alesina and Giuliano, 2011; Fisman et al., 2015; Piketty, 2018), and that these preferences in turn affect voting (Fisman et al., 2017). Existing work

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<sup>3</sup>This is reminiscent of Larreguy (2013), who shows that Mexican brokers in control of communal land overlapping entire electoral districts are able to exert greater control over voters.

<sup>4</sup>We also show that sectoral, gender and age composition of treated and control towns remained similar after the reform: this suggests that migrants were similar to stayers in terms of these observables and makes selective migration less plausible.



also shows that land titling programs may make beneficiaries more pro-market (Di Tella et al., 2007; De Janvry et al., 2014). We examine this channel in our setting and find little evidence for such mechanism. First, economic conservatism cannot explain why in treated towns voters support DC agenda in the divorce referendum. Second, the reform did not affect income in 1981 nor home-ownership rates between 1961 and 2001. This result speaks against the idea that people living in treated towns earned more or were wealthier. Third, we look for evidence of economic conservatism in the post-1992 elections, when Berlusconi's party, *Forza Italia*, ran on a right-wing platform based on tax cuts and other conservative economic policies. We find no evidence of greater support for *Forza Italia*, or for other right-wing parties, in treated towns. This result suggests again that greater economic conservatism cannot explain the lasting support for DC. Taken together, these additional results provide little support for persistent changes in political preferences driving our results.

Our results suggest that clientelist brokering and patronage may have been important determinants of the persistent electoral effects of redistribution. As is often the case in analysis of long-term impacts (e.g. (Dell, 2010)), two caveats apply. First, we suggest a plausible mechanism, but other channels, unobservable to us due to data limitations, may have also played a role. For example, it is hard to measure systematically gratitude and reciprocity among beneficiaries, and our results there can only be suggestive. Second, the short-term results on voting likely capture the direct effects of the reform, but the long-term effects naturally compounds direct effects of redistribution with the indirect effects of intermediating channels. The crucial identification assumption is that these changes in intermediating channels are induced by the reform. For example, it must be the reform that increases Coldiretti's strength, not other unrelated factors. Under this assumption, our long-run analysis identifies reduced-form effects of the reform and allows us to document how clientelist practices interact with redistribution policies, a novel contribution.

Our paper thus contributes to two main strands of literature. First, credibly identifying the electoral impact of redistribution policies is challenging and there is little work over their *long-run* electoral effects. Existing empirical studies mostly looks at electoral impact of various fiscal transfer programs over few election cycles (e.g., Bechtel and Hainmueller, 2011, Manacorda et al., 2011, and Zucco Jr, 2013). In the context of land redistribution, few papers have looked at short-term electoral outcomes, exploiting the gradual rollout of the reforms in a difference-in-differences framework (see, e.g., De Janvry et al., 2014, Larreguy et al., 2018).<sup>5</sup> Since in these cases redistribution eventually expands to initial control areas,

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<sup>5</sup>Several authors look at the political economy of land reforms (e.g., Bardhan and Mookherjee, 2010 and Bhattacharya et al., 2019). Several public policies contain elements of redistribution that may affect voting.

it is harder to study the long-term effects of the reforms with these methods. Relatedly, González (2013) studies the effect of the 1960s Chilean land reform on electoral outcomes over nine years, in a design leveraging correlation between pre-reform land concentration and land reform intensity. In contrast with these studies, our spatial regression discontinuity design enables us to identify the impact of the reform over several decades. The long-term analysis of our study is similar to Dell (2012), who uses an IV based on rainfall to study the impact of the Mexican revolution insurgency and subsequent land reform on long-run development. Relative to these works, we are the first paper to document the electoral impact of redistribution over several decades.<sup>6</sup>

Second, scholars of clientelism have long studied the effect of land ownership on clientelistic systems (Baland and Robinson, 2008). This literature has emphasized how landed elites exploit access to land to influence rural workers and to perpetuate clientelistic exchanges (see also Anderson et al., 2015 for India). Relatedly, in Mexico, Larreguy et al. (2018) finds that assigning land property rights decreases the vote share of incumbent parties in local elections, arguably because it reduces their control over clients who occupy land illegally. In contrast, in our setting, the land reform appears to have strengthened clientelistic brokers, arguably because it facilitated new forms of control over beneficiaries. In particular, new landowners started to rely on DC brokers for farm support, a dependence that may have perpetuated their vulnerability (Shefter, 1977; Bobonis et al., 2022).<sup>7</sup> Thus, our results indicate that land may not be the only input that allows politicians to establish clientelistic relationships.

## 2 Background

### 2.1 The 1950 Land Reform

*The Road to the Reform.* At the end of World War II, Italian agriculture was backward and poor. In 1948, out of almost 8 million agricultural workers, around 2.5 million were

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See for example Acemoglu et al. (2021) analysis of the long-term electoral effects of redistribution policies, focusing on educational reforms in Norway.

<sup>6</sup>Our results also relate to the broad literature on persistence (Voth, 2021). Unlike much of this literature, we are also able to document at which point the electoral persistence disappears: we find no differential support for other Christian Democrat and center-right parties after a major corruption scandal in 1992 upended the Italian political system.

<sup>7</sup>Important studies of clientelism include Brusco et al. (2004); Stokes (2005); Kitschelt et al. (2007); Acemoglu et al. (2011); Robinson and Verdier (2013); Cruz et al. (2017); Fergusson et al. (2022) and Piattoni (2001). See Hicken (2011) and Bardhan and Mookherjee (2016) for reviews. See Galli (1993); Marzano (1996) and Giovagnoli (1996) for studies of DC and Buonanno et al. (2017); Fontana et al. (2018); Carillo (2018) and Durante et al. (2019) for recent political economy studies of Italy.

landless rural workers (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. At the time of the 1948 political elections, it was still unclear which (if any) of these proposals would be enacted. Only at the end of 1949 it became clear that the new DC government would redistribute some land, a move designed to prevent a Communist coalition between rural and urban workers (Segni, 1952).<sup>8</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 experts drew these areas based on agricultural productivity, land distribution, and the need for agricultural investments (Negri, 1949; Canevari, 1949; Servizio Statistica UNSE, 1949).

The initial *legge di riforma agraria* was never approved. Instead, the DC government presented to the Parliament a second proposal, the *legge stralcio*, eventually enacted in October 1950.<sup>9</sup> The *legge stralcio* targeted only the highest-priority regions in the original proposal (roughly “Zone B” of the *legge di riforma agraria*, see Salomone, 1950) 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>10</sup>

Politics influenced the definition of borders in South Italy. 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), while towns with landlords close to DC successfully lobbied to exclude their town from the reform (Calasso, 1952). Overall, in the South the Government followed only loosely the indications contained in the *legge di riforma*

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<sup>8</sup>Acemoglu and Robinson (2000) develops a model where *élites* extend the franchise to avoid a revolution.

<sup>9</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>10</sup>The areas were: Delta Padano (North East), Maremma (Center-West), Fúciño (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.

*agraria*: it excluded 40% of the 1.6 million hectares originally included in the proposal and included a group of towns not contiguous to the rest of the reform (Ruini, 1951; Ministry of Agriculture’s Spokesman, 1951)<sup>11</sup>. 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 political manipulation appear less important. For instance, private, confidential records of the then Ministry of Agriculture, suggest limited interest in adjusting borders for political gain, citing technical and financial concerns which did not feature in the discussions over Southern borders. The same records also reveal frustration of DC politicians for the *absence* of political manipulation in the North.<sup>12</sup> 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).

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 own party. The formal tests in Section 4 support these historical accounts by providing evidence on parallel pre-trends.

***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). It also prevented landowners’ opportunistic behavior, by invalidating sales and donations of land that occurred after January 1948. Landowners received compensation for the property lost in the form of 25-year fixed-rate government bonds yielding 5% a year—a form of compensation that many landowners opposed (Capua, 1950; Pecoraro, 1950). After enacting the law, the government implemented the reform quickly: it expropriated all land before April 1953 and

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<sup>11</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).

<sup>12</sup>These documents were recently made publicly accessible (Mura, 2017). Appendix Figure A.2 shows documents in which DC politicians complained with the Ministry for the exclusion or inclusion of reform towns in the North (Panels A, B and C) as well as the Ministry’s replies (Panel D and E).

redistributed it shortly thereafter (cf. Segni, 1952; Russo, 1955).

The reform expropriated around 18% of agricultural land in reform areas in the North and around 13% in the South (Marciani, 1966). Rural workers who wanted a plot of land had to apply through one of the Reform Boards, and they purchased the estate with the help of thirty-year public loans at generous rates (3.5%). They could not re-sell nor rent out 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 residents of the town where the land was located (Dickinson, 1954, Rossi-Doria, 1958, Marciani, 1966).

DC maintained firm control of the whole land redistribution process. Responsibility for land allocation fell on the Reform Boards, whose directors were appointed by the Ministry of Agriculture. During the crucial years of land redistribution, every president of these agencies was a prominent DC personality (e.g. Rossi in Delta and Medici in Maremma). These agencies were relatively free to select beneficiaries: although the law specified eligibility criteria to receive the land, applications exceeded available land and agencies had to decide among many qualified applicants (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). Strategic land allocation may thus have created the pre-conditions to start the clientelistic exchange. For instance, one of the land beneficiaries leaders said during an assembly in 1955: “My fellow farmers, [...] at the polling stations we have to show our reciprocity, our gratitude and our loyalty to [DC]” (Ufficio DC, 1956).

As is often the case in land reforms around the world (see, e.g., King, 2019; Wolfe, 2017), also in Italy land redistribution came with land improvement projects. These projects included land reclamation, irrigation, roads and housing for the new landowners. Reform Boards managed these projects: between 1951 and 1961 they spent 583 billion lira (about 0.5 percent of 1951 Italy GDP). Maremma and Delta received about 15 percent of these

funds (Barbero, 1960). The intervention represented a redistribution in favor of poor farmers and our evaluation studies the electoral effects of the entire bundle. We do not attempt to disentangle the specific impact of land improvement projects.

## 2.2 Fifty years of DC rule

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

***Political Brokers.*** Grassroot associations acting as political brokers were crucial for DC. Most relevant to our context were the associations of small landowners. While several such associations existed, the largest one, *Coldiretti*, was tightly connected to DC: it routinely campaigned for the party (Appendix Figure A.5) and its leaders served as MPs with DC. *Coldiretti* coordinated a network of farmer cooperatives (Primavera, 2018) and offered farmers many services, including input purchases, output sale, credit, fiscal assistance, and healthcare. While the land reform was irreversible, subsequent benefits such as credit and health care were discretionary and easier to withdraw. *Coldiretti* played a major role as political broker in managing the provision of these reversible benefits. The organization was particularly influential among the beneficiaries of the land reform, in part because *Coldiretti* leaders held several managerial 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 DC government in a healthcare reform four years after the land reform.<sup>13</sup> These local health centers collected health insurance contributions from their members, received additional financial support from the central government, 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. Control of these institutions gave real power, as they had to approve hospitalizations and provision of specific services. Thus, to access healthcare, the farmers had to interact with local *Coldiretti* leaders, who used these targeted benefits to increase *Coldiretti* membership and mobilize voters during electoral cam-

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<sup>13</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.

paigns (Luzzi, 2004; Lanza, 1991). In Section 6.2 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 practice became arguably more common during the 1980s (Allum, 1997). In Section 6.2 we use public sector employment as an indicator for local incidence of patronage.<sup>14</sup>

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

### 3 Data Sources

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

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

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

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<sup>14</sup>Anecdotal evidence also suggests that DC politicians often engaged 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 both to reward core constituency. For this reason, in Section 6.2 we investigate this practice by studying discretionary transfers from the central government to municipalities.

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

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

***Agricultural Variables.*** Data on land distribution before the reform come from Medici (1948), who collected farm-level information on the value and size of land in 1948. The publication was commissioned by the Italian Parliament and served as the basis of the discussion of the land reform. From the original publication, we digitized Table II, which reports for each town the number and the value of estates broken down by 11 separate categories of taxable income. We follow the land reform rules and consider estates that could be expropriated as those in one of the top 4 categories. All estates in these categories were worth at least £20’000, and the reform bill prescribed expropriation for estates worth £30’000 or more (Appendix Figure A.3). The 1961 agricultural census includes tables with the number of farms by type of management (e.g. owner-operated, tenant farming, share-cropping). We digitize these tables from the original volumes of the census. We find the same variable for a subset of our towns in the 1929 land registry, 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 Alleanza Nazionale dei Contadini (1970). We digitize the 1965 budgets of all *Casse Mutue* from Federazione Nazionale Casse Mutue (1966). We measure the relative size of *Casse Mutue* with total revenues and total expenditure per capita (from 1961 population census). Two towns have missing budgets, 81 have missing voting records and 82 have missing information on the votes to Bonomiana: these missings account for the smaller sample sizes in these regressions.

***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 (other years are either lost or recorded at province level). 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 1991 are from decadal population and economic censuses, including the newly digitized population census of 1936 and population and economic censuses of 1951 and 1961. Income information was not collected in the early decadal censuses. We digitize town-level income in 1981 from Marbach and Ciapparelli (1983) which is the earliest collection of town-level income. We calculate the distance to the coast and to Rome based on the 1951 map of towns. Potential yield of wheat and maize is from FAO (2015) and elevation and slope from USGS (2005).

## 4 Empirical Strategy

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

### 4.1 Panel Spatial Regression Discontinuity

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

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

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

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

Our parameters of interest are the  $\beta_t$  year-specific treatment coefficients. The model also includes town fixed effects,  $\eta_i$ , and reform area  $\times$  year fixed effects:  $\eta_{rt}$ . Throughout the paper, we report two main specifications for the function  $f_t$ : a linear function in distance from the reform area border, which we allow to vary on the two sides of the border, and a quadratic polynomial in latitude and longitude, estimated separately for the two reform areas included in the sample (Maremma and Delta Padano). In both cases, we estimate these functions for each election year.<sup>15</sup> The unidimensional model identifies the discontinuity in the most parsimonious way, while the multidimensional model allows geography to have non-linear effects while avoiding overfitting (Dell, 2010; Gelman and Imbens, 2019). In Section 5.3 we also report results from specifications between these two extremes. We cluster standard errors by town and show robustness to spatial autocorrelation.

We derive the optimal bandwidth for the Christian Democrat vote share in both the unidimensional and multidimensional RDD. First, we consider distance to the reform border as running variable and obtain an optimal bandwidth of 20 km, following Calonico et al. (2014). Second, for the multidimensional regression discontinuity design, we obtain an optimal bandwidth of 35 km, following Keele and Titiunik (2015).<sup>16</sup> In addition, since there is no consensus on how to compute optimal bandwidths in panel RDD, we report for both specifications results for alternative bandwidths between 10 km and 50 km. The narrowest bandwidth is half the shortest optimal bandwidth and provides very conservative estimates. Figure 1-Panel B shows 20 km buffers inside (dark red) and outside (orange) the reform areas we consider in the analysis.<sup>17</sup> These buffers include 406 towns.

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

<sup>16</sup>For both approaches, we first derive optimal bandwidth (Calonico et al., 2014) separately for each election year. In the unidimensional case, 20 km is both median and mean across years (range: 15-23 km). When we pool observations for all years, we obtain 21 km. For the multidimensional case, following Keele and Titiunik (2015), we estimate optimal bandwidths at different points on the border: across all years we obtain a median optimal bandwidth of 35 km. Range is wide and average higher (81 km), as optimal bandwidth is large in border points with low density of observations (as explained in Keele and Titiunik, 2015). To be conservative, we consider the median and not the average.

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

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

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

## 4.2 Testing the Identification Assumptions

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

### 4.2.1 Parallel Pre-Trends at the Border

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

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

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

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

***Christian Democrat Vote Share: 1946-1948.*** We first present estimates of Equation (2) when the outcome variable is the change in vote shares of DC. In both specifications,

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<sup>18</sup>To our knowledge, there is no standard test to check the smoothness of the density when the discontinuity is multidimensional. Appendix C.2 discusses the result of the local polynomial density estimator of Cattaneo et al. (2020) on the distance to the border.

we find evidence of parallel pre-trends between treatment and control towns at the border. The coefficient on the DC pre-trend is never significant and, with the exception of very small bandwidths, it is always lower than 0.02, from a control mean of 0.12. At the optimal bandwidths the coefficients are 0.004-0.009 ( $p$ -value: 0.6). Figure 4-Panel B presents a bin scatter and shows graphically the continuity at the border.<sup>19</sup>

One concern is that only two elections took place before the land reform and after World War II. To address this concern, we look at pre-Fascism elections in 1919, 1921 and 1924. We proxy the support for the Christian Democratic party with votes for the Italian Popular Party (PPI), the Catholic party to which most DC founders belonged before the war. As the DC, the PPI had a strong connection with the Church (his founder and leader was a priest) and aimed at organizing Catholic voters. Appendix Figure C.3 reports treatment coefficients from a panel RDD regression with all elections between 1919 and 1948; we report the two specifications with the distance (Panel A) and coordinate polynomial (Panel B). Panels C and D estimate a single coefficient for all pre-1946 elections for every bandwidth between 10 and 50 km. Across specifications and bandwidths, parallel trends hold over the 30 years preceding the land reform.

**Census Variables.** We digitized town-level data from the 1936 and 1951 population censuses. The bottom half of Figure 2 shows parallel pre-trends both in population and employment variables. Point estimates for the changes in the share of active population and for employment shares in agriculture and public sector are never significant at conventional levels. Log population and manufacturing share exhibit a handful of significant coefficients in the graphs, but the patterns are not systematic across bandwidths and specifications, and most of the coefficients are small and insignificant.

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

**Balance.** Since town fixed effects capture time-invariant differences, our panel RDD design strategy does not require balance in levels. Nevertheless, in Appendix Figure C.1, we also test for balance on the two sides of the border at the time of the reform for the following variables: *i*) land distribution, including the share of expropriable estates and the share of

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

farms and land managed by the owner (i.e., a proxy for small-scale cultivation);<sup>20</sup> *ii*) vote shares for all the major parties in 1946 and 1948, the two parliamentary elections before the reform, and for the 1946 mayor elections; *iii*) 1951 census variables, including population, labor force participation, sectoral employment share, and plants per capita, as well as 1952 fiscal transfers; *iv*) geographical variables, including town area, elevation, crop suitability and distance from coast and Rome.

Overall, at the border, treatment and control towns are similar in terms of baseline land distribution, vote shares and economic variables. Balance in these key outcomes provides additional confidence in our research design.<sup>21</sup> We do observe unbalance in some geographical variables (e.g., town area, distance from Rome, and cereal suitability). However, none of these differences is robust across specifications (distance and coordinates). Importantly, town fixed effects in our panel RDD absorb all time-invariant differences such as geography.

## 4.2.2 Contemporary Policies

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

These contemporary policies do not represent a threat for our empirical design. First, many of these policies targeted entire provinces or large urban centers: for instance, the *Cassa del Mezzogiorno* mostly followed province borders, and the *Piano Casa* built homes in major cities. The land reform instead targeted a subset of towns in some province and

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

<sup>21</sup>To the best of our knowledge, town-level data on *Coldiretti* membership do not exist (Lanza, 1991 p.58, footnote 36). In Section 6.2 we measure the presence of *Coldiretti* at town level indirectly, using the results of the elections of *Casse Mutue*. Three observations suggest balance of *Coldiretti* before the reform: first, small landowners are balanced in 1929, which implies that reform towns did not have more potential *Coldiretti* members before 1951; second, the balance in DC vote shares in 1946-48 speaks against greater activity of the association in treated towns before 1951 (since *Coldiretti* was very effective at promoting DC candidates); third, the strongest tool that *Coldiretti* had to influence its members were the *Casse Mutue*, which were established only after the reform, in 1954. Fourth, aggregate data cited in Lanza (1991) indicate that after the war *Coldiretti* was primarily concentrated in the South, and experienced the fastest growth during the 1950s.

our results are robust to the inclusion of province fixed effects. Second, Appendix Figure C.4 shows that exposure to these policies did not change discontinuously at the borders of the land reform.<sup>22</sup>

### 4.2.3 Stable Unit Treatment Value Assumption

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

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 beneficiaries 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.11 presents the analysis in detail. While we acknowledge that none of these tests is perfect, we find it reassuring that they consistently fail to find evidence of this type of spillovers.

### 4.2.4 Discussion

The evidence of this section suggests that the identification assumptions required for the panel Regression Discontinuity hold. Pre-trends are parallel at the border for voting, patronage and other economic variables. In addition, while the panel approach does not require balance in levels, it is reassuring that towns located just inside the reform area were broadly similar to those just outside. Finally, we find no evidence that contemporary policies affected differentially treated and control towns at the border.

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<sup>22</sup>In addition, Maremma did not overlap with *Cassa del Mezzogiorno* target areas. In the 20 (35) km bandwidth, only 2 (22) control towns were included in the *Cassa del Mezzogiorno*: respectively 0.6% and 4% of the control group.

<sup>23</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 (cf. Section 2). Section 6.3 discusses migration in depth.

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## 5 The Electoral Effects of the Reform

This section presents the main results of the paper. As a preliminary step, we show that the reform altered land distribution. We then show the electoral effect of the reform, using the spatial panel RDD described in Section 4: in the first elections after the reform, treatment towns experienced a sharp increase in the support for Christian Democrats, which then persisted for four decades. In addition, we document the effects on DC policy agenda, by looking at a contested 1974 referendum on divorce. We also provide evidence on the end of the electoral benefits of the reform, after a major corruption scandal upended the Italian political system in 1992. Finally, we discuss several robustness checks.

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

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

Figure 3 shows the effect of the reform on the share of farms (Panels A-B) and on the share of land (Panels C-D) managed by the owner, using the two specifications of the RDD model in Equation 2. We report coefficients for bandwidths from 10 to 50 km. Treated towns have on average between 10 and 15 percentage points (p.p.) more owner-operated farms, from a control average of about 70 percent. The effect is significant and stable across bandwidths in the distance specification and grows with the bandwidth in the coordinate specification. Similarly, in treated towns, the share of land in owner-operated farms is 10-15 p.p. higher than in control towns, from a control average of 41-47 percent. These results are not affected by controlling for the value of this variable in 1929 (Appendix Figure D.4) as this was balanced between treatment and control (see Appendix Figure C.1). Taken together, these results indicate that the reform significantly increased the presence of small-holding agriculture in treated areas.

### 5.2 The Effect of the Reform on DC Support

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

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

***Panel RD Estimates.*** Next, we estimate the Panel RDD described in Equation (1) for elections 1946-92. Figure 5-Panels A-B report point estimates and 95% confidence intervals for  $\beta_t$ : the effect of the land reform on DC vote shares in every election. Panel A presents estimates controlling for distance to the border and Panel B for the polynomial in coordinates. The sample in Panel A (B) consists of 406 (647) towns within 20 (35) km from the reform border in the North. Twenty and thirty-five km are the optimal bandwidths in the two specifications: Figure 2 and Figure 5-Panels C and D show that at these bandwidths DC vote share was on parallel pre-trends in 1946-48 ( $\beta$ : 0.004-0.008;  $p$ -value: 0.6). The baseline year is 1948, the last election before the reform. The treatment coefficient in the 1953 election suggests that in treated towns DC vote share increased by between 2 and 4 p.p. during the first election after the land reform, from a control mean of 36.4%: an increase of 4 to 11 percent. The effect is larger in the unidimensional specification with distance, but precisely estimated in both specifications.

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 anecdotes and descriptive statistical evidence produced at the time. Amintore Fanfani, a DC leader, noted in 1956 that “in the reform areas, the *Scudo Crociato* [the DC symbol: a crusader shield] shines while the hammer and sickle rust” (Ufficio DC, 1956). A 1957 academic study records the gains that DC made in the elections following the reform (Toldo, 1957). In one dramatic episode, 220 PCI members who received land, publicly tore down their membership cards and joined DC (Il Mattino, 1951). More broadly, the impact in the first election is consistent with the literature of the short-term electoral impact of public policies (Manacorda et al., 2011; Zucco Jr, 2013).



DC gains remain large and stable 1992 (Figure 5-Panels A-B; in 1953-92, the average DC vote share in control towns ranges between 29% and 36%). During the four decades following land redistribution, the land reform quickly lost prominence in political debates and Italian society changed profoundly. Nevertheless, DC maintained firm control of the Italian government throughout the period. The long-term persistence of the voting effects of the reform contrasts with the short-term electoral impact of other redistribution policies (Bechtel and Hainmueller, 2011). We explore potential mechanisms for this persistence in Section 6.

Figure 5-Panels C-D reports the pooled (1951-1992) treatment coefficients and pre-trends (1946) of the panel RDD with town fixed effects (base year 1948) for bandwidths between 10 and 50 km. Across bandwidths, the effect is stable for both specifications (top panels). Consistent with Figure 2, there are some non-significant pre-trends at very small bandwidths (10km is half of the optimal bandwidth in the distance specification and less than a third in the coordinate one). However, these patterns do not drive the results: the coefficient of interest is very similar between small and large bandwidths.<sup>24</sup>

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

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

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<sup>24</sup>Contrary to Figure 2, in Figure 5-Panels C-D we estimate pre-trends with town fixed effects.

<sup>25</sup>1992 elections are an exception. In 1992, turnout fell substantially, possibly in response to early scandals revealing corruption within the major parties. The decrease was stronger in treated towns (marginally significant).

<sup>26</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:  $dc^T - dc^C = 0.043$  or  $0.033$ , the two RDD estimates of the effect of the reform on DC vote share;  $dc^0 = 0.43$  or  $0.44$  the share of DC in control towns before the reform in the 20 and 35 km bandwidth;  $b^C = 0$ , the share of beneficiaries in control towns;  $b^T = 0.10$ , 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

0.73 (0.56): out of four people who received land and were not already DC voters, almost three started voting for DC as a result of the reform (one out of two). The effect is large but plausible, given the magnitude of the asset transfer. We note that positive spillover on non-beneficiaries (e.g. on beneficiaries' relatives) would imply a lower persuasion rate. In contrast, if envy and resentment among non-beneficiaries *living in treated towns* lead them to stop voting for DC, this would imply a higher persuasion rate.

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

Using the panel RDD approach of Equation (1), we test whether support for the repeal of divorce in the 1974 referendum was higher in treated towns. In this exercise, we include the referendum results in our panel RDD, and estimate the effect of the reform on the 1974 referendum along with its effect on Parliament elections by decade. Appendix Figure D.12-Panels A-B shows that repeal received between 3.2 and 3.4 p.p. more preferences in treated towns, from a control mean of 40-41%. The effect is quantitatively sizable and statistically significant ( $p=0.075$  in the distance specification and  $p=0.002$  in the coordinate specification). Appendix Figure D.12-Panels C-D shows that these effects are relatively stable across bandwidths and specifications. Overall, these results suggest that treated towns did not vote with DC only out of economic interest and instead aligned themselves more broadly with DC agenda.<sup>27</sup> Section 6.5 elaborates on this point.

***The effect after 1992.*** In 1993, a major corruption scandal led to the break-up of DC. Newly created Christian Democratic parties participated in the elections of 1994, 1996 and 2001. The symbols, platforms and politicians of these new parties came directly from the former DC party. However, none of these parties gained a majority. In Figure 5-Panels A-B we aggregate the votes of post-1993 DC parties and plot the treatment effect in blue. We do not find significant support for Christian Democrat parties in reform areas in 1994-2001: by

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votes, so that the average town has  $(244 - 7) \times 3 = 711$  net beneficiaries. There are 6900 (6800) voters in the average town within 20 (35) km from the border, so net beneficiaries over voters is about 0.10. Calculations with median number of voters (2600) still yield persuasion rates larger than in the literature: 0.21 to 0.27. Three voters per family appears conservative in our context: in 1951 the average family counted 4 members, and most families would not have more than two eligible voters. Nevertheless, larger families would imply smaller persuasion rates.

<sup>27</sup>Divorce was still rare in 1981 (0.25% of couples). Moreover, divorces in 1981 are continuous at the border (Appendix Figure D.13) indicating that voters on the two sides of the border did not have different individual interest in seeing divorce repealed.

2001 the DC advantage halves in the coordinate specification and entirely disappears in the distance specification. In the next section we show that the new right-wing parties did not inherit DC support in reform towns, possibly because they had not established a relationship through a large redistribution policy. The dramatic change to the Italian political system after 1992 invites caution when interpreting these results.<sup>28</sup> Nevertheless, they are consistent with explanations emphasizing the ability of the incumbent party to maintain electoral support while in power. Along these lines, in the next section we explore the role of clientelism and distributive politics.

To summarize, the reform generated persistent political benefits for the Christian Democrats. Voters supported not only DC candidates, but also its policy agenda, as shown in the analysis of the divorce referendum. After the 1992 corruption scandal, these electoral benefits for Christian democrats decreased substantially.

### 5.3 Robustness

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

***Alternative Specifications.*** In Appendix Tables C.1 through C.4, we experiment with different specifications. Panels A report results for DC vote share. In col. 1 of these tables we report the main specification (1) at the optimal bandwidth (20 km when controlling for a linear polynomial in distance and 35 km with a quadratic polynomial in latitude and longitude). We then estimate variations of this specifications: Table C.1 and C.2 drop all provincial seats including Rome (col. 2); control for a 2<sup>nd</sup> order polynomial in distance (col. 3 of C.1) or a linear polynomial in coordinates (col. 3 of C.2); interact the linear polynomial in distance with reform area dummies (col. 4 of C.1) or we no longer allow the effect of coordinates to vary by reform area (col. 4 of C.2). In the remaining columns of Table C.1 and C.2, we include three different fixed effects  $\times$  decade: eight electoral districts (col. 5), ten segments (col. 6), and twenty-nine provinces (col. 7). Tables C.3 and C.4 demonstrate the robustness of our estimates to controlling for predetermined characteristics that are sometime unbalanced: we control for each of these variables separately (cols. 2–10) and then add them in the same regression (col. 11). Results from these specifications are robust and similar to

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<sup>28</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.

our baseline: if anything, more parsimonious specifications such as a linear polynomial in coordinates point to stronger effects.

We also split the reform border into 10 segments and assign every town to one of these segments (Appendix Figure C.7). We then estimate the effect of the reform after dropping towns close to each of these 10 segments. Figure C.8, Panels A-B presents the results for DC. While reducing the sample sometimes affects the significance, no specific section of the reform border appears to drive the results.

***Instrumental Variable: the Borders of the Original Legge di Riforma Agraria.*** Reform areas that redistributed land largely followed the definition of “Zone B” spelled out in the *legge di riforma agraria*. As we discuss in Section 2, these original reform areas were drawn with the inputs of agricultural technical consultants, with no evidence of strategic manipulation. Later changes in the border definition were minor (8%). Here, we show that our results hold when we consider the original borders of the *legge di riforma agraria* as instruments for the actual reform borders. Appendix Tables C.5 and C.6-Panels A report reduced form, first stage and IV estimates for DC shares and confirm the electoral effects of the reform on DC vote shares when using the original reform area. The tables, however, also suggest some differential pre-trends in vote shares before 1950 when using the border of the original reform area. Because we find no pre-trends when using the actual reform borders, we use these in the main specification.

***Placebo Borders.*** Appendix Figure C.9 presents the results of the following experiment. We simulate 20 fictitious reforms, by moving the reform border inside and outside the reform area in steps of 2 km. For each of these fictitious reforms, we estimate a single coefficient for the impact of the reform. Figure C.9-Panel A plots the 20 coefficients of these regressions (on the y-axis) against the location of the fictitious border (on the x-axis). In the same graphs we also report the real coefficient (in red). Figure C.9-Panel B plots the  $t$ -statistics of these coefficients. Both coefficient and  $t$ -statistics are highest when estimated on the real 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. The results suggest that the actual border of the reform is the only source of discontinuity in our sample.

***Standard Errors.*** Our results are also robust to alternative inference approaches. Appendix Table C.9 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.9. Correcting for spatial correlation has no material impact on the significance of the reform on DC vote (col 1). Appendix Table C.10 presents  $p$ -values calculated following Calonico et al. (2014) and Calonico et al. (2020), which compute bias-corrected standard errors for unidimensional RDDs on a cross section (they are not available for panel RDDs nor for multidimensional RDDs). To produce these SEs, we estimate treatment effects in the cross-section of Italian towns using the average change in DC vote between 1946-48 and 1953-92. Table C.10 shows significant effects ( $p$ -value: 0.04) also with bias-corrected robust standard errors.

## 6 Mechanisms

Since the Italian land reform never extended to control areas, our empirical strategy allows us to credibly trace the impact of the reform over several decades. In Section 5 we showed that the reform created persistent electoral gains for DC. In this section, we consider several potential mechanisms for these effects including gratitude and reciprocity, clientelistic practices and distributive politics, migration, growth and development, and voters' conservatism.

### 6.1 Gratitude

Gratitude is perhaps the most natural explanation of our effects, and it likely played a role in 1953, when the land reform was a major campaign issue. However, the electoral effects of public policy tend to be short-lived (Bechtel and Hainmueller, 2011; Zucco Jr, 2013; Achen and Bartels, 2017), and we expect gratitude to weaken overtime, as memories fade and original beneficiaries pass away. Indeed, the impact of the reform diminished as beneficiaries grew older. In Appendix Table D.1 we use the 1951 median share of over 65 (8%) to split our sample, and allow the treatment effect to vary across the two samples of towns. In the 1950s and 1960s age structure did not matter for how the reform was received. From the 1970s however, treatment effects become smaller in towns that had older population at the time of the reform: by the 1980s, the reform coefficient in towns with an above-median share of over 65 at the time of the reform was half the size of younger towns. This is consistent with the fact that by the 1980s many of these voters were dead and their children did not feel as indebted to DC as the parents. Because gratitude was not transmitted across generations perfectly, it cannot explain the persistent impact of the reform over 40 years. We now turn

to one mechanism that may help explain such persistence: clientelism.

## 6.2 Clientelistic Practices

The impact of large-scale redistribution policies on clientelism is *a priori* ambiguous. On the one hand, redistribution may reduce poverty and break down clientelistic networks (Stokes, 2005). On the other hand, politicians may implement redistribution in ways that make voters more dependent on them, in an attempt to increase political support (Robinson and Verdier, 2013). In the Italian case, several features of the land reform—discussed in detail in Section 2—limited its benefits and increased beneficiaries’ dependence on DC politicians: discretion in land allocation, immobility of new landowners, and dependence on local organizations for essential services, such as agricultural inputs, credit, and healthcare. While the land transfer was irreversible, several features of the reform may have thus promoted the rise of clientelist practices. In this section, we analyze two of such practices: political brokering and patronage through public employment.

***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 have played such role for DC. As we discussed in Section 2, *Coldiretti* held significant sway among its members because it provided several important services including credit and access to markets.

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 obtained 100% of the votes in one every five elections. Lanza (1991) estimates that in 1955 and 1958 *Coldiretti* effectively controlled around 97% of *Casse Mutue*, and used differential access to healthcare to incentivize farmers to join the organization and influence them during elections. To understand the role of *Coldiretti*, we digitized new town-level data on *Casse Mutue* budgets and board elections.<sup>29</sup>

We find two results. First, Figure 6 shows RDD estimates with data from the 1965 *Casse Mutue* budgets. We observe treatment effects of 30-50% in both revenues (Panels A-B) and expenses (Panels C-D) per inhabitant. *Casse Mutue*’s boards had some discretion in the use of these funds, and *Coldiretti* members sitting on the boards often used this discretion

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<sup>29</sup>We searched for *Coldiretti* town-level membership, but, as far as we know, such data do not exist.

politically (Lanza, 1991). Larger *Coldiretti* budgets in treated areas suggest that these towns were more exposed to the influence of DC political brokers.

Second, Figure 6 shows RDD results on the board elections between 1955 and 1970. Consistent with the previous results, the number of voters in the board elections is substantially higher in treated towns (by 1-1.5 voters per capita, or approximately 32-46% of the control mean: Panels E-F), suggesting again that more people relied on the *Casse Mutue*'s service provision in reform towns. Panels G-H show that approximately 80% of the new voters chose *Coldiretti*, which controlled most *Casse Mutue*.<sup>30</sup> These results indicate that *Coldiretti* could access a larger base of voters in the reform areas and thus could be more effective in brokering votes for DC.

The land reform not only increased the potential member base of *Coldiretti* (i.e. the number of small land owners), but also its actual support among small landowners: on average, 36% of small landowners who received land through the land reform supported *Coldiretti*, vs 24% among owners in control towns, a 52% increase.<sup>31</sup> This result suggests that the reform strengthened the role of DC brokers in reform areas.

**Public Sector Employment.** Governments' patronage in public sector employment—a common practice around the world (Shefter, 1977, cf. Acemoglu et al., 2011 for a theoretical treatment)—was widespread in post-war Italy, where governments routinely appointed political supporters to public offices (Chubb, 1982, Ferrera, 1996, Golden, 2003, Alesina et al., 2001). Using the panel RDD of Equation (1), we test whether reform towns experienced an increase in public sector employment.

Figure 7-Panels A-B presents the dynamic coefficients. In treated towns, the share of public sector employment was similar at the time of the reform ( $\beta = 0$  with the multidimensional,  $\beta = -0.015$  with the unidimensional RDD). Treated towns experience a differential increase in public sector employment in each of the decades following the reform: all coefficients are highly significant in the coordinate specification and they are significant at 8.9% or less in the distance specification. When we pool the post-reform data, the treatment coefficient is around 1-1.6 p.p. (20-30% of control mean) in both specifications and relatively stable across bandwidths (Figure 7-Panels C-D). In the distance specification we find some evidence of reduction in public employment prior to the reform (1936-51: Figure 7-Panels C-D). These pre-trends disappear at larger bandwidths, where the main effect remains sta-

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<sup>30</sup>The reform had no significant effect on *Coldiretti* share, which was already very high at baseline (73%).

<sup>31</sup>In 1961, there were 12.2 small farms per 100 inhabitants in the control group of 20 km sample. *Coldiretti* received on average 2.9 votes every 100 people: 24% of small farmers supported *Coldiretti*. Treatment effects for small farmers per capita and *Coldiretti* votes per capita are 3.6% and 1.3% respectively, implying that 36% of the new farmers supported *Coldiretti*.

ble and significant, and are absent in the coordinate specification. Importantly, the 1981 coefficient is significantly different from both 1951 and 1936 in the two specifications. This 1981 effect is also large: about two-fifths of the average in control towns (6%) suggesting a significant impact in the decade of the 1980s, when both local and national DC politicians systematically used patronage to maintain electoral support (Allum, 1997; we are not able to distinguish local and national public workers in our data).

These results provide suggestive evidence that, as the economy shifted away from agriculture, DC politicians changed the type of targeted benefits provided to the treated towns that had turned Christian Democrat after the reform. This may have been especially important after 1978, when *Casse Mutue* were abolished: the large effect on public employment in 1981 may help explain the persistence of the electoral effect in the 1980s. Since we cannot document whether public employment was contingent on electoral support, we cannot rule out distributive politics as an alternative explanation of this result. However, political brokers are specific to clientelistic systems and, as DC was known for both clientelism and distributive politics (Piattoni, 2001; Golden and Picci, 2008), it may have used both strategies in reform areas.

To conclude, this section provides evidence that the land reform led to practices consistent with clientelism, including political brokering and patronage. Appendix C presents several exercises similar to those described in Section 5.3 for DC: we find that *Casse Mutue* and public sector estimates are generally robust to these checks.<sup>32</sup> These results suggest that although land redistribution was irreversible, the presence of reversible benefits may have promoted the emergence of a system of contingent exchange. More benefits towards areas that had become aligned with the DC after the land reform is consistent with models predicting politicians transfer more resources to core voters (Dixit and Londregan, 1996).

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<sup>32</sup>We also examine the impact of the reform on fiscal transfers from the central government to local administrations. While distributive politics (pork) is not always associated with clientelism (Hicken, 2011), it was commonly used by DC to maintain consensus (Golden and Picci, 2008). We use the only publications available for town-level municipal budgets (years 1952, 1955 and 1959) and present results in the Appendix Figure D.5. The reform had no impact on transfers in 1955, around the end of land redistribution, and two years after the first post-reform Parliament had taken office. In contrast, in 1959, we find with the distance specification a large (20%) but noisy difference between treatment and control towns (Panel A:  $\beta=0.20$ , s.e.=0.20, p-value=0.31). In the coordinates specification, the coefficient remains positive and sizable, but it is smaller (7%: Panel B). The effects remain stable across bandwidths, and in the distance specification coefficients become significant at 10 to 5% with larger samples. These results thus provide some tentative support to the idea that DC rewarded treated towns with pork after the land reform was completed. However, the differences in the results across specifications grant caution and prevent us from drawing definitive conclusions.



### 6.3 Migration

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

**Total Population.** The reform may affect the patterns of migration. To explore this channel, we estimate Equation (1) with the log of the number of eligible voters as dependent variable. Appendix Figure D.14-Panels A and C show the estimated  $\beta_t$  with our two specifications. There is little evidence of differential migration in the 1950s, the first decade after the reform: the point estimate is zero in the coordinates specification and negative but small when controlling for distance from the border. Starting from the 1960s treated towns appear to lose population. The effect is more precisely estimated in the coordinates specification and indicates that by the early 1990s treated towns were 15% less populous than control towns.

**Population Composition.** We next consider how out-migration affected the demographic composition of treated towns by looking at the decadal censuses.<sup>33</sup> Appendix Table D.2 presents the results with our two specifications. The table indicates that emigrants were more likely to be young (cols. 1-4 and 6-9) men (cols 5 and 10). By 1991, treated towns had 0.4% fewer males (base = 50%) and between 0.3% and 1.9% fewer under-20 (1991 control mean: 16%). Similar to the total population, these effects grow over time and are precisely estimated only when we control for a polynomial in coordinates. The emigration of young males is consistent with common patterns of economic migration across developing countries. For instance, Galán (2018) finds that the children of land reform recipients exhibit higher migration to urban centers. Migration of younger cohorts, or of individuals without their own land, can also take place while other people—the landowners who cannot re-sell the land acquired through the reform (See Section 2)—are relatively immobile.

**Discussion.** In the decades after the land reform, treatment areas experience gradual population decline. However, migration is unlikely to explain the electoral results for at least two reasons. First, the pattern of treatment effects on population changes does not match those of the electoral results: Appendix Figure D.14 suggests that differential out-migration was relatively small in the 1950s, right after the reform, and then it grew over the following decades. Similarly, Table D.2 indicates that the demography of treated and control towns

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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.

slowly diverged overtime. In contrast, Figure 5 shows that support for DC in treated towns increased sharply immediately after the reform, and then remained stable over the following forty years. Second, Appendix Figure D.14 Panels B and D reports the effect of the reform on the *number* of DC votes, normalized by the 1948 value. Importantly, the graphs show that in 1953, in spite of a small reduction in the number of eligible voters, the number of DC votes in treated towns increased by 3.1%. in the distance specification ( $p$ -value = 0.03) and by 1.1%. in the latitude-longitude specification (not significant:  $p$ -value = 0.17). This increase in the number of votes for DC cannot be explained by the out-migration of PCI supporters and it is consistent with the hypothesis that the land reform persuaded some voters to change their electoral choices. In addition, these new DC voters were unlikely to be moving in from control towns because the vast majority of beneficiaries were previous resident of treated towns (Section 2).

Migration patterns are also relevant for the analysis of clientelism. A large literature suggests that, by increasing voters' mobility and outside options, migration may weaken clientelism (e.g. Stokes, 2005; Kitschelt et al., 2007). In the case of the Italian land reform, we do not observe such an effect. Treated towns experience greater outmigration by the 1980s but this is not accompanied by a reduction in support for DC nor by less patronage.

## 6.4 Economic Growth and Development Patterns

The land reform may also have political consequences through its impact on economic growth and development.<sup>34</sup> In this section, we consider the effect of the reform on several economic outcomes.

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

Appendix Figure D.15 reports results for Equation (1) with home-ownership as dependent variable. The treatment coefficients in the five decades after the reform are small and almost never significant. The table shows that the reform had a small positive effect on home ownership in 1961 (only in the coordinates specification). This effect, however, vanished already from the following decade and remains small and insignificant for the rest of the period. While the reform may have promoted higher home ownership in 1961, the effect appears

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<sup>34</sup>Much empirical work on land reform focuses on economic outcomes, such as agricultural productivity (Montero, 2022; Adamopoulos and Restuccia, 2020), poverty reduction (Banerjee et al., 2002; Besley and Burgess, 2000), and structural transformation (Galán, 2018).

small, short-lived and not robust across specifications. This speaks against permanent effects of the reform on wealth accumulation.

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

***Agricultural Productivity.*** We then consider the role of agricultural productivity. While, to the best of our knowledge, town-level data on agricultural productivity do not exist, it is plausible that, at least in the short run, the reform increased agricultural productivity (Barbero, 1960). However, two observations suggest that agricultural productivity is unlikely to explain the electoral results. First, the electoral effects of the reform appear already in the 1953 elections, which took place after the expropriations, but before most of the reform beneficiaries received the land (and even fewer had collected the first harvest). Second, if the electoral impact of the reform was driven by its impact on agricultural productivity, we would expect it to fade overtime, as agriculture lost importance during the second half of the twentieth century (Appendix Figure D.18-Panel A). This is not what we find, as the effect of the reform is stable until 1992.

***Labor Force Participation and Structural Transformation.*** In Section 6.2, we showed that the reform increased public sector employment, a form of patronage. Here, we provide additional details on labor force participation and sectoral labor shares. Appendix Table D.3 shows a decline in labor force participation in treatment areas. This decline already emerges in 1961 and it is estimated more precisely in the coordinates specification. We conjecture that measurement issues drive some of this decline: the reform is likely to have induced a shift from employed work in agricultural to own-farm work. Census enumerators were instructed to count women primarily occupied on household activities (including those working part-time on the household farm) as out of the labor force (ISTAT, 1955, Dati sommari per Comune - Avvertenze). Thus, the wives of land beneficiaries may have been counted among the rural workers in the 1951 Census, and as out of the labor force afterwards, when they helped their husbands on the new family farm. Two pieces of evidence confirm this hypothesis. First, these declines are three times larger among women than among men (cols. 3 and 9).<sup>35</sup> Second, the decline in the labor force share is driven primarily by a reduction in the share of workers in agriculture (cols. 4 and 10).

We find little evidence of declines in manufacturing or services (i.e., if anything, the ser-

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<sup>35</sup>On the contrary, the results on public sector employment (Section 6.2) are not driven by women.

vice share increases by a small amount). We conclude that the reform neither hindered nor fostered structural transformation. Two subsequent papers, Albertus (2023) and Bianchi-Vimercati et al. (2022), use research designs that rely on different identification assumptions—respectively, matching of towns at the border and difference-in-differences—and suggest that the reform led to specialization in agriculture. With our panel spatial RDD analysis we find generally negative, small and insignificant effects on agricultural employment share: for instance until 1981 the average point estimate is -2.3% and the 95% confidence intervals rule out positive effects of more than 3%.

***Firm Growth.*** Third, we consider firms’ growth, using information from the economic census (including the newly digitized 1951 and 1961). We focus on two variables: number of plants per capita and number of workers per plant. Appendix Figure C.1 showed that these outcomes are balanced in 1951. Appendix Table D.4 reports estimates of the panel spatial RDD Equation (1). There is no effect on the number of plants per capita. In one specification (coordinates) we find suggestive evidence of a reduction in the number of workers per plant, starting in 1971. These results are consistent with the decline in population we discussed earlier (Appendix Figure D.14).

***Discussion.*** To summarize, analysis in this section suggests a reduction in the share of active population, driven at least in part by mismeasurement of agricultural producers. Such a decline in active population, if real, would arguably lead to a decline in support for the ruling party, which is inconsistent with the electoral results documented in the paper. The reform had limited effects on other measures of growth, including wealth and income, sectoral reallocation, and firm development. Only plant size appears to start falling in treated towns two decades after the reform, a timing that is not consistent with the immediate and stable electoral effects.

## 6.5 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). This phenomenon may offer another plausible explanation to the persistent electoral effect of the reform, as beneficiaries of the land redistribution became richer than control farmers who did not receive land. Land beneficiaries may then be more conservative than families on the other side of the border simply because they own more wealth. Their children may also be more conservative because of inter-generational transmission of wealth,

of values—or both. Several pieces of evidence do not support this interpretation. First, the premise of the economic conservatism argument is that voters are richer in treated towns, but we already showed the reform did not have any significant long-term impact on wealth (home ownership) or income per capita. Second, economic conservatism cannot explain why in treated towns voters support DC agenda on *family* policies in the 1974 divorce referendum. In this section, we present a third additional piece of evidence that speaks against this interpretation.

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

If richer voters in treated towns favor more conservative policies regardless of the identity of the right-wing party that implements them, we would expect greater support for Berlusconi in reform areas. We test this idea with the RDD model and data on Berlusconi’s party vote shares in the elections of 1994, 1996 and 2001. Appendix Figure D.17-Panels A-F report the treatment coefficient on *Forza Italia* vote share for multiple bandwidths. Treatment coefficients are small and insignificant in each election year. Moreover, controlling for DC vote share in 1948 has no effect on point estimates. When we look at the entire center-right coalition, we obtain similar results (Appendix Figure D.17-Panels G-L). This evidence speaks against the intergenerational transmission of conservative values as a driver of electoral persistence.

***Discussion.*** To sum up, we find little evidence for effects of the reform on economic conservatism or for effects of economic conservatism on electoral outcomes. First, available data does not suggest that treated towns became richer. Second, this mechanism cannot explain the support for the DC *family policy* agenda during the 1974 referendum. Third, support for DC did not translate in support for the next right-wing party after 1992, possibly because these parties did not have a chance to establish a relationship with voters in treated towns through a large redistribution policy.

## 6.6 Mechanical Persistence

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

## 7 Conclusions

While the political objective of many large-scale redistribution policies is to establish long-lasting political support, evidence on the persistence of such electoral gains is rare. In this paper, we study the electoral impact 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, the Christian Democrats (DC), gains 4 percentage points in treated towns after the reform. These electoral gains persisted for over 40 years. Voters in treated towns also support Christian Democrats' policy *agenda* (i.e., in a divisive family policy referendum), as well as their candidates.

We explore several mechanisms for the persistence of the electoral gains, including clientelist brokering and patronage, selective migration, economic development, and changes in voters' beliefs. We suggest that the reform created a class of relatively immobile landowners, who could be targeted with contingent benefits such as agricultural services and health care, and it laid the foundation for a long-lasting relationship with DC governments that evolved with the society and persisted for over four decades. The way in which DC politicians continued to favor reform areas supports theories of political redistribution to core voters (Dixit and Londregan, 1996): our results suggest that this type of redistribution may generate long-lasting political support.

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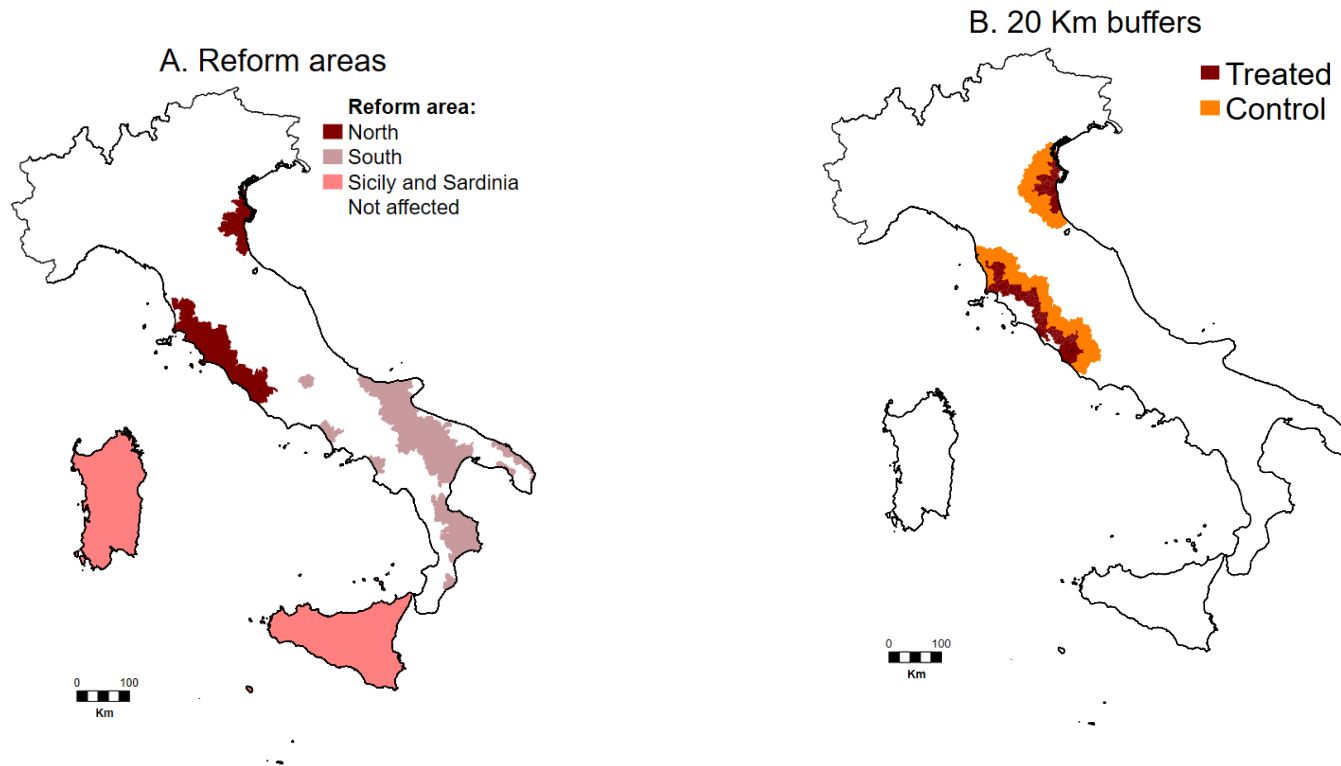
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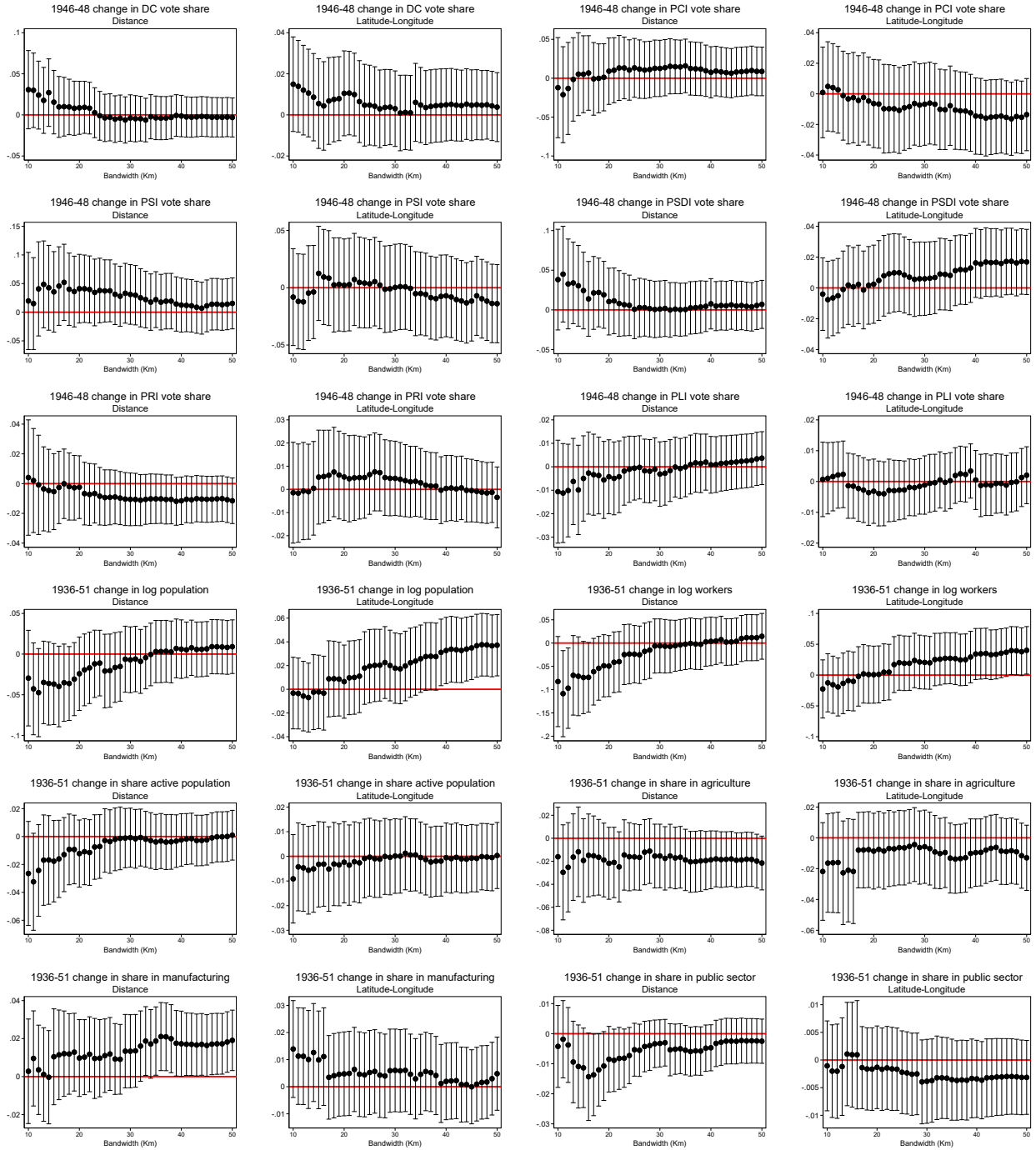
# Figures

Figure 1: Reform Areas and Buffers



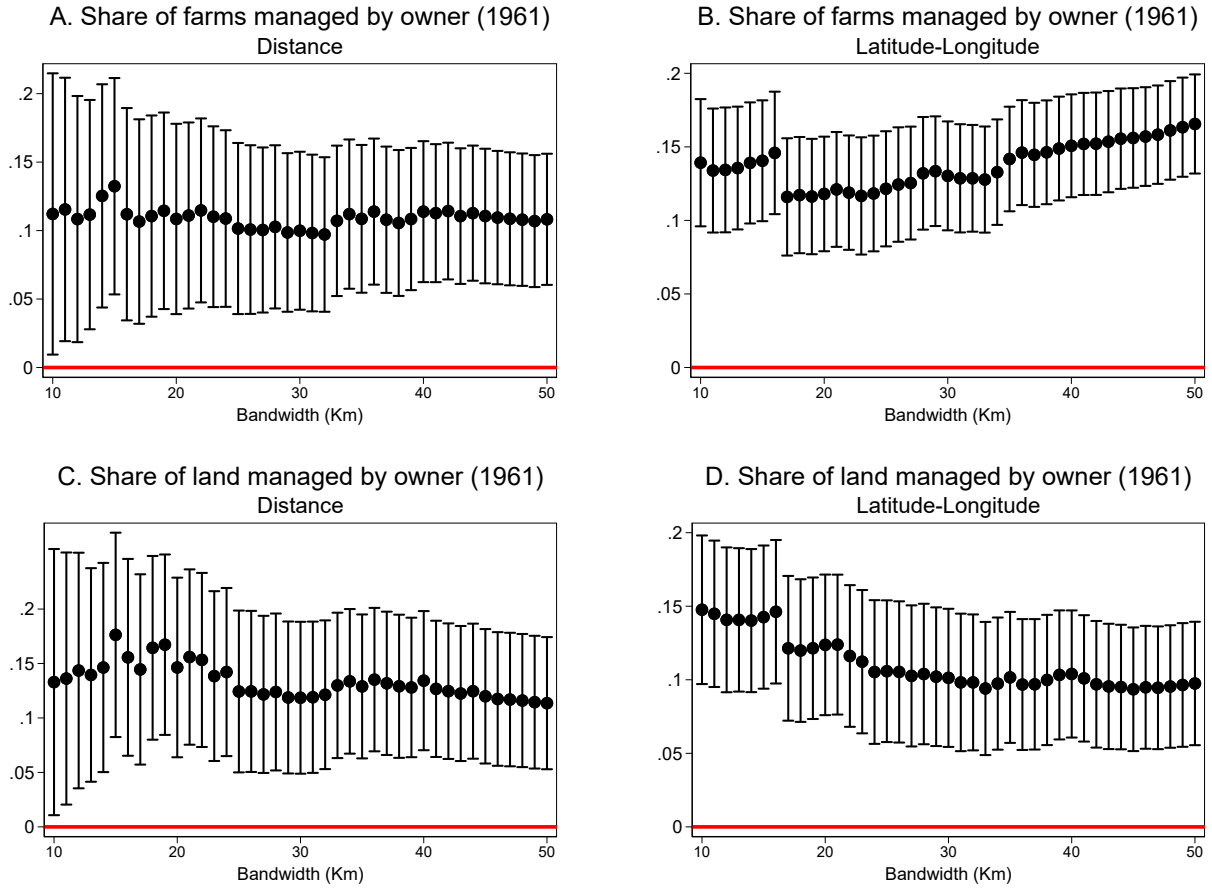
Notes: Panel A: land reform areas as defined in the 1951 executive orders (D.P.R.66/1951; D.P.R.67/1951; D.P.R.68/1951; D.P.R.69/1951; D.P.R.70/1951; D.P.R.264/1951; D.P.R.265/1951). In dark red the areas of Delta Padano (North-East) and Maremma (Center-West). In light brown the areas of Fucino (Center), Opera Combattenti (South-West), Puglia and Lucania (South-East), Sila (South). In pink the islands of Sicily and Sardinia. Panel B: 20 km buffers inside and outside the border of Delta Padano and Maremma (preferred bandwidth in the unidimensional specification).

Figure 2: Pre-Trends at the border



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

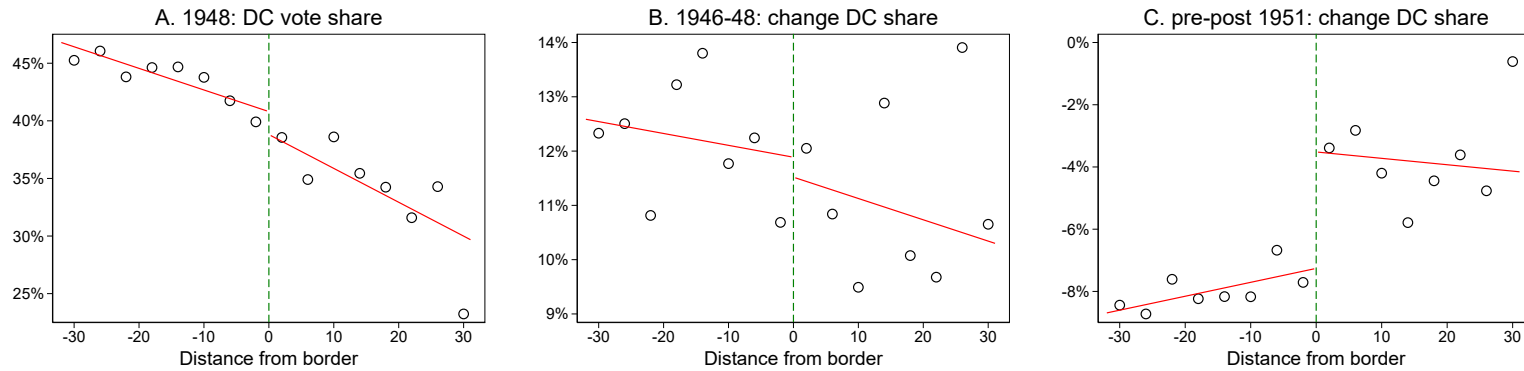
**Figure 3:** The impact of the reform on farm management.



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

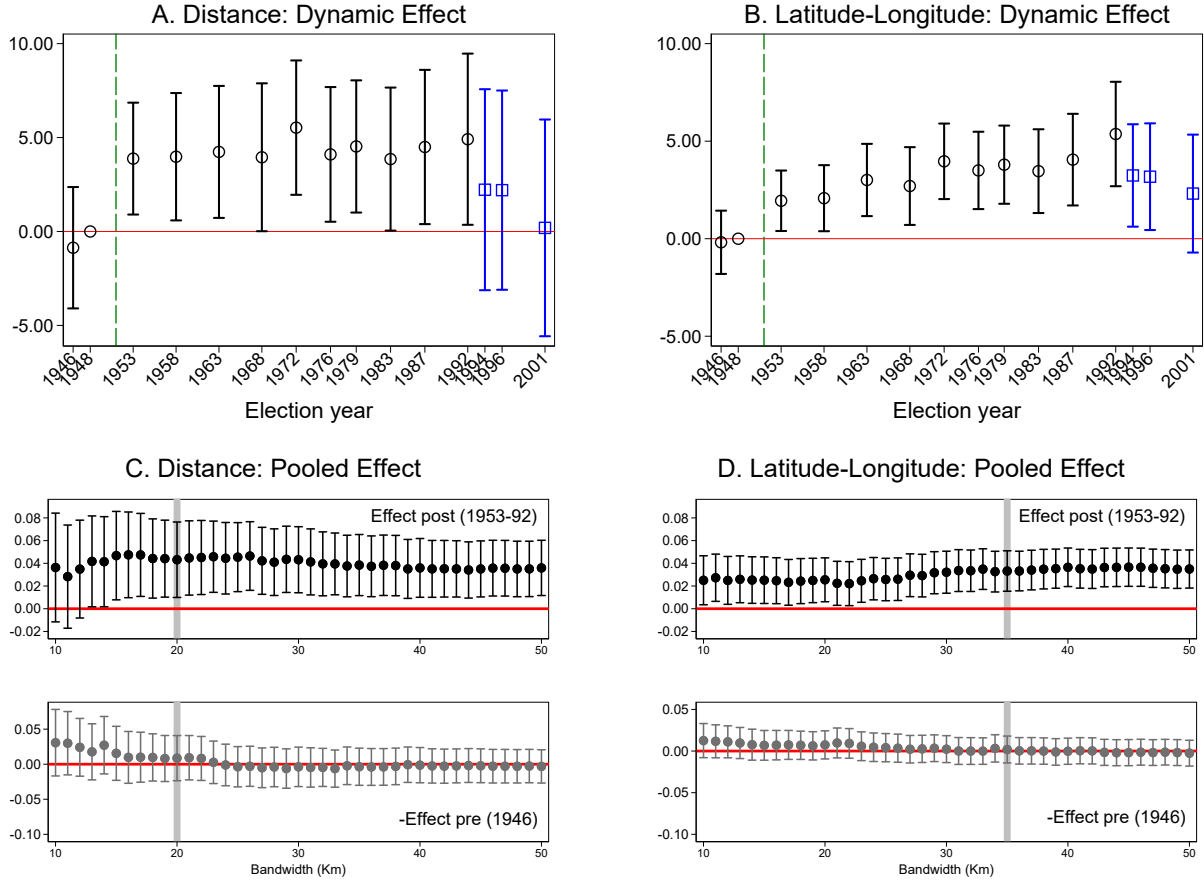


**Figure 4:** Balance, pre-trends and effect of reform on DC vote share: graphical evidence



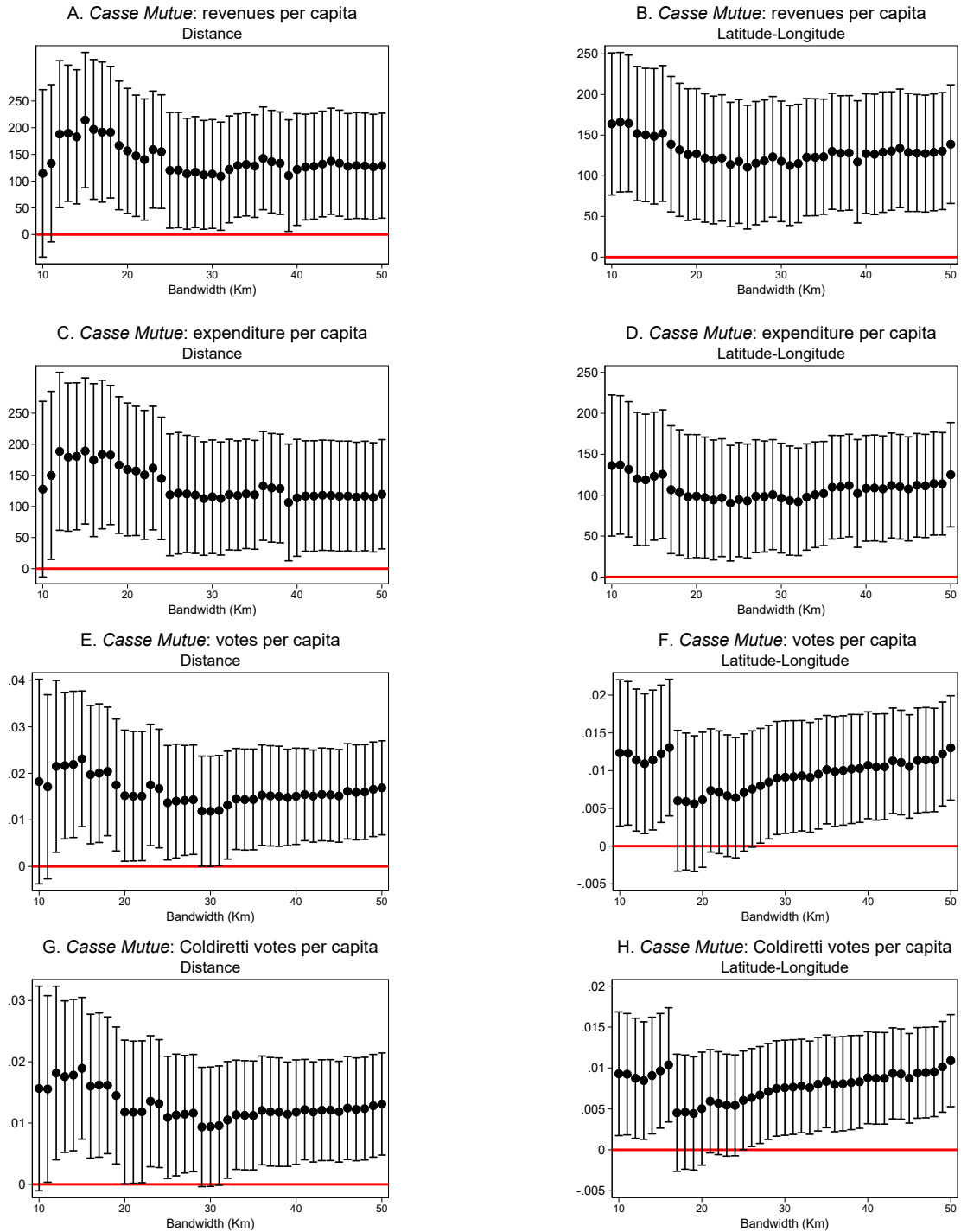
*Notes:* The Figure presents graphical evidence on the panel RDD. On the y-axes we plot electoral outcomes; on the x-axes the distance to the border. In each Panel, we bin data in 4 km intervals. Treated towns have positive distance and control towns have negative distance. The red lines report linear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

**Figure 5:** The Impact of the Reform on DC vote share: Panel RDD Results



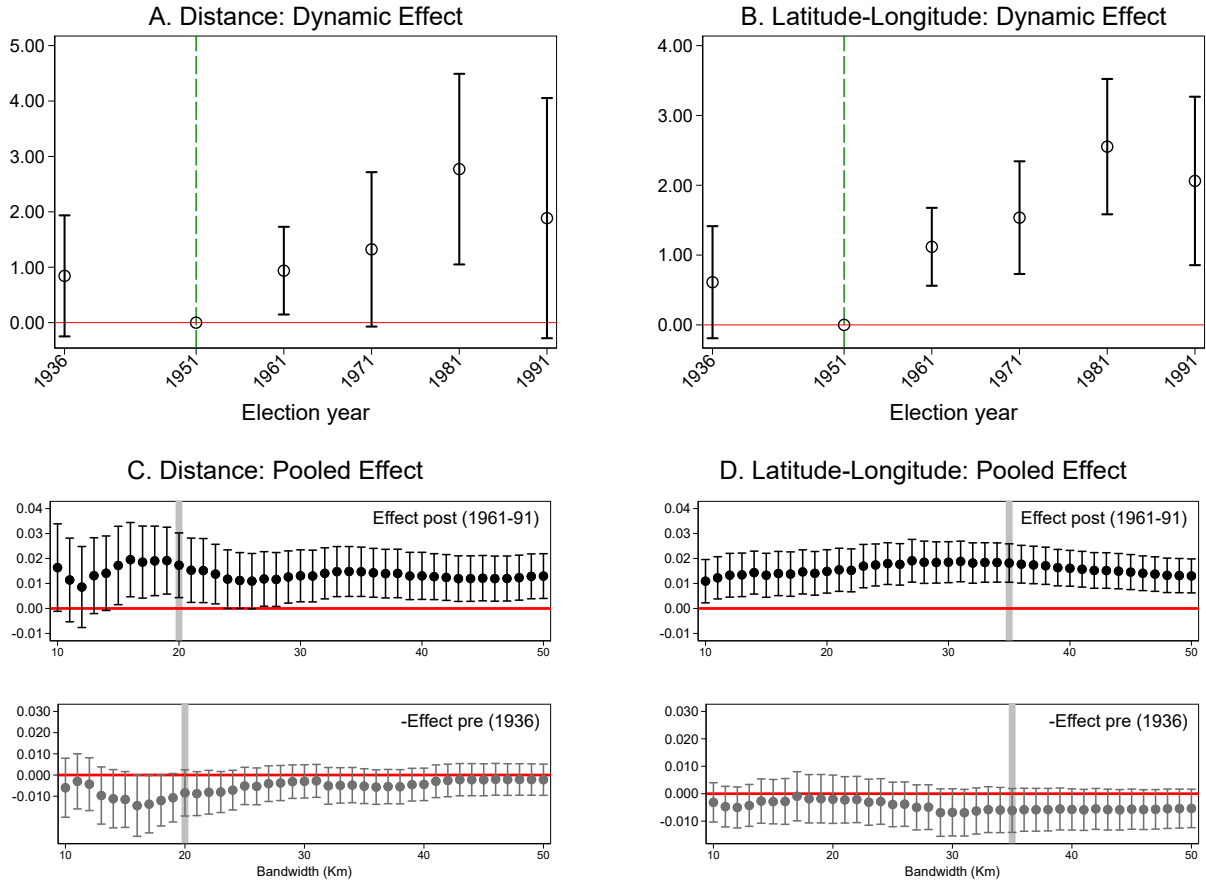
*Notes:* Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients  $\beta_t$  from the panel RDD Equation (1),  $t = \{1946, \dots, 2001\}$ . The omitted category is the  $\beta$  of 1948 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. The vertical dashed lines mark the 1951 land reform. In the post-1992 elections we consider DC the following parties: *Italian Popular Party* and *Patto Segni* (1994); *Italian Popular Party*, *Lista Dini*, the Christian Democratic Center and the Christian Democratic Union (1996); *Margherita*, Christian Democratic Center and the Christian Democratic Union (2001). Panels C and D report pooled panel RDD coefficients for all years between 1946 and 1992: top panels report the post-1951 coefficient, the bottom panels minus the 1946 coefficient from the same regression. We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. Shaded areas indicate 20 and 35 km: the optimal bandwidths in the two specifications; these are the bandwidths on which coefficients in Panel A and B are estimated. All regressions control for year  $\times$  reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

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



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

**Figure 7:** The impact of the reform on patronage.

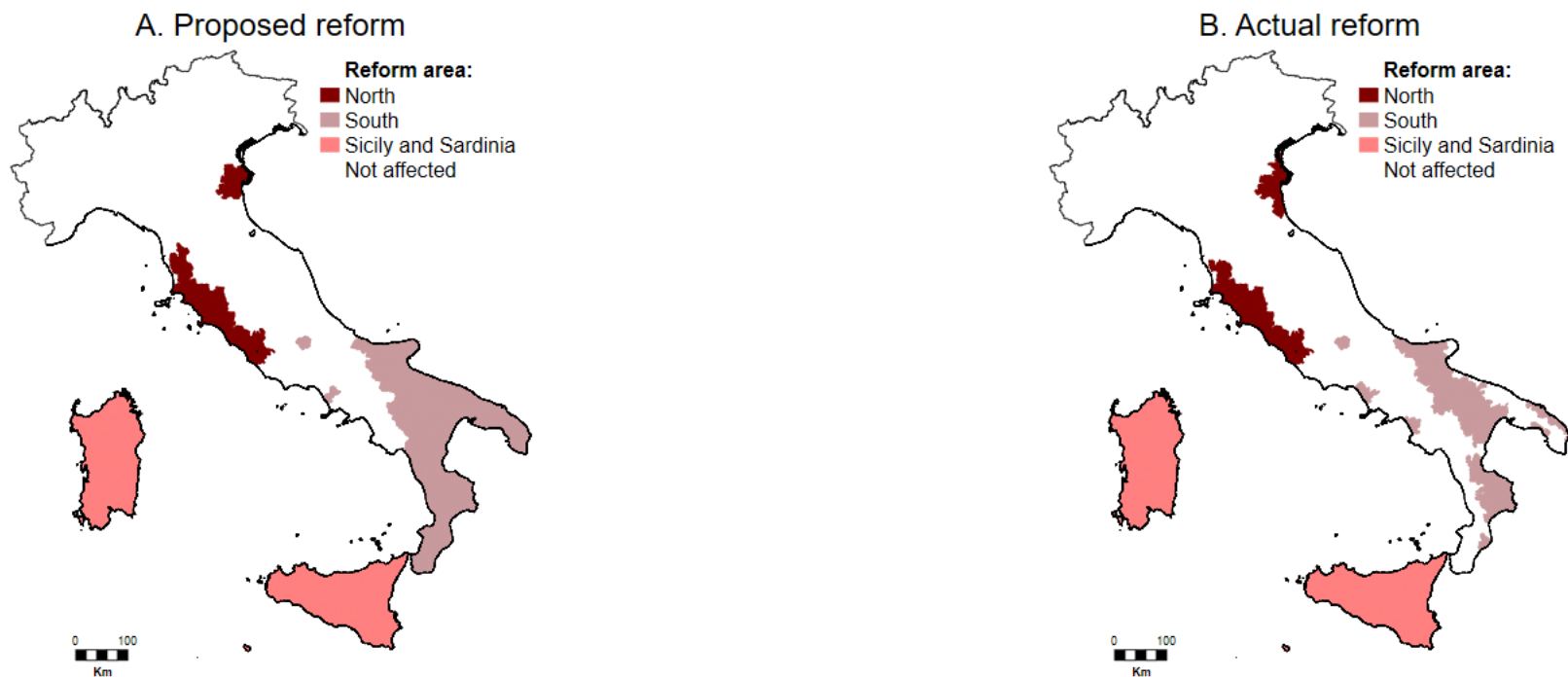


*Notes:* The dependent variable is the share of active workers employed in the public sector, a form of patronage. Panels A and B display coefficients  $\beta_t$  from the panel RDD Equation (1),  $t = \{1946, \dots, 2001\}$ . The omitted category is the  $\beta$  of 1951 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. The vertical dashed lines mark the 1951 land reform. Panels C and D report pooled treatment effects for all census years between 1936 and 1991: top panels report the post-1951 coefficient, the bottom panels minus the 1936 coefficient from the same regression. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. Shaded areas indicate 20 and 35 km: the bandwidths on which coefficients in Panel A and B are estimated. All regressions control for year  $\times$  reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

# Appendix (For Online Publication)

## A The 1950 Italian Land Reform

Figure A.1: Proposed and actual reform



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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

CAMERA DEI DEPUTATI 23/11/50

Caro Segni,  
mi dicono che non  
parlerai? Perché?  
Mi fusti prima  
suntati  
Costituito per  
suo legittimo  
Non parlerò perché in effetto  
non ho le cose; dopo un quarto  
d'ora dovrà smettere.  
Però, non ti uanando che  
non parlo volentieri: se non  
sarò soddisfatto sulla tua  
"fondazione" (per le condizioni  
del cord che nessuno ha  
le), non sarò soddisfatto

(Panel A) Messages between Segni and Sampietro

DEMOCRAZIA CRISTIANA  
COMITATO PROVINCIALE DI SIENA

Sienna, 2 Febbraio 1951  
Tel. 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, al da produrre una sfavorevole 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.

DEMOCRAZIA CRISTIANA  
COMITATO PROVINCIALE  
SIENNA

(Panel B) Letter to Segni from DC Siena Committee

REPUBBLICA ITALIANA  
 Il Presidente  
 del Consiglio dei Ministri

212

Per Segni

Rodinò mi dice che agri-  
 coltori ribollano di sdegno  
 contro estensione compres-  
 sorio (Cavarzere  
 Candelina ecc.)  
 Comunque ho promesso  
 incontro Segn. Rodinò.

(Panel C) Note to Segni from De Gasperi

Roma, / febbraio 1951

Carissimo Presidente,

Ho avuto la lettera che mi hai inviato, letta quella in-  
 viata a Raini, 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 ri-  
 cadono nella legge; sulla zona del Delta (Coppara, Follanda di  
 Savoia ecc.) sono ora iniziati a cura e spese dello Stato, ope-  
 re pubbliche di irrigazione, che importano una trasformazione  
 sostanziale; ancor più influente e decisiva è l'opera di appo-  
 deramento da fare, che riguarda tutti i territori del Delta in-  
 seriti 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 ir-  
 rigazione, e quindi geograficamente fa parte del Delta Padano,  
 che è anche più esteso, in quanto vi sono compresi altri comu-  
 ni 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 ordi-  
 ne finanziario - è più ristretta di quel che si sarebbe dovuto largamen-  
 te fare. Non si sono compresi i territori estensivi della provincia di  
 Siena, di gran parte della campagna romana, di alcune parti di Pisa, Li-  
 vorno, Latina, Frosinone, appunto per tali considerazioni.

(Panel E) Technical Report for the Ministry of  
 Agriculture

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

Figure A.3: Expropriation Criteria

**Percentuali di scorporo  
riferite agli scaglioni di reddito imponibile**

SCAGLIONI DI REDDITO IMPONIBILE TOTALE		Imponibile medio per Ha.									
		Lire									
Lire		1000 e oltre	900	800	700	600	500	400	300	200	100 e meno
Fino a	30.000	—	—	—	—	—	—	—	—	—	—
Da oltre	30.000 a 60.000	—	—	—	—	—	0	15	30	55	70
»	60.000 a 100.000	—	—	—	—	0	10	30	60	70	85
»	100.000 a 200.000	35	40	47	55	60	65	70	75	80	87
»	200.000 a 300.000	45	50	55	60	65	70	75	80	85	90
»	300.000 a 400.000	52	57	60	65	70	75	80	85	90	95
»	400.000 a 500.000	60	64	66	71	76	80	85	90	95	95
»	500.000 a 600.000	64	70	76	78	80	85	90	95	95	95
»	600.000 a 700.000	68	74	79	82	85	90	95	95	95	95
»	700.000 a 800.000	72	78	82	85	90	95	95	95	95	95
»	800.000 a 900.000	76	82	86	90	93	95	95	95	95	95
»	900.000 a 1.000.000	82	86	90	93	95	95	95	95	95	95
»	1.000.000 a 1.200.000	90	92	95	95	95	95	95	95	95	95
	Oltre 1.200.000	95	95	95	95	95	95	95	95	95	95

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



Figure A.4: Original applications

Mod. D. A. T. **U. n. 3.60** **Unità Regg.**

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

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

**22** **23**

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 Pannofice*

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

Il sottoscritto Maurizio Mannino di Cornus  
e di Polli Lucania nato a Montebelluno e residente  
in Montebelluno Lucania Via Enclio N. \_\_\_\_\_ avendo la qualifica  
principale di (2) Braccianti e la secondaria di (3) Contadino  
così come risulta dal proprio Libretto di Lavoro N. \_\_\_\_\_ o dal tesserino personale rilasciato dall'Ufficio  
Comunale del Lavoro di \_\_\_\_\_ fa domanda per ottenere la assegnazione di una quot  
di terreno ai sensi degli articoli 16 e 17 della Legge 12 maggio 1950, n. 230 e dell'art. 21 della Legge 21 ott  
bre 1950, n. 841.

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

Montebelluno, li 6-5-1952 FIRMA DEL RICHIEDENTE

**MISSYATA** 1 FEB 1951

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

**MONELLI** **Bruna** -  
Via Filangieri 13  
**BRUNATA**

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

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

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

*Si resterà in attesa e si è in attesa*

IL DIRETTORE DEL CENTRO  
(Dr. Carlo Corno)

**TOTALE**

Con la presente contestato espresso mandato alla Cooperativa  
di questo Centro la predetta somma di L. \_\_\_\_\_  
in contante dalla vendita dei prodotti del mio podere.

L'ASSEGNETARIO

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>36</sup> with a shapefile of 2001 Italian towns<sup>37</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>38</sup> We compute the distance between the town centroid and each reform area border and assign each town uniquely to its closest reform area.

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

### B.2 Variable Construction

#### Treatment

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

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

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<sup>36</sup>We find these lists on <http://www.elesh.it>.

<sup>37</sup>From ISTAT. ISTAT provides a shapefile for 1991 towns, but ELES website does not have a 1991 list of towns.

<sup>38</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 50 km buffer, are caused by towns created from territories that in 1951 were part of several towns. For example: Semproniano was created in 1963 with territories taken from Manciano, Roccalbegna and Santa Fiora; Sellia Marina was created in 1957 with territories from Albi, Soveria Simeri, Sellia, Cropani and Magisano. These events are balanced at the reform border.

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

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

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

## **Electoral outcomes**

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

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

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

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

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

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

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

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

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

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

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

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

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

## Land distribution

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

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



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

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

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

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

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

### ***Casse Mutue***

***Casse Mutue elections.*** We digitize new data on board elections of *Casse Mutue*: farmers' local healthcare providers. From 1955, elections of *Casse Mutue*'s board of directors were generally held every three years (L.1136/1954). We digitize all available election results (1955-1970) from the fonds of the Alleanza Nazionale 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 Federazione Nazionale Casse Mutue (1966). We measure the relative size of *Casse Mutue* with total revenues and total expenditure per agricultural worker (from 1961 population census).

## Other public policies

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

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

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

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

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

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

## Economic and demographic characteristics

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

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

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

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

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

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

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

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

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



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

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

***Number of workers per plant.*** The number of workers per plant is the total number of workers employed in manufacturing, extraction, construction, commerce, 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.

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

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

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

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

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

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

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

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

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

## Geographic characteristics

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

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

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

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

***Gothic line map.*** We draw the "Gothic line" using the map contained in (Oland, 1995).

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

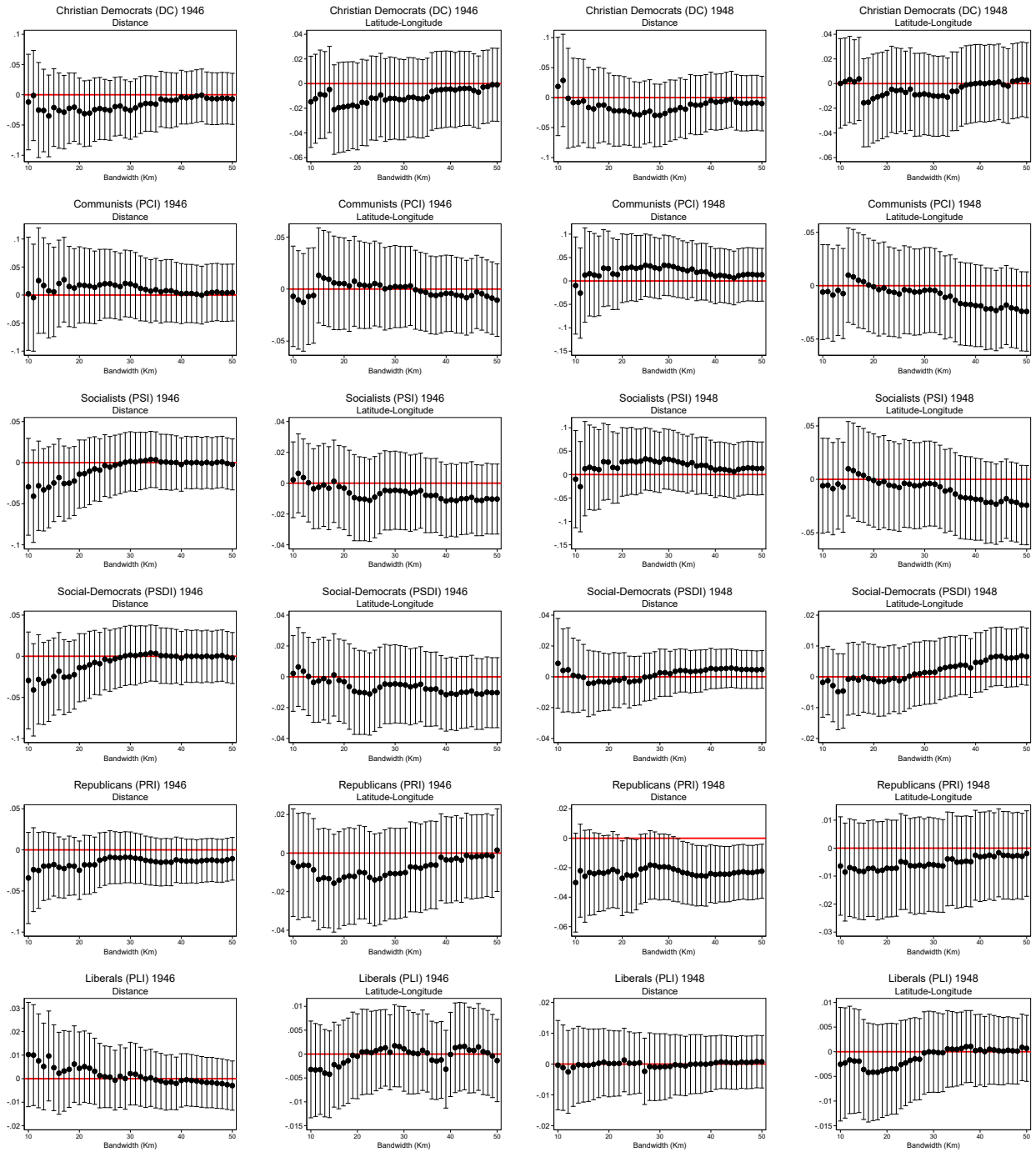
***Potential yield: maize.*** Potential yield data is from FAO-GAEZ (FAO, 2015). This data is defined on a  $9.25 \times 9.25$  km grid covering the entire planet. We join the raster to the map of 1951 Italian towns and assign to every town the average potential yield of maize with medium-level of inputs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

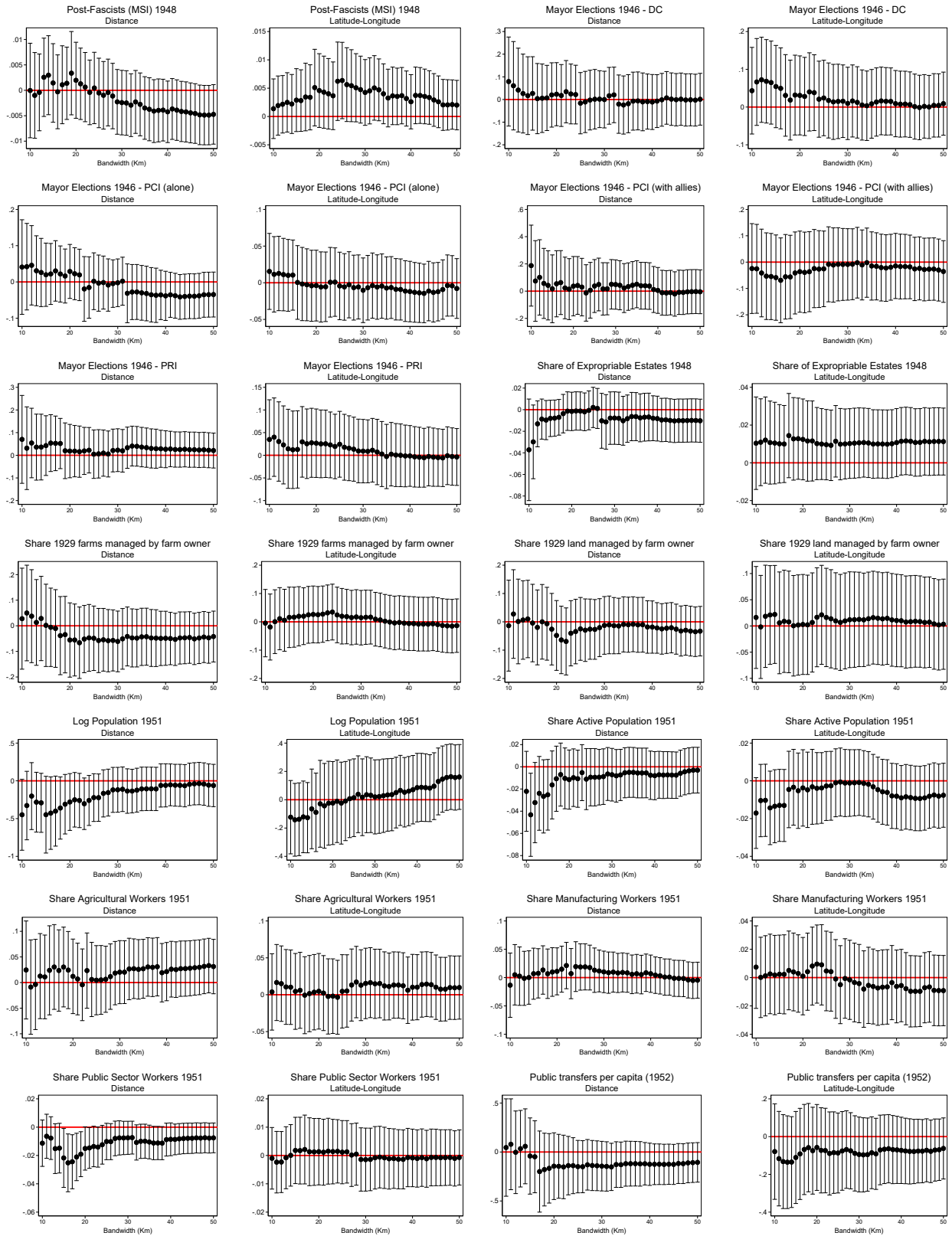
***Share of border exposed to treatment.*** The variable is the length of the town limits that touch treated towns divided by the total length of these limits.

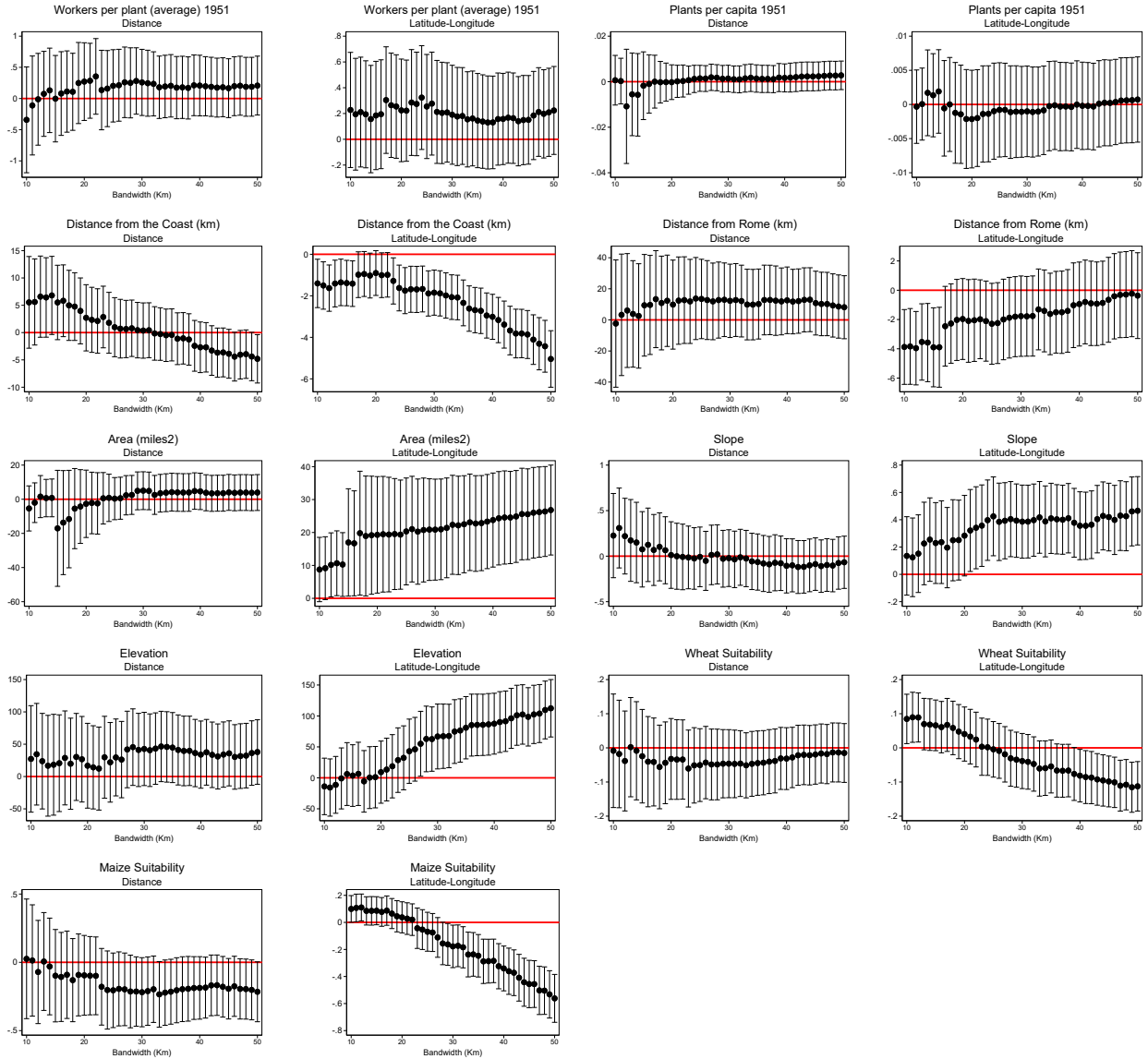
# C Robustness and Alternative Specifications

## C.1 Balance – Northern Italy

Figure C.1: Balance Northern Italy







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

## C.2 Continuity of the Running Variable

To our knowledge, there is no standard manipulation test for two-dimensional RDD studies. This appendix discusses results from a local polynomial density test based on Cattaneo et al. (2020) and run on the distance to the border (unidimensional RDD).

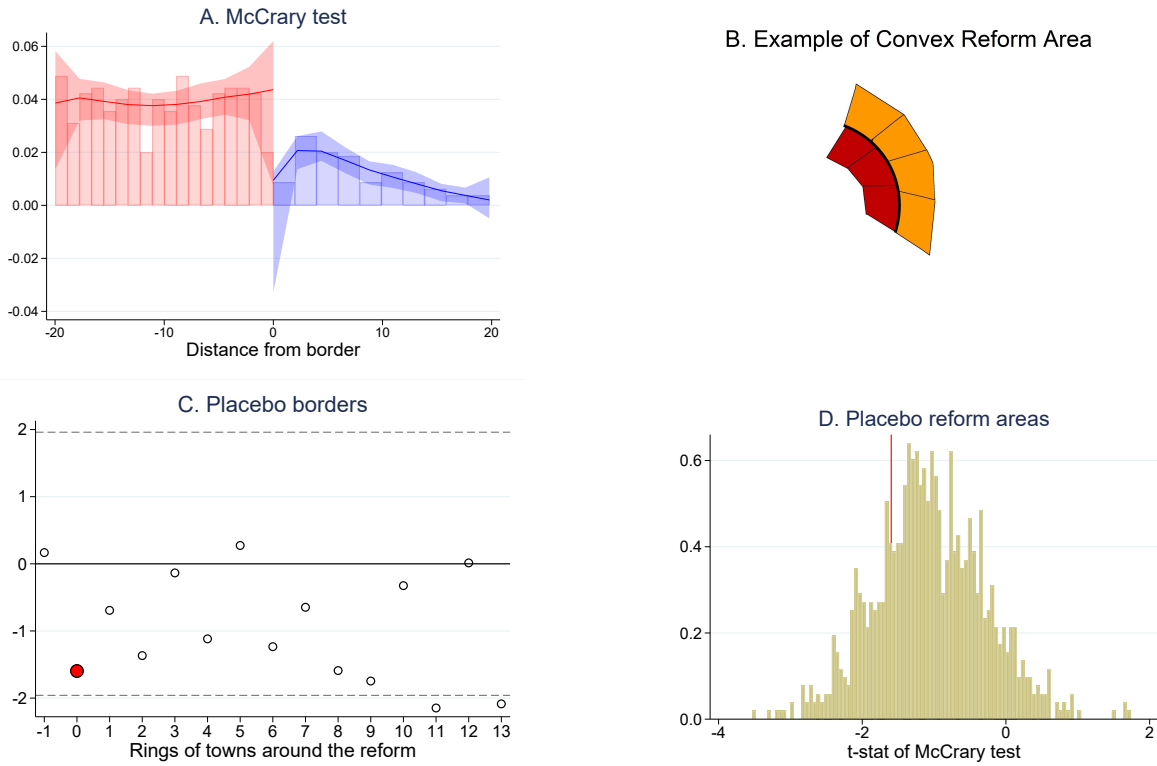
Appendix Figure C.2-Panel A presents the density approximation of the number of towns in the North (y-axis) as a function of the distance to the border (x-axis). The formal test has a  $t$ -statistic of -2.44 and rejects the null of no jump at the border. We believe that the jump in the density is a 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 20 km of the border occupy around 38% more area than treated towns within the same distance (roughly 10'600 vs 14'600 km<sup>2</sup>). Thus, it must be the case that there are more towns outside than inside the reform area (see Appendix Figure C.2-Panel B). We would then expect a greater number of control towns at every given absolute value of distance to the border, including values very close to the border (see Appendix Figure C.2-Panel A).

We validate this intuition with two separate exercises. In the first exercise, we move the border of Maremma inside and outside of the actual reform area by including progressively every “ring” of towns touching the previous border. For each of these fictitious borders “parallel” (so to speak) to the actual reform, we re-estimate the McCrary test. Appendix Figure C.2-Panel C reports the  $t$ -statistics of the McCrary test (y-axis) against the number of rings of towns we moved the reform border (x-axis). Most fictitious borders have McCrary estimates close to the McCrary  $t$ -statistics of Maremma towns (-1.60).

In the second exercise we use randomized inference and we simulate 999 separate fictitious reform areas on the true map of northern Italy. To build these fictitious reform areas, we follow rules that replicate the actual reform: we draw areas of (i) contiguous towns, (ii) located at least partially on the coast and (iii) that cover the same area of the actual reform. For each of these replications we estimate the McCrary test. We plot the distribution of the 999  $t$ -statistics on Appendix Figure C.2-Panel D. The average  $t$ -statistic is -1.05 and our observed  $t$ -statistic (-1.60) lies at the 25th percentile of the distribution.

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

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

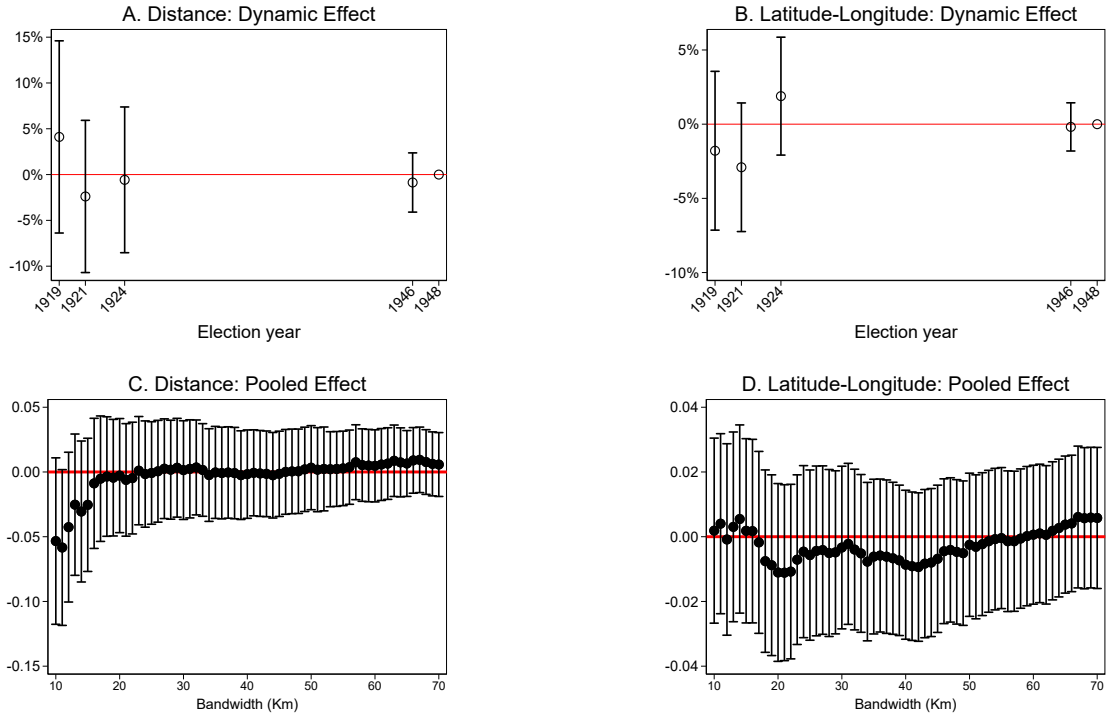


*Notes:* Panel A: density approximation of the number of towns within 20 km from the border of Delta Padano and Maremma. The approximation estimates separate densities on the two sides of the border and it is the basis of the test proposed by McCrary (2008) as updated in the Cattaneo, Jansson and Ma `rddensity` Stata program. The  $t$ -statistic of the test is -2.44. Panel B: an example of convex reform area with towns of similar size on the two sides of the border. In this case the number of towns just outside of the border (orange) is greater than the number of towns just inside (red). Panel C and D:  $t$ -statistics of McCrary tests estimated on fictitious reform areas. Panel C: 14 fictitious reform areas; y-axis:  $t$ -statistics of the McCrary tests. The first area is created by removing from Maremma all treated towns lying on the reform border (point -1 on the x-axis). The other 13 areas are created by expanding Maremma so that it includes all towns lying on each successive reform border (points 1-13 on the x-axis). The  $t$ -statistic of the McCrary test of the true Maremma area is in red (point 0 on the x-axis). Panel D: 999 randomly generated fictitious reform areas. Each of these areas consist of contiguous towns with the same area as Maremma. We calculate the  $t$ -statistic of the McCrary test for each of them on the sample of towns that lie within 25 km from these fictitious borders. The Figure reports the distribution of these  $t$ -statistics. The red vertical line marks the  $t$ -statistic of the McCrary test of the true Maremma area.



### C.3 1919-1948 Pre-Trends

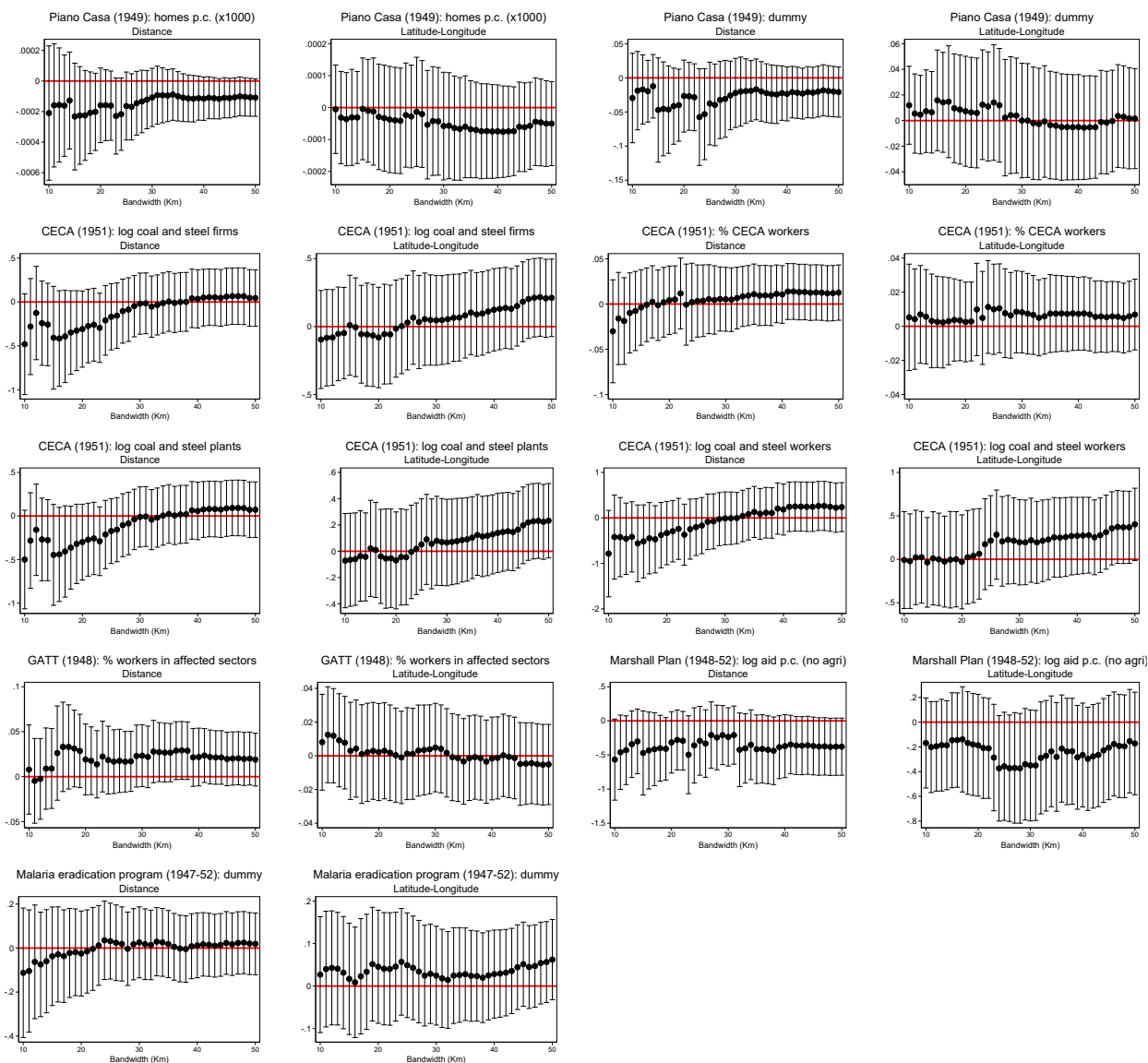
Figure C.3: Pre-Fascism Elections



*Notes:* Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients  $\beta_t$  for each year before the reform from the panel RDD Equation (1). The omitted category is the  $\beta$  of 1948 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. Panels C and D report pooled treatment effects for all electoral years between 1919 and 1946. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. All regressions control for year  $\times$  reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. We use the vote share of the Italian Popular Party (PPI) in the years 1919-24. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

## C.4 Contemporaneous Policies

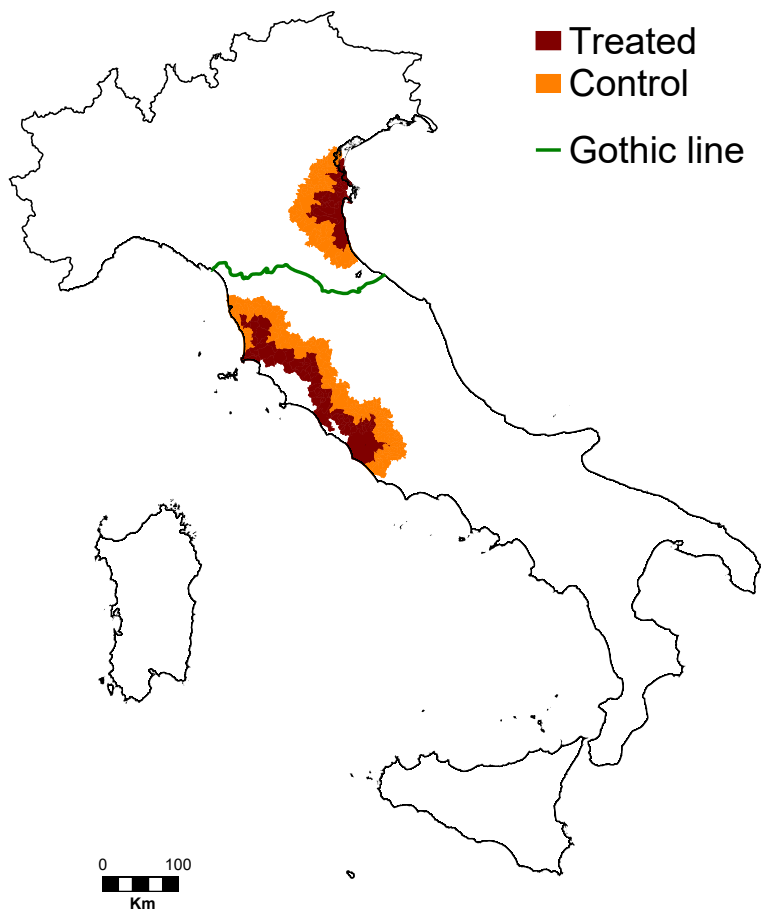
Figure C.4: Contemporaneous policies



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

## C.5 Gothic Line

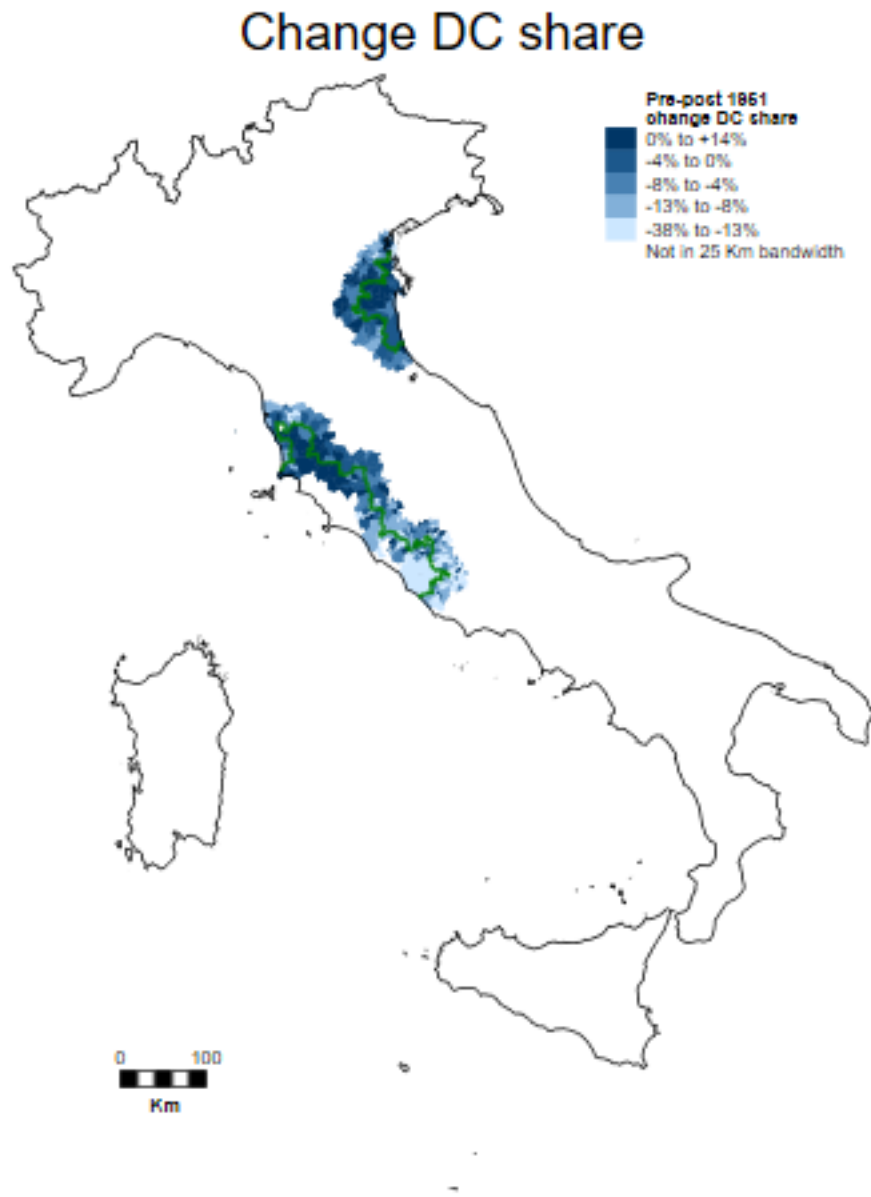
Figure C.5: Gothic Line



*Notes:* The map shows the position of the Gothic line (green) and the towns within 20 km from the reform borders of Delta Padano and Maremma (orange, outside; red, inside).

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

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



*Notes:* Change in DC vote share between pre- (1946-48) and post-reform elections (1953-92) in the towns within 20 km from the reform borders of Delta Padano and Maremma.

## C.7 Robustness to alternative specifications

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

	Distance						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	No prov. seats	Distance <sup>2</sup>	Reform area × distance	Elec. dist. FEs	Segment FEs	Prov. FE
<b>A. DC vote share</b>							
Treatment × 1946	-0.009 [0.016]	-0.011 [0.016]	-0.038 [0.027]	-0.011 [0.017]	-0.015 [0.015]	-0.008 [0.014]	-0.017 [0.016]
Treatment × Post	0.043** [0.017]	0.041** [0.017]	0.035 [0.028]	0.045*** [0.017]	0.043*** [0.016]	0.034** [0.016]	0.039** [0.016]
<b>B. Casse Mutue revenues p.c.</b>							
Treatment	156.689*** [59.730]	147.434** [59.547]	235.322*** [83.945]	147.169** [57.943]	182.393*** [60.880]	136.113** [59.572]	162.721*** [59.940]
<b>C. Casse Mutue expenses p.c.</b>							
Treatment	159.537*** [54.521]	151.982*** [54.585]	202.272*** [75.978]	152.435*** [53.857]	161.162*** [53.963]	142.756*** [53.526]	139.236*** [52.370]
<b>D. Casse Mutue votes p.c.</b>							
Treatment	0.015** [0.007]	0.015** [0.007]	0.034** [0.013]	0.012* [0.007]	0.019** [0.007]	0.016** [0.007]	0.015** [0.007]
<b>E. Coldiretti votes p.c.</b>							
Treatment	0.012** [0.006]	0.012* [0.006]	0.029*** [0.010]	0.009* [0.005]	0.015** [0.006]	0.012** [0.005]	0.011** [0.005]
<b>F. Public Sector Employment</b>							
Treatment × 1936	0.008 [0.006]	0.007 [0.005]	0.011 [0.008]	0.009 [0.006]	0.010* [0.005]	0.005 [0.005]	0.009* [0.005]
Treatment × Post	0.017*** [0.007]	0.015** [0.006]	0.016 [0.010]	0.019*** [0.007]	0.020*** [0.006]	0.012** [0.006]	0.021*** [0.006]

Notes: The Table reports coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area × year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area × year fixed effects. Panel A: The omitted category is the  $\beta$  of the elections of 1948. Panel F: omitted category is the  $\beta$  of 1951. Col. 1: baseline specification controlling for a linear polynomial in distance. Col. 2: drop 10 provincial seats (including Rome). Col. 3: control for 2<sup>nd</sup> order polynomial in distance interacting with decades and reform area on both side of the border. Col. 4: controls for a linear polynomial in distance interacting it with reform area identifiers. Col. 5: controls for electoral districts × year fixed effects. Col. 6: control for ten segments × year fixed effects (see Figure C.7). Col. 7: control for province × year fixed effects. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns within 20 km of the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level (Panels A, D, E, F) or heteroschedastic robust (Panels B, C) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

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

	Latitude-Longitude						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	No prov. seats	Linear	No reform area interactions	Elec. dist. FEs	Segment FEs	Prov. FE
<b>A. DC vote share</b>							
Treatment $\times$ 1946	-0.002 [0.008]	-0.003 [0.008]	0.006 [0.007]	-0.000 [0.008]	-0.006 [0.009]	0.007 [0.009]	0.002 [0.012]
Treatment $\times$ Post	0.033*** [0.009]	0.032*** [0.009]	0.057*** [0.008]	0.040*** [0.009]	0.030*** [0.010]	0.028*** [0.010]	0.022* [0.012]
<b>B. Casse Mutue revenues p.c.</b>							
Treatment	123.425*** [36.147]	125.593*** [36.348]	163.517*** [30.484]	155.224*** [33.050]	132.154*** [35.732]	128.263*** [37.074]	99.072** [38.625]
<b>C. Casse Mutue expenses p.c.</b>							
Treatment	101.980*** [32.439]	101.508*** [32.721]	149.386*** [26.179]	156.104*** [27.736]	110.674*** [33.233]	109.353*** [34.383]	83.421** [34.335]
<b>D. Casse Mutue votes p.c.</b>							
Treatment	0.010*** [0.004]	0.010*** [0.004]	0.013*** [0.003]	0.014*** [0.003]	0.010*** [0.004]	0.010*** [0.004]	0.007* [0.004]
<b>E. Coldiretti votes p.c.</b>							
Treatment	0.008*** [0.003]	0.008*** [0.003]	0.009*** [0.003]	0.010*** [0.003]	0.008*** [0.003]	0.008*** [0.003]	0.004 [0.003]
<b>F. Public Sector Employment</b>							
Treatment $\times$ 1936	0.006 [0.004]	0.005 [0.004]	0.006 [0.004]	0.006* [0.004]	0.003 [0.004]	0.004 [0.003]	0.005 [0.005]
Treatment $\times$ Post	0.018*** [0.004]	0.018*** [0.004]	0.014*** [0.004]	0.019*** [0.004]	0.013*** [0.004]	0.011*** [0.004]	0.015*** [0.005]

*Notes:* The Table reports coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area  $\times$  year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area  $\times$  year fixed effects. Panel A: The omitted category is the  $\beta$  of the elections of 1948. Panel F: omitted category is the  $\beta$  of 1951. Col. 1: baseline specification controlling for a quadratic polynomial in latitude and longitude, interacted with reform area identifiers. Col. 2 drop 10 provincial seats (including Rome). Col. 3: control for 1<sup>st</sup> order polynomial in latitude and longitude interacted with decades and reform area on both side of the border. Col. 4: controls for a quadratic polynomial in latitude and longitude without interacting it with reform area identifiers. Col. 5: controls for electoral districts  $\times$  year fixed effects. Col. 6: control for ten segments  $\times$  year fixed effects (see Figure C.7). Col. 7: control for province  $\times$  year fixed effects. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns within 35 km of the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level (Panels A, D, E, F) or heteroschedastic robust (Panels B, C) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

## C.8 Robustness to controlling for predetermined characteristics

Table C.3: Robustness to controlling for predetermined characteristics. Distance.

	Distance										
	(1) Baseline	(2) PRI 48	(3) % Active	(4) 51 Dist coast	(5) Dist Rome	(6) Area	(7) Slope	(8) Elev.	(9) Wheat suit.	(10) Maize suit.	(11) All
<b>A. DC vote share</b>											
Treatment × 1946	-0.009 [0.016]	-0.009 [0.016]	-0.008 [0.016]	-0.009 [0.016]	-0.009 [0.016]	-0.009 [0.016]	-0.009 [0.016]	-0.009 [0.016]	-0.009 [0.016]	-0.009 [0.016]	-0.008 [0.016]
Treatment × Post	0.043** [0.017]	0.044*** [0.017]	0.046*** [0.017]	0.041** [0.017]	0.041** [0.016]	0.043** [0.017]	0.043** [0.017]	0.044** [0.017]	0.044** [0.017]	0.043** [0.017]	0.041*** [0.015]
<b>B. Casse Mutue revenues p.c.</b>											
Treatment	156.689*** [59.730]	133.359** [59.295]	161.013*** [59.629]	149.801** [60.520]	156.971*** [59.748]	153.115** [59.436]	156.886*** [59.980]	154.472** [60.117]	155.218*** [59.947]	152.060** [59.840]	115.317** [55.880]
<b>C. Casse Mutue expenses p.c.</b>											
Treatment	101.980*** [32.439]	89.263*** [30.398]	103.643*** [32.234]	98.971*** [32.423]	104.375*** [32.153]	115.126*** [33.482]	102.279*** [32.843]	103.819*** [33.159]	103.585*** [32.404]	103.460*** [32.751]	101.495*** [31.900]
<b>D. Casse Mutue votes p.c.</b>											
Treatment	0.015** [0.007]	0.014* [0.007]	0.015** [0.007]	0.015** [0.007]	0.017** [0.007]	0.016** [0.007]	0.013* [0.007]	0.013* [0.007]	0.015** [0.007]	0.015** [0.007]	0.013* [0.007]
<b>E. Coldiretti votes p.c.</b>											
Treatment	0.012** [0.006]	0.011* [0.006]	0.011* [0.006]	0.012** [0.006]	0.013** [0.006]	0.013** [0.006]	0.010* [0.006]	0.010* [0.006]	0.012** [0.006]	0.012** [0.006]	0.009 [0.006]
<b>F. Public Sector Employment</b>											
Treatment × 1936	0.008 [0.006]	0.006 [0.006]	0.009 [0.005]	0.009 [0.006]	0.006 [0.005]	0.009 [0.006]	0.009 [0.006]	0.009 [0.006]	0.009 [0.006]	0.009 [0.006]	0.007 [0.005]
Treatment × Post	0.017*** [0.007]	0.017** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017*** [0.007]	0.017** [0.007]

Notes: The Table reports coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area  $\times$  year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area  $\times$  year fixed effects. Panel A: The omitted category is the  $\beta$  of the elections of 1948. Panel F: omitted category is the  $\beta$  of 1951. Col. 1: baseline specification controlling for a linear polynomial in distance. Col. 2-10: include the control specified in the column title. Col. 11: include all controls together. Panel A, F: the controls are interacted with a post 1951 dummy. Panel B, C, D, E: the controls are included without interaction. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns within 20 km of the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level (Panels A, D, E, F) or heteroschedastic robust (Panels B, C) in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table C.4:** Robustness to controlling for predetermined characteristics.  
Latitude-Longitude.

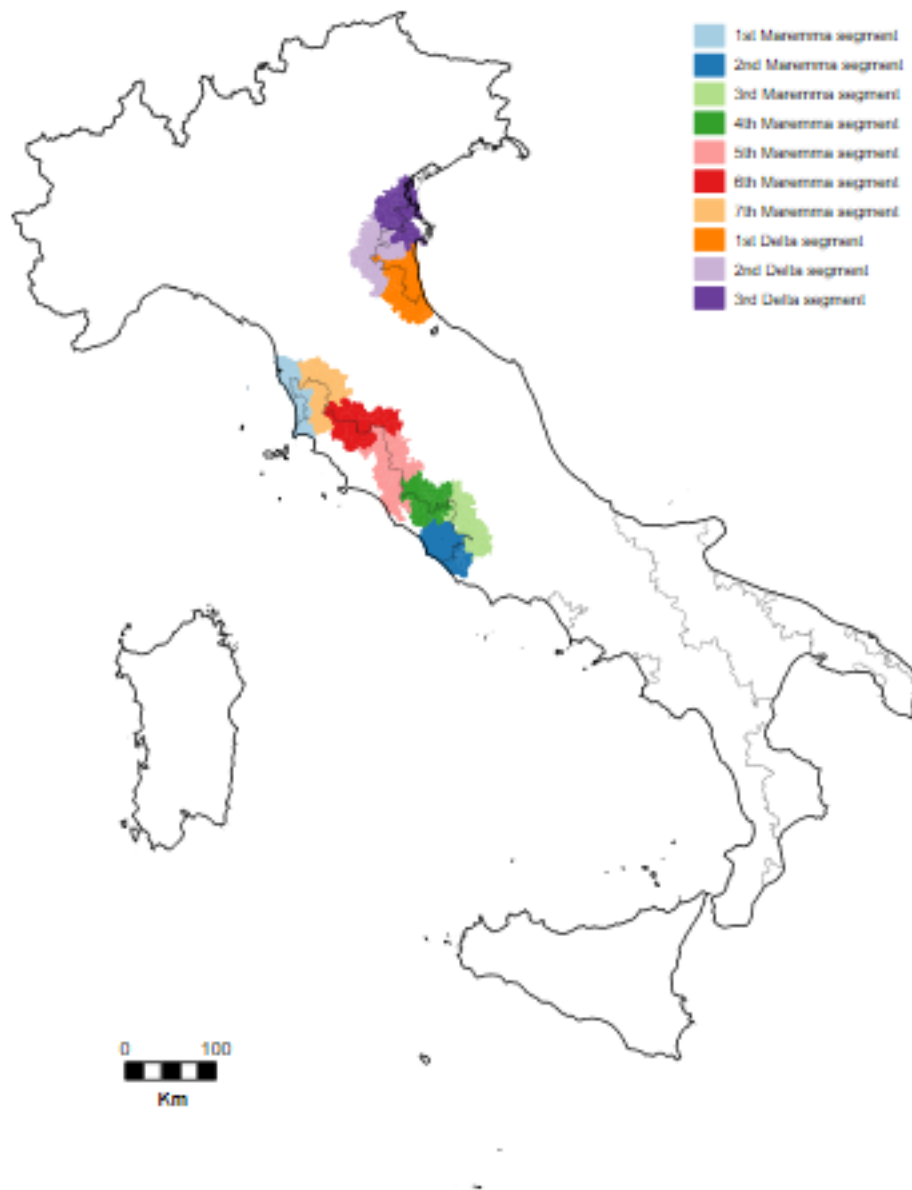
	Latitude-Longitude										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	PRI 48	% Active 51	Dist coast	Dist Rome	Area	Slope	Elev.	Wheat suit.	Maize suit.	All
<b>A. DC vote share</b>											
Treatment × 1946	-0.000	-0.000	0.001	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	0.001
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
Treatment × Post	0.040***	0.041***	0.036***	0.040***	0.041***	0.040***	0.040***	0.041***	0.041***	0.041***	0.048***
	[0.009]	[0.009]	[0.008]	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
<b>B. Casse Mutue revenues p.c.</b>											
Treatment	123.425***	112.330***	122.016***	113.297***	125.136***	144.592***	119.336***	123.113***	125.085***	123.622***	112.711***
	[36.147]	[35.773]	[35.933]	[37.017]	[35.921]	[37.129]	[36.404]	[36.417]	[36.168]	[36.390]	[37.413]
<b>C. Casse Mutue expenses p.c.</b>											
Treatment	101.980***	89.263***	103.643***	98.971***	104.375***	115.126***	102.279***	103.819***	103.585***	103.460***	101.495***
	[32.439]	[30.398]	[32.234]	[32.423]	[32.153]	[33.482]	[32.843]	[33.159]	[32.404]	[32.751]	[31.900]
<b>D. Casse Mutue votes p.c.</b>											
Treatment	0.010***	0.009**	0.011***	0.009**	0.010***	0.014***	0.010***	0.010***	0.010***	0.010***	0.011***
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
<b>E. Coldiretti votes p.c.</b>											
Treatment	0.008***	0.007**	0.009***	0.007**	0.008***	0.012***	0.008***	0.008***	0.009***	0.008***	0.008***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
<b>F. Public Sector Employment</b>											
Treatment × 1936	0.006*	0.006	0.006	0.005	0.009**	0.004	0.006*	0.007*	0.006*	0.007*	0.008*
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Treatment × Post	0.019***	0.020***	0.019***	0.019***	0.019***	0.019***	0.019***	0.019***	0.019***	0.019***	0.020***
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]

*Notes:* The Table reports coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area × year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area × year fixed effects. Panel A: The omitted category is the  $\beta$  of the elections of 1948. Panel F: omitted category is the  $\beta$  of 1951. Col. 1: baseline specification controlling for a quadratic polynomial in latitude and longitude, interacted with reform area identifiers. Col. 2-10: include the control specified in the column title. Col. 11: include all controls together. Panel A, F: the controls are interacted with a post 1951 dummy. Panel B, C, D, E: the controls are included without interaction. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns within 35 km of the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level (Panels A, D, E, F) or heteroschedastic robust (Panels B, C) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.



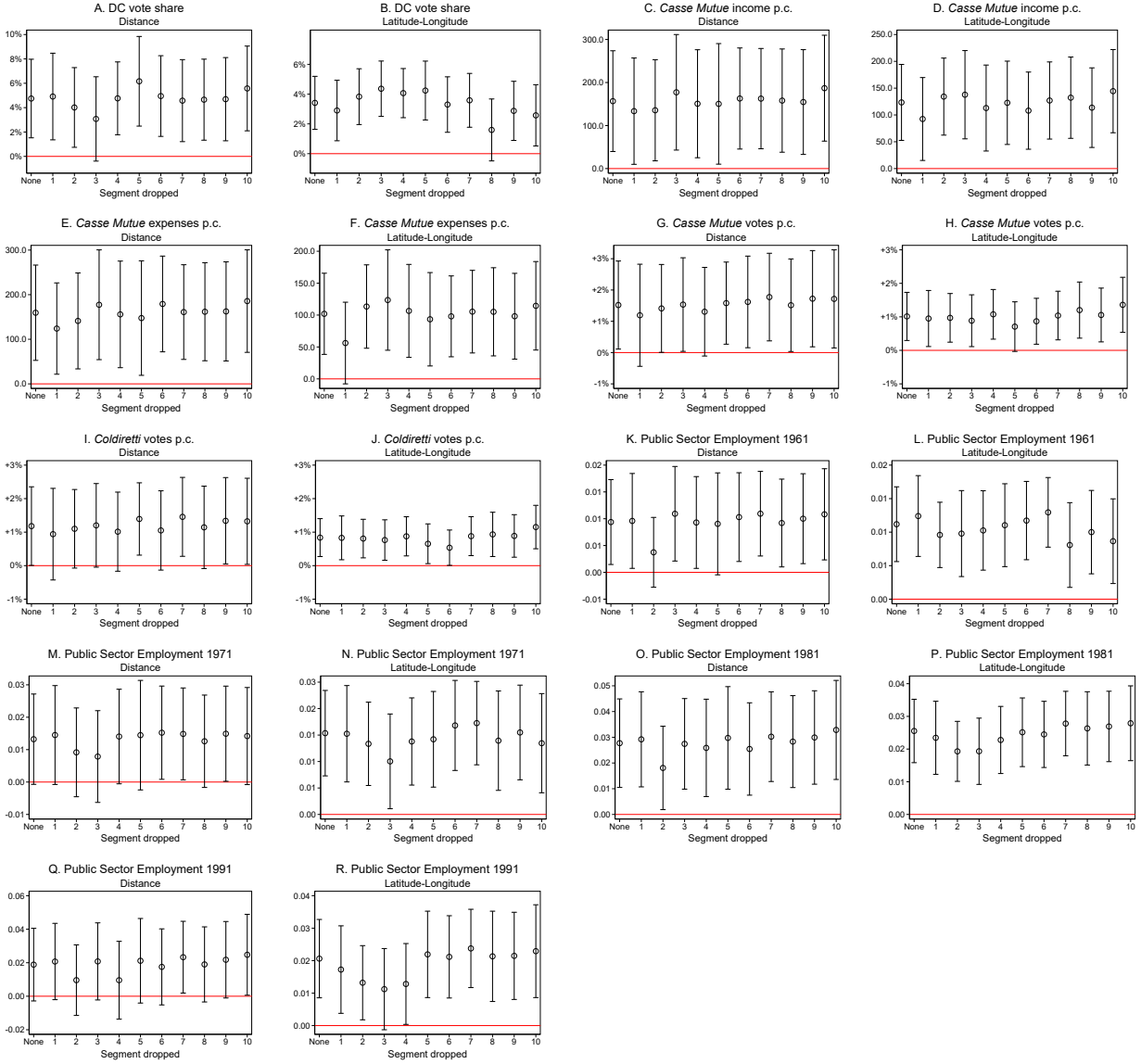
## C.9 Robustness to Dropping Portions of the Reform Border

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



*Notes:* The Map shows how we split the borders of Delta Padano and Maremma into 10 segments of equal length. Each towns within 20 km of the border of Delta Padano and Maremma is assigned to the closest segment. We report estimates of Equation (1) when we drop each of these segments in Figure C.8.

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



Notes: The Figure reports coefficients  $\beta_t$  and  $\beta$  from Equations (1) (Panels A, B, and K to R) and (2) (Panels C to J). Panels A, B, and K to R: regressions control for reform area  $\times$  year and town fixed effects. Panel C to F: regressions control for reform area fixed effects. Panels G to J: regressions control for reform area  $\times$  year fixed effects. Panels A, B, and K to R: we estimate a single coefficient for treated towns in the post-reform years (1953-92). The first estimate (point “None” on the x-axis) corresponds to our baseline coefficient. We obtain the other coefficients after dropping all towns closest to one of the 10 segments marked on Map C.7. Units of observation are town-years (Panels A, B, and G to R) or towns (Panels C to F). The subtitle *Distance* indicates all regressions controlling for a linear polynomial in distance: for these the sample consists of all towns within 20 km of the reform borders of Delta Padano and Maremma. The subtitle *Latitude-Longitude* indicates all regressions controlling for a quadratic polynomial in latitude and longitude: for these the sample consists of all towns within 35 km of the reform borders of Delta Padano and Maremma. We estimate standard errors clustered at the town level (Panels A, B, and G to R) or heteroscedasticity robust (Panels C to F) and plot 95% confidence intervals as bars around the coefficients.

## C.10 Instrumental Variable

**Table C.5:** RF: the impact of the land reform.

	Distance			Latitude-Longitude		
	<i>Preferred Bandwidth</i> < 20km	<i>Alternative Bandwidths</i> < 10km < 50km		<i>Preferred Bandwidth</i> < 35km	<i>Alternative Bandwidths</i> < 10km < 50km	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. DC vote share</b>						
Treatment × 1946	-0.013 [0.014]	-0.030 [0.019]	0.003 [0.011]	-0.012* [0.007]	-0.023** [0.009]	-0.006 [0.007]
Treatment × 1950s	0.033*** [0.013]	0.017 [0.018]	0.034*** [0.009]	0.018*** [0.007]	0.013 [0.008]	0.019*** [0.006]
Treatment × 1960s	0.043*** [0.015]	0.032 [0.022]	0.037*** [0.011]	0.028*** [0.009]	0.020* [0.011]	0.033*** [0.008]
Treatment × 1970s	0.050*** [0.015]	0.040* [0.022]	0.046*** [0.011]	0.037*** [0.009]	0.028** [0.011]	0.042*** [0.008]
Treatment × 1980s	0.034** [0.017]	0.015 [0.025]	0.051*** [0.014]	0.038*** [0.011]	0.024* [0.013]	0.048*** [0.010]
<b>B. Casse Mutue revenues p.c.</b>						
Treatment	111.714** [50.684]	80.698 [67.909]	76.724* [42.428]	89.354*** [31.895]	111.381*** [36.195]	110.221*** [34.689]
<b>C. Casse Mutue expenses p.c.</b>						
Treatment	139.840*** [44.735]	128.206** [57.409]	100.996*** [37.325]	83.226*** [27.775]	104.140*** [33.484]	105.060*** [29.355]
<b>D. Casse Mutue votes p.c.</b>						
Treatment	0.008 [0.005]	0.006 [0.008]	0.010** [0.004]	0.010*** [0.003]	0.011*** [0.004]	0.012*** [0.003]
<b>E. Coldiretti votes p.c.</b>						
Treatment	0.009** [0.004]	0.009 [0.006]	0.009*** [0.003]	0.010*** [0.003]	0.010*** [0.003]	0.011*** [0.003]
<b>F. Public Sector Employment</b>						
Treatment × 1936	0.013*** [0.005]	0.011* [0.006]	0.006* [0.003]	0.007** [0.004]	0.005 [0.003]	0.008*** [0.003]
Treatment × 1961	0.008** [0.004]	0.009* [0.005]	0.009*** [0.002]	0.009*** [0.002]	0.005* [0.003]	0.009*** [0.002]
Treatment × 1971	0.010 [0.006]	0.003 [0.009]	0.004 [0.004]	0.014*** [0.003]	0.006 [0.004]	0.013*** [0.003]
Treatment × 1981	0.023*** [0.008]	0.017 [0.012]	0.015*** [0.005]	0.024*** [0.004]	0.020*** [0.005]	0.020*** [0.004]
Treatment × 1991	0.012 [0.010]	0.007 [0.014]	0.005 [0.007]	0.020*** [0.005]	0.013** [0.006]	0.018*** [0.005]

*Notes:* The Table reports reduced form coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Panels A, D, E, F) and (2) (Panels B, C). The independent variables are treatment and distance to the borders of “Zone B” in the *legge di riforma agraria* which was proposed and never enacted, but formed the basis of the actual reform. Units of observation are town-years (Panels A, D, E, F) or towns (Panels B, C). The sample consists of all towns close to the reform borders of Delta Padano and Maremma. Estimates reported for the preferred bandwidth (20 and 35 km) and two alternative bandwidths (10 km and 50 km). Columns 1 to 3 use a linear specification in distance, columns 4 to 6 use a quadratic specification in latitude and longitude. Standard errors clustered at the town level (Panels A, D, E, F) or heteroschedastic robust (Panels B, C) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

**Table C.6:** First stage and IV: the impact of the land reform.

	Distance			Latitude-Longitude		
	<i>Preferred</i>	<i>Alternative</i>		<i>Preferred</i>	<i>Alternative</i>	
	<i>Bandwidth</i>	<i>Bandwidths</i>		<i>Bandwidth</i>	<i>Bandwidths</i>	
	< 20km	< 10km	< 50km	< 35km	< 10km	< 50km
	(1)	(2)	(3)	(4)	(5)	(6)
<b>First stage (cross-section)</b>						
Treatment	0.561*** [0.073]	0.548*** [0.109]	0.639*** [0.058]	0.709*** [0.041]	0.657*** [0.048]	0.737*** [0.039]
Joint F-test	282.91	78.84	425.99	304.59	184.80	360.85
Observations	426	238	885	657	238	885
<b>A. DC vote share</b>						
Treatment × 1946	-0.022 [0.023]	-0.047 [0.034]	0.005 [0.016]	-0.018* [0.011]	-0.035** [0.015]	-0.008 [0.010]
Treatment × 1950s	0.055** [0.022]	0.033 [0.033]	0.052*** [0.014]	0.026** [0.010]	0.021 [0.013]	0.026*** [0.009]
Treatment × 1960s	0.073*** [0.026]	0.058 [0.040]	0.058*** [0.017]	0.040*** [0.013]	0.031* [0.017]	0.046*** [0.011]
Treatment × 1970s	0.084*** [0.026]	0.072* [0.040]	0.073*** [0.018]	0.054*** [0.014]	0.043** [0.018]	0.060*** [0.012]
Treatment × 1980s	0.059** [0.029]	0.032 [0.045]	0.080*** [0.021]	0.055*** [0.016]	0.037* [0.020]	0.066*** [0.014]
<b>B. Casse Mutue revenues p.c.</b>						
Treatment	199.661*** [77.132]	158.935 [114.174]	128.100** [59.479]	172.947*** [41.905]	177.712*** [59.973]	149.495*** [45.904]
<b>C. Casse Mutue expenses p.c.</b>						
Treatment	236.030*** [71.593]	231.696** [95.552]	161.423*** [54.077]	173.321*** [38.552]	166.308*** [52.662]	142.494*** [39.282]
<b>D. Casse Mutue votes p.c.</b>						
Treatment	0.022** [0.011]	0.031 [0.029]	0.019*** [0.007]	0.019*** [0.005]	0.021*** [0.007]	0.019*** [0.005]
<b>E. Coldiretti votes p.c.</b>						
Treatment	0.023** [0.009]	0.035 [0.024]	0.017*** [0.006]	0.016*** [0.004]	0.020*** [0.006]	0.017*** [0.004]
<b>F. Public Sector Employment</b>						
Treatment × 1936	0.021*** [0.008]	0.019* [0.011]	0.009* [0.005]	0.007 [0.005]	0.008 [0.005]	0.008** [0.004]
Treatment × 1961	0.013** [0.006]	0.016* [0.008]	0.012*** [0.004]	0.010*** [0.003]	0.007* [0.004]	0.008*** [0.003]
Treatment × 1971	0.015 [0.010]	0.005 [0.014]	0.005 [0.006]	0.015*** [0.005]	0.009 [0.007]	0.014*** [0.005]
Treatment × 1981	0.037*** [0.013]	0.030* [0.018]	0.021*** [0.008]	0.026*** [0.006]	0.032*** [0.008]	0.023*** [0.006]
Treatment × 1991	0.020 [0.016]	0.013 [0.022]	0.006 [0.010]	0.019** [0.008]	0.020** [0.009]	0.017** [0.007]

*Notes:* First stage estimated with Equation (2). IV estimated with Equations (1) (Panels A, D, E, F) and (2) (Panels B, C). Instruments 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: town-years (Panels A, D, E, F) or towns (Panels B, C). Sample: all towns close to the reform borders of Delta Padano and Maremma. Preferred bandwidth: 20 km (distance) and 35 km (coordinates). Cols 1–3: linear specification in distance, cols 4–6: quadratic specification in coordinates. S.e. clustered at town level (Panels A, D, E, F) or heteroschedastic (Panels B, C) in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

## C.11 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>39</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 with the two specifications are in Columns 1 and 2 of Table C.7: both coefficients are insignificant, and have opposite signs. Because land invasions are a form of resentment, these results are not consistent with greater grievances in control towns.

***Heterogeneity by share of agricultural workers.*** Second, if resentment were a factor, the reduction in support for the Christian Democrats would likely be higher in control towns with a higher share of agricultural workers, as these workers would have higher benefits from the reform. To test this hypothesis, we study heterogeneity by the pre-reform share of agricultural workers in a difference-in-difference specification.<sup>40</sup>

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

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>41</sup> If angry potential beneficiaries in control towns punished DC, we would expect  $\gamma$  to be negative.

In Table C.7 we compare pre-reform elections (1946, 48) with the first two elections after the reform (1953, 58). Column 3 shows that, in control towns, places with high and low share of agricultural workers showed similar support for DC after the reform (if anything, support

<sup>39</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>40</sup>We include towns in a 20 km bandwidth. We performed the same exercise with a more complex panel RDD specification, which requires more power than the difference-in-differences. However, the specification controlling for latitude and longitude suffers from overfitting. Results for the RDD controlling for a linear polynomial in distance are similar and available upon request.

<sup>41</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.

for DC is higher in towns with more agricultural workers:  $\gamma=0.016$ , s.e.=0.019).<sup>42</sup>

**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.7-Column 4 report the estimates of modified versions of Equation (C.1), where we interact  $Post_t$  and  $T_i$  with the share of the town limits located on the reform border ( $B_i^T/B_i$ ). The positive and insignificant coefficient on the interaction between  $Post_t$  and  $B_i^T/B_i$  suggests that control towns where the reform was more visible did not vote against DC after the reform. Overall, these regressions suggest again that resentment in control towns is unlikely to drive our main results.

**Donut Panel RDD.** In the last exercise, we estimate Equation (1), but drop towns close to the reform border.<sup>43</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.8-Columns 1 and 5 report our baseline results: this is the increase in the support for DC after 1950 in treated towns within 20 (col. 1) and 35 (col. 5) km from the border. In Columns 2 through 4 and 6 through 8 of Table C.8, we estimate the same regressions after dropping towns that are within 1.5, 2.5 and 5 km from the border. Across these samples, the point estimate remains generally stable and highly significant. 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>42</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 ( $\delta=0.056$ , s.e.=0.042).

<sup>43</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.7:** Tests of resentment.

	Land Invasions		Christian Democrats	
	(1)	(2)	(3)	(4)
Treatment	-0.031	0.084		
	[0.112]	[0.074]		
Treatment $\times$ Post			-0.006	0.030*
			[0.025]	[0.015]
Share agricultural workers $\times$ Post			0.016	
			[0.019]	
Share agricultural workers $\times$ Treatment $\times$ Post			0.056	
			[0.042]	
Share of town limit on reform border $\times$ Post				0.013
				[0.026]
Share of town limit on reform border $\times$ Treatment $\times$ Post				-0.017
				[0.050]
Mean Y Control	0.06	0.05	0.36	0.31
Number of Towns	406	647	401	155
Bandwidth	20 Km	35 Km	20 Km	20 Km
Specification	Distance	Lat-Long	DiD	DiD
Observations	406	647	1601	620

*Notes:* The Table reports coefficients from Equation (2) (cols. 1 and 2) and (C.1) (cols. 3 and 4). Cols. 1 and 2 controls for reform area fixed effects; cols. 3 and 4 control for year  $\times$  reform area and town fixed effects. *Post* = 1 for elections after the land reform (1953-58). There are 2 elections before the reform: 1946 and 1948. Column 1: one-dimensional RDD controlling for a linear polynomial in distance. Column 2: two-dimensional RDD controlling for a quadratic polynomial in latitude and longitude. Column 3: difference-in-differences with heterogeneity in share of agricultural workers. Column 4: difference-in-differences with heterogeneity in share of town limits touching the reform border. Columns 1 and 2: dependent variable is land invasions in 1951-52. Columns 3 and 4: dependent variable is Christian Democrat (DC) vote share. Share of agricultural workers is from the population census. Units of observation are town-years (cols. 3 and 4) and towns (cols. 1 and 2). Columns 1 and 3: the sample consists of all towns within 20 km to the reform borders of Delta Padano and Maremma. Column 2: the sample consists of all towns within 35 km to the reform borders of Delta Padano and Maremma. Column 4: 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.8:** Donut Panel RDD. Effect on Christian Democrats vote share.

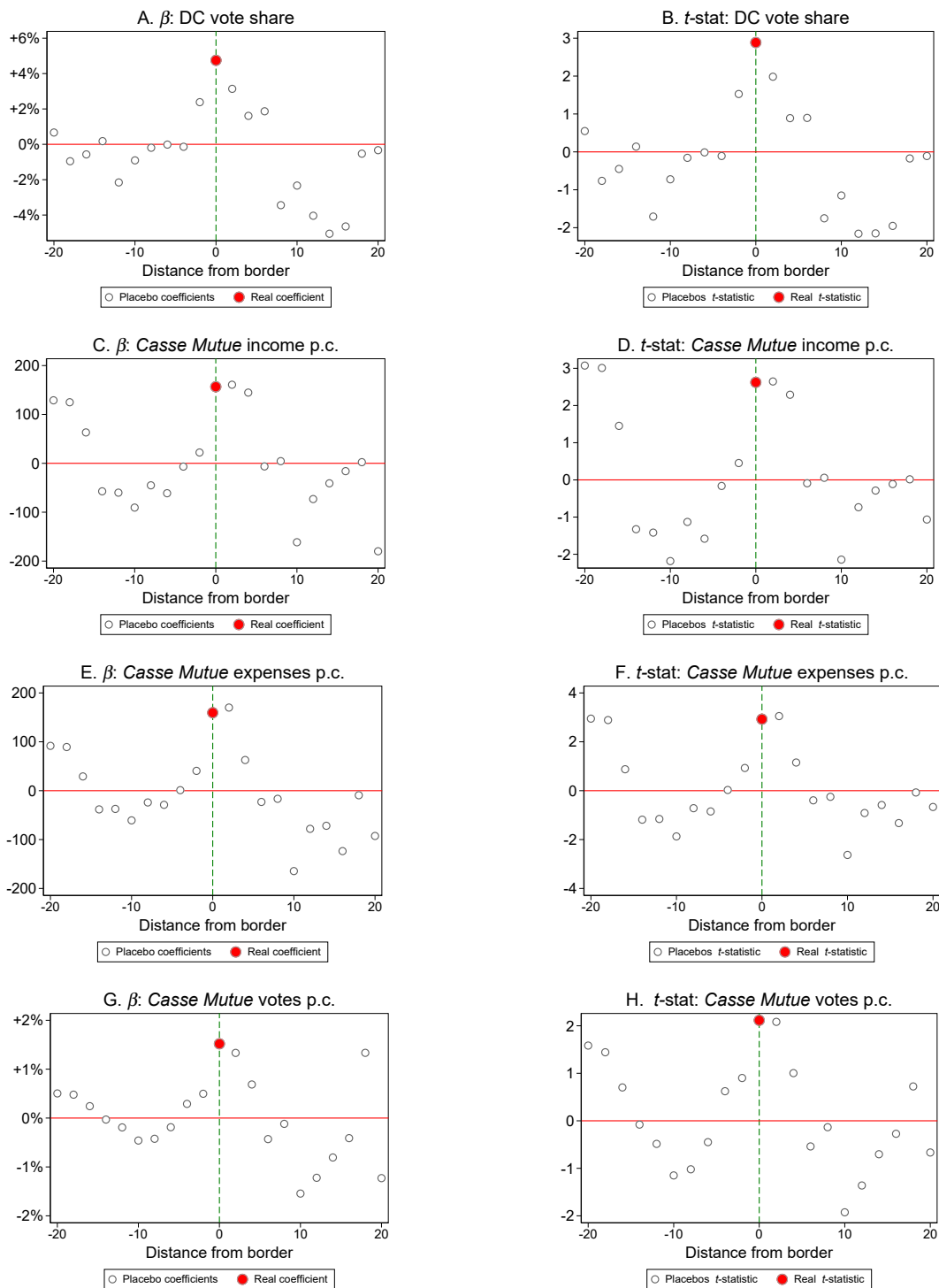
	Distance					Latitude-Longitude		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Donut: 1.5 km	Donut: 2 km	Donut: 2.5 km	All	Donut: 1.5 km	Donut: 2 km	Donut: 2.5 km
Treatment $\times$ 1950s	0.044*** [0.015]	0.035*** [0.013]	0.032** [0.014]	0.041*** [0.015]	0.021*** [0.008]	0.016** [0.007]	0.015** [0.007]	0.018** [0.008]
Treatment $\times$ 1960s	0.045** [0.018]	0.041** [0.016]	0.038** [0.017]	0.045** [0.018]	0.029*** [0.009]	0.026*** [0.009]	0.026*** [0.009]	0.027*** [0.009]
Treatment $\times$ 1970s	0.051*** [0.017]	0.050*** [0.018]	0.047** [0.019]	0.047** [0.020]	0.037*** [0.010]	0.037*** [0.010]	0.036*** [0.010]	0.035*** [0.010]
Treatment $\times$ 1980s	0.048** [0.019]	0.053*** [0.020]	0.051** [0.020]	0.055** [0.022]	0.043*** [0.011]	0.044*** [0.012]	0.045*** [0.012]	0.045*** [0.012]
Mean Y Control Group	0.35	0.36	0.36	0.36	0.36	0.37	0.37	0.37
Number of Towns	406	387	377	360	647	628	618	601
Observations	4845	4622	4502	4298	7696	7473	7353	7149

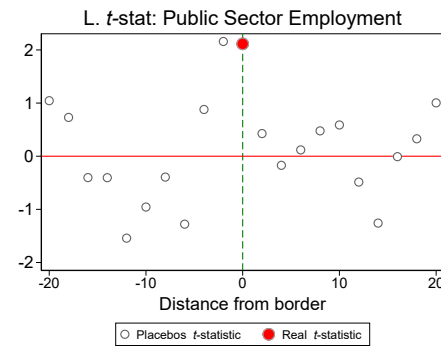
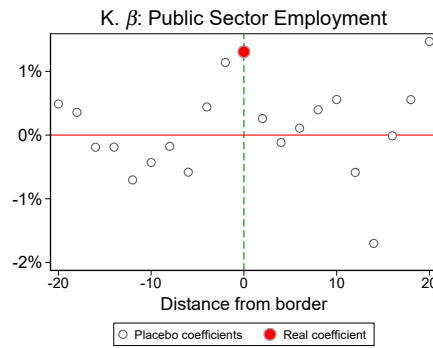
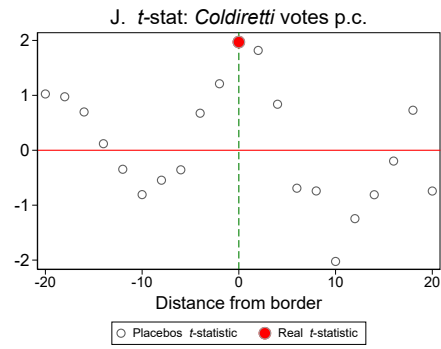
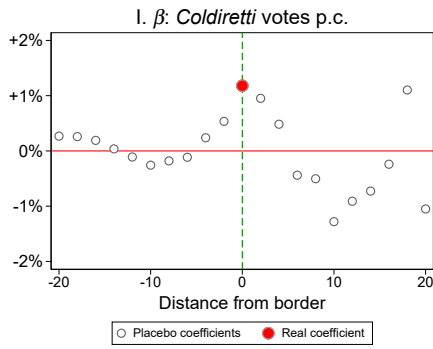
*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. We include the 1992 election in the 1980s decade. The omitted category is the  $\beta$  of the elections of 1946 and 1948. The dependent variable is Christian Democrat (DC) vote share. Columns 1-4 control for a linear polynomial in distance, columns 5-8 control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. Column 1 and 5: baseline; the sample consists of all towns within 20 km (cols. 1-4) and 35 km (cols. 5-8) to the reform borders of Delta Padano and Maremma. Columns 2-4 and 6-8: “donut” RDD; the sample consists of all towns within 20 km (cols. 1-4) or 35 km (cols. 5-8) 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.12 Placebo Borders

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





*Notes:* The Panels report results of placebo regressions discussed in Section 5.3. We simulate 20 fictitious reforms, by moving the reform border inside and outside the reform area in steps of 2 km and creating a new sample of all towns within 20 km from this new border. For each of these fictitious reforms, we estimate a single coefficient for the impact of the reform on in the post-reform years. Panels A, C, E, G, I and K: estimated  $\beta$ . Panels B, D, F, H, J and L:  $t$ -statistics calculated from standard errors clustered at the town level (Panels B, H, J and L) or heteroschedastic robust (Panels D and F). In every panel we plot in red the coefficient and  $t$ -statistic we obtain when we estimate the effect in the true reform area. Units of observation are town-years (Panels A, B and G-L) or towns (Panels C-F).

## C.13 Spatial Standard Errors

Table C.9: Standard error robust to temporal and spatial correlation.

Dep. var.:	<i>Casse Mutue</i>					Public sct.
	DC	Revenues p.c.	Expenditure p.c.	Votes p.c.	<i>Coldiretti</i> votes p.c.	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A. Distance</b>						
Treatment $\times$ Post	0.047	156.689	159.537	0.015	0.012	0.013
Cluster: town	(0.016)***	(59.730)***	(54.521)***	(0.007)**	(0.006)**	(0.006)**
Conley s.e.: cutoff = 5 km	(0.016)***	(59.209)***	(54.942)***	(0.007)**	(0.006)**	(0.006)**
Conley s.e.: cutoff = 10 km	(0.016)***	(59.532)***	(55.988)***	(0.007)**	(0.006)**	(0.006)**
Conley s.e.: cutoff = 25 km	(0.017)***	(59.565)***	(57.155)***	(0.007)**	(0.006)*	(0.006)**
Conley s.e.: cutoff = 50 km	(0.018)***	(55.397)***	(56.087)***	(0.007)**	(0.005)**	(0.006)**
Conley s.e.: cutoff = 100 km	(0.020)**	(51.449)***	(51.609)***	(0.007)**	(0.005)**	(0.007)*
Mean Y Control Group	0.35	448.24	326.06	0.03	0.02	0.06
Number of Towns	406	404	404	325	324	406
Observations	4845	404	404	1212	1184	2435
<b>Panel B. Latitude-Longitude</b>						
Treatment $\times$ Post	0.034	123.425	101.980	0.010	0.008	0.015
Cluster: town	(0.009)***	(36.147)***	(32.439)***	(0.004)***	(0.003)***	(0.004)***
Conley s.e.: cutoff = 5 km	(0.009)***	(35.985)***	(32.570)***	(0.004)***	(0.003)***	(0.003)***
Conley s.e.: cutoff = 10 km	(0.009)***	(37.283)***	(33.490)***	(0.004)***	(0.003)***	(0.004)***
Conley s.e.: cutoff = 25 km	(0.009)***	(39.780)***	(36.211)***	(0.004)**	(0.003)**	(0.004)***
Conley s.e.: cutoff = 50 km	(0.009)***	(40.255)***	(37.873)***	(0.005)**	(0.004)**	(0.005)***
Conley s.e.: cutoff = 100 km	(0.010)***	(35.971)***	(35.765)***	(0.005)*	(0.004)*	(0.006)***
Mean Y Control Group	0.36	450.02	318.38	0.03	0.02	0.05
Number of Towns	647	645	645	532	530	647
Observations	7696	645	645	1912	1858	3881

*Notes:* The Table reports on the first row coefficients  $\beta_t$  and  $\beta$  from alternative specifications of Equations (1) (Cols. 1, 4, 5, 6) and (2) (Cols. 2, 3). Panel A controls for a linear polynomial in distance, Panel B for a quadratic polynomial in latitude and longitude. Cols. 1, 6: regressions control for reform area  $\times$  year and town fixed effects. Cols 2, 3: regressions control for reform area fixed effects. Cols. 4, 5: regressions control for reform area  $\times$  year fixed effects. Units of observation are town-years (Cols. 1, 4, 5, 6) or towns (Cols. 2, 3). The sample consists of all towns within 20 km (Panel A) and 35 km (Panel B) of the reform borders of Delta Padano and Maremma. Row 2 of each panel: standard errors clustered at the town level (Cols. 1, 4, 5, 6) or heteroschedastic robust (Cols. 2, 3) in parentheses. Rows 3-7 of each panel: standard errors robust to time-series and spatial correlation calculated with the formula of Conley (1999). In these estimates, spatial correlation is assumed to decay linearly until a cutoff. We report results from 5 different cutoffs: 5 km, 10 km, 25 km, 50 km and 100 km. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## C.14 RDD-robust standard errors

Table C.10: Robust RDD inference.

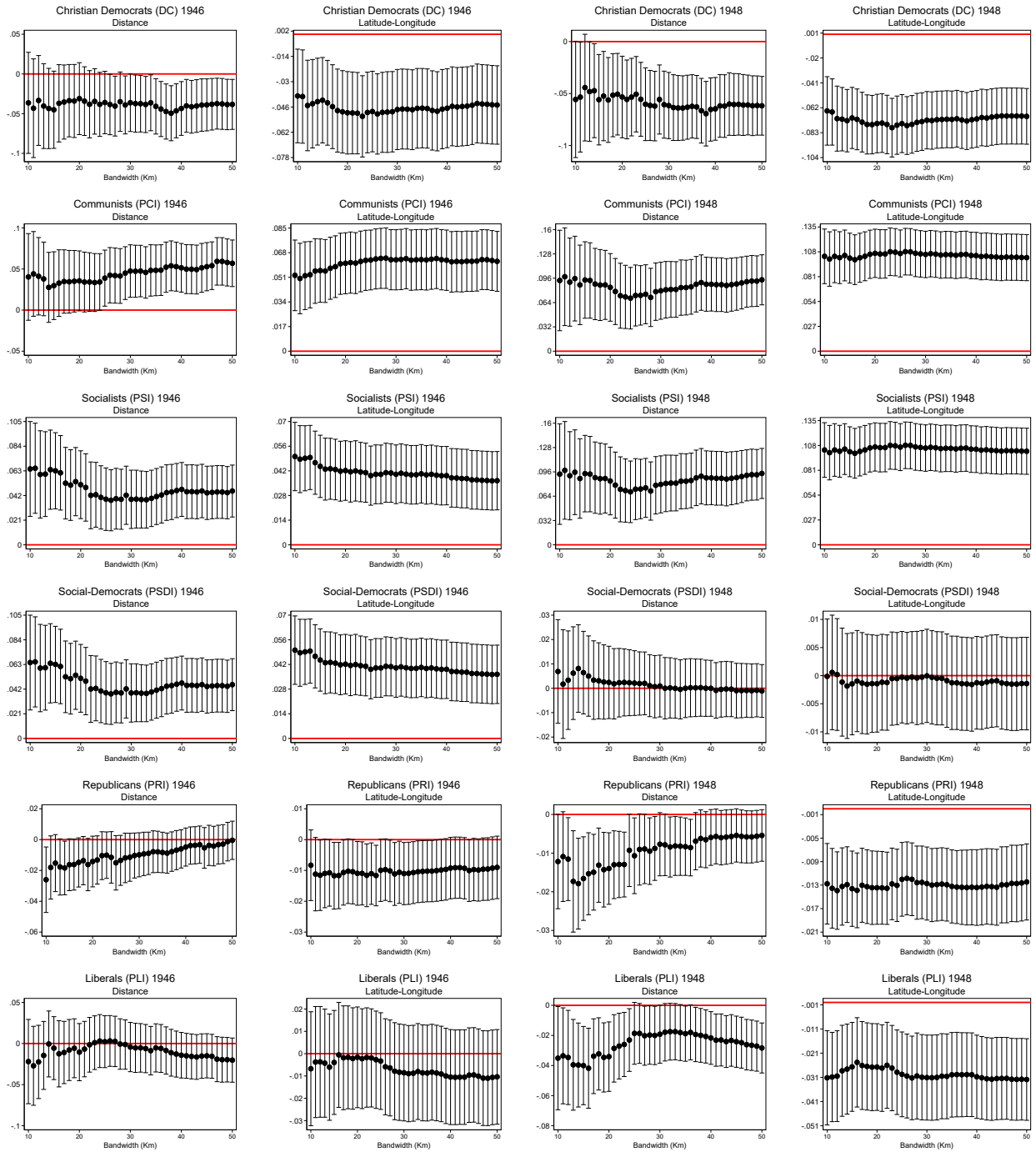
Dep. var.:	<i>Casse Mutue</i>					Public sect.
	DC	Revenues p.c.	Expenditure p.c.	Votes p.c.	<i>Coldiretti</i> votes p.c.	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0499**	166.7	164.0*	0.0326**	0.0273***	0.0115
	(0.0192)	(80.89)	(72.17)	(0.0116)	(0.00889)	(0.00681)
Observations	6352	1660	1660	1788	1780	4213
Covariates	Reform FEs	Reform FEs	Reform FEs	Reform FEs	Reform FEs	Reform FEs
Bandwidth	22.38	11.47	11.68	16.86	16.37	17.32
Conventional p-value	0.01	0.04	0.02	0.01	0.00	0.09
Robust p-value	0.04	0.10	0.07	0.02	0.01	0.13

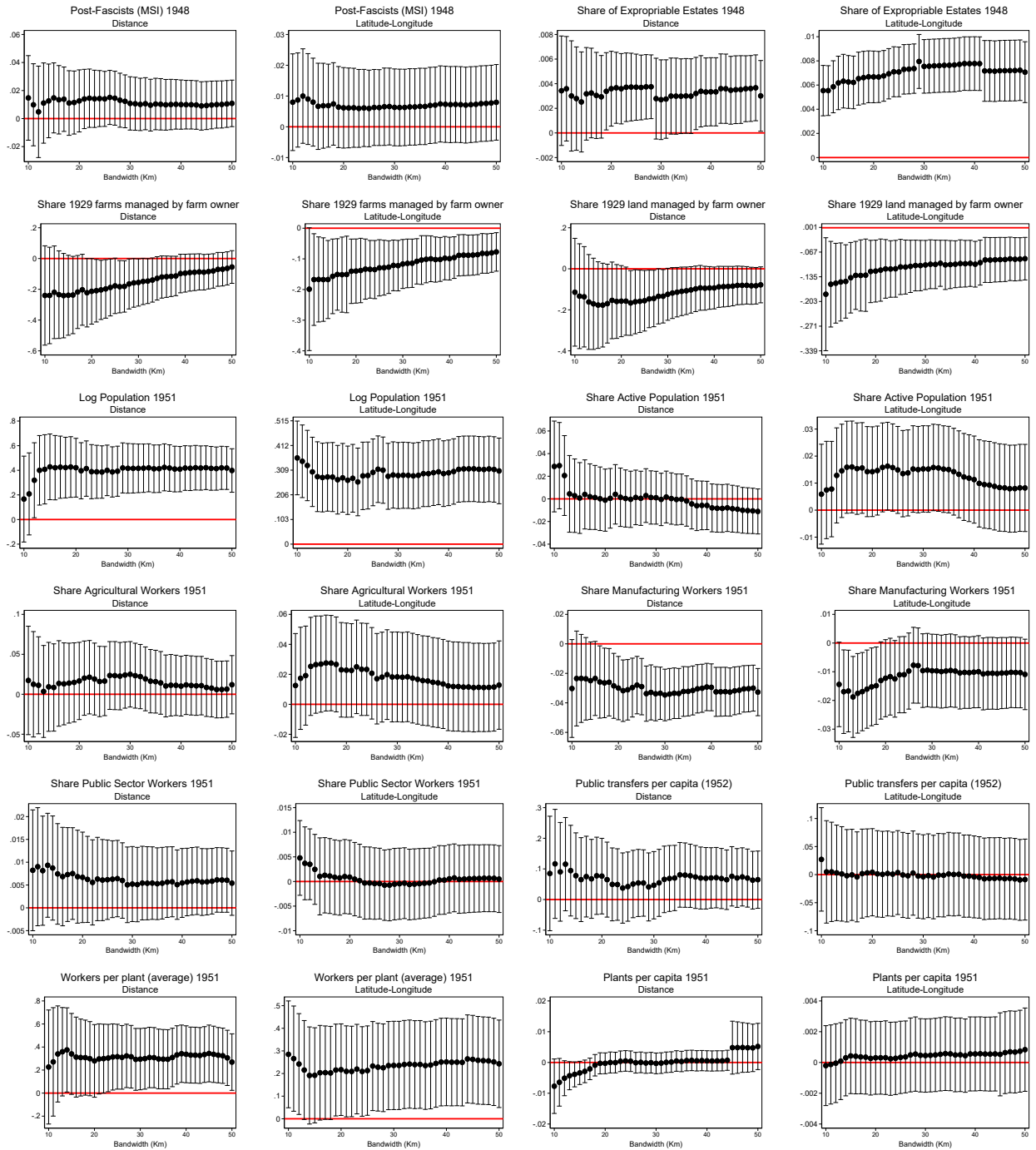
*Notes:* The Table reports coefficients  $\beta$  from Equation (2) estimated with a first order polynomial in distance with the Stata routine `rdrobust`. All regressions control for reform area fixed effects. Dependent variables: change in DC vote share from 1946-48 to 1953-92 (col. 1); *Casse Mutue* per capita revenues (col. 2), expenses (col. 3) and votes (col. 4); *Coldiretti* per capita votes (col. 5); change in public sector employment from 1936-51 to 1961-91 (col. 6). Units of observation are towns. The sample consists of all towns close to the reform borders of Delta Padano and Maremma, within a bandwidth specified at the bottom of the table. The last two rows of the table report two versions of standard errors: conventional, and bias-corrected robust (Calonico et al., 2014). \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

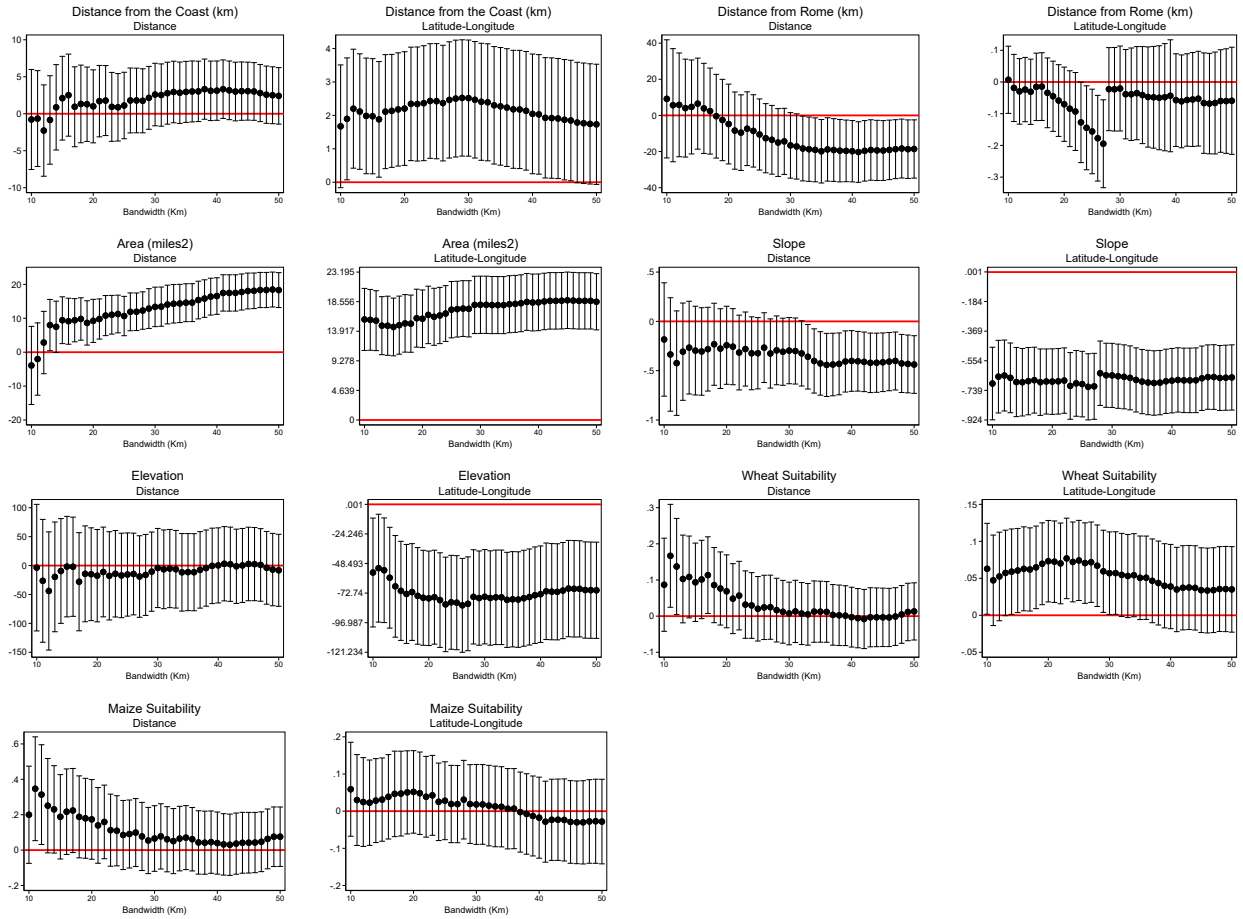
# D Additional Results

## D.1 Southern Italy

Figure D.1: Balance – Southern Italy

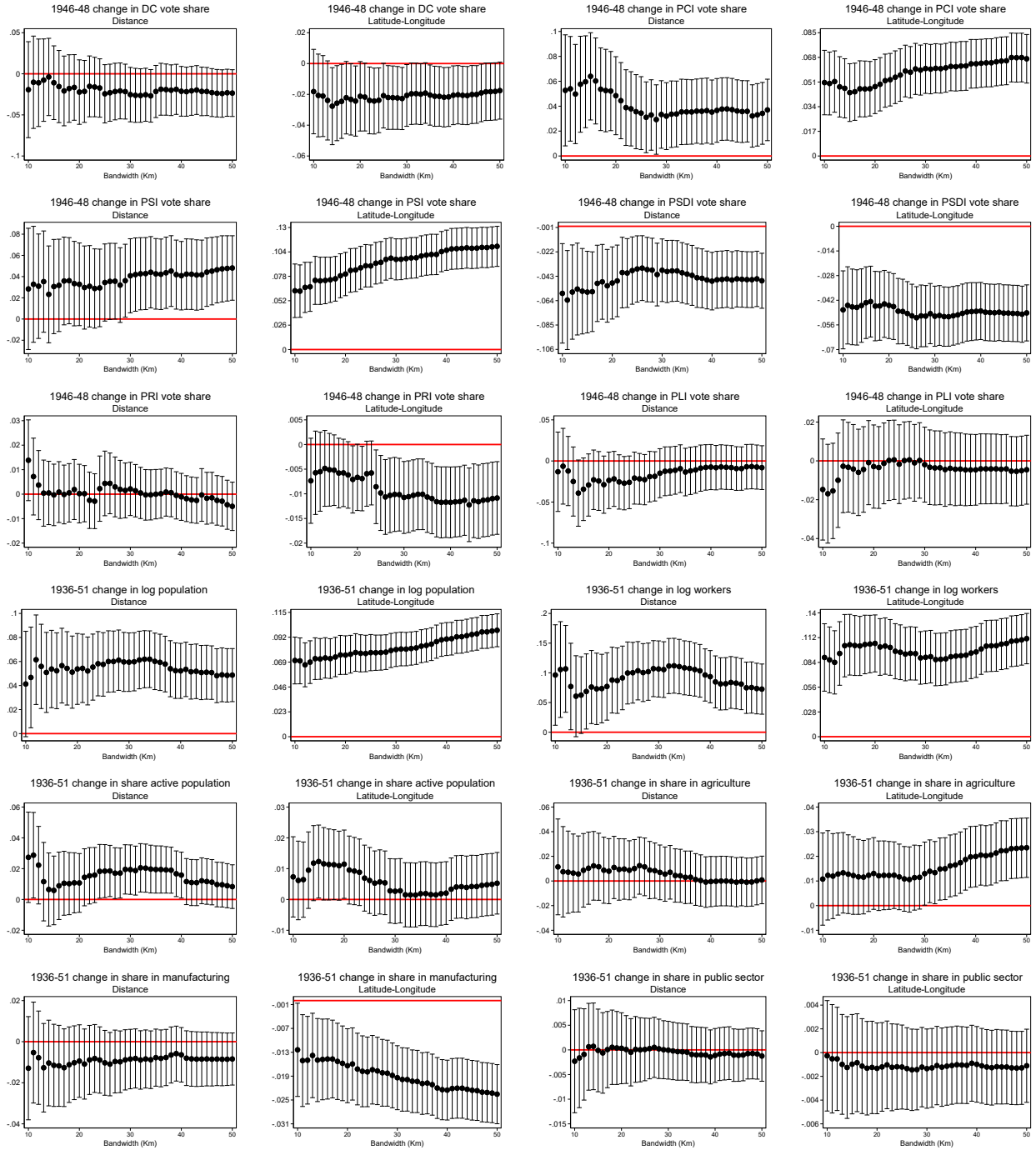






*Notes:* The graphs report the coefficients of separate regressions of the RDD specification in Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. The subtitle *Distance* indicates the RDD regressions controlling for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

Figure D.2: Pre-trends – Southern Italy

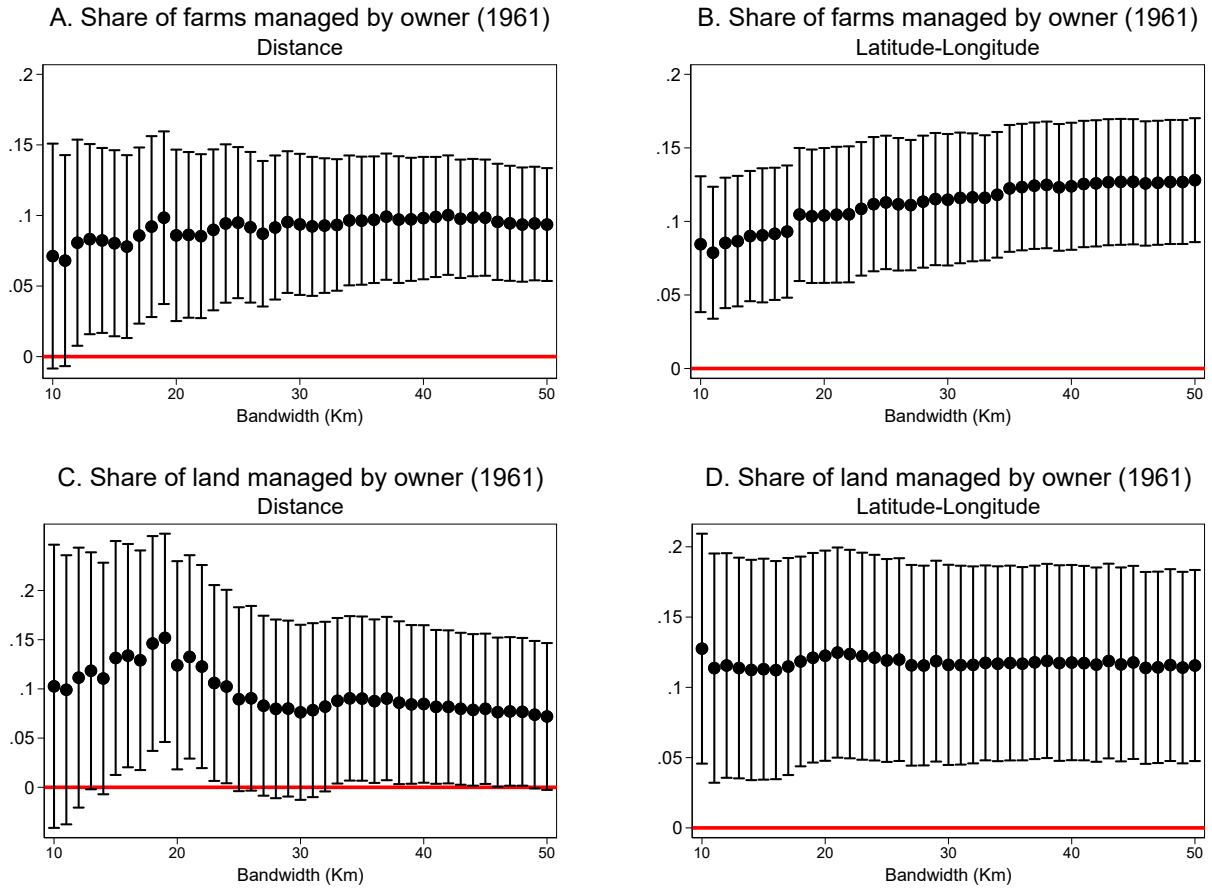


*Notes:* The graphs report the coefficients of separate regressions of the RDD specification in Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. Dependent variables are changes in the outcome variable between 1946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



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

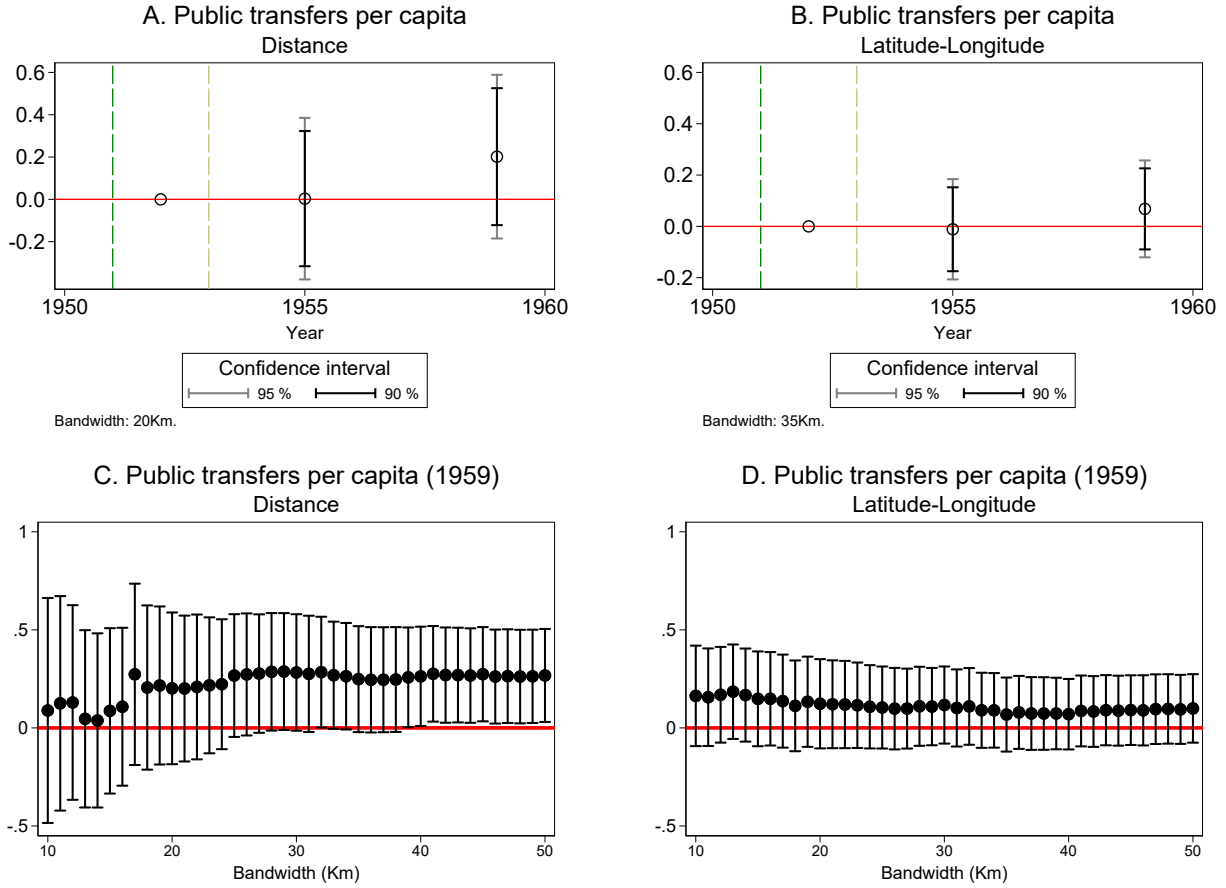
Figure D.4: The impact of the reform on farm management.



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

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

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



*Notes:* Dependent variable is the inverse sine of public transfers (from central to local government) per capita, a proxy for pork barrel spending. Panels A and B display coefficients  $\beta_t$  for each year from the panel RDD Equation (1). The omitted category is the  $\beta$  of 1952 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. The green vertical dashed lines mark the 1950 land reform, the yellow ones mark the end of the land redistribution (1953). Panels C and D report the coefficients for year 1959, for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. All regressions control for year  $\times$  reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

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

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

***Preliminary Graphical Evidence.*** Appendix Figure D.6 presents graphical evidence by plotting vote shares against distance to the border. PCI vote share was similar across the border in 1948 (Panel A) and shows parallel pre-trends between 1946 and 1948 (Panel B). The reform determined a large drop in the support for PCI: relative to pre-reform elections (1946-48), in 1953-1992 treated towns at the border reduce PCI support by 3 p.p. Appendix Figure D.7 shows these effects on a map of Italy, confirming that border towns inside of the reform area swung away from PCI.

***Panel RD Estimates.*** These effects appear immediately after the reform, and are stable until the 1990s. Appendix Figure D.8-Panels A-B plot the effect of the reform on PCI vote share over the entire period ( $\beta_t$  in Equation (1)) in the two specifications. The 1946 coefficient confirms the absence of differential trends pre-reform (see also Figure 2). The treatment coefficient in the 1953 election suggests that in treated towns PCI vote share decreased by between 2 and 4 p.p. during the first election after the land reform, from a control mean of 33%: a decrease of between 6 and 12 percent. This effect is larger but noisier in the distance specification. Finally, PCI loss remains large and quite stable until 1987, even as they become noisier in the 80s. When we pool together all post-reform elections until 1992 in Appendix Figure D.8-Panels C-D, we find that the size of the effect is stable across bandwidths. However, the effects are smaller and noisier in narrower bandwidths in the specification controlling for distance. These results confirm that the land redistribution was successful in stemming the rise of communism in the countryside.

***Pre-Fascism elections.*** Long-term pre-trends suggest that these effects may underestimate the true effect of the reform on PCI support. Appendix Figure D.9 reports treatment coefficients from a panel RDD regression that includes the 1919, 1921, and 1924 elections (the last ones before the Fascist dictatorship), as well as the 1946 and the 1948 ones. We look at two left-wing parties: the Italian Socialist Party (PSI) and the Italian Communist Party (PCI). PSI was the largest left-wing party until 1947: it won relative majorities in the elections of 1919 and 1921 and had one of his leaders, Giacomo Matteotti, killed by fascist hit men in 1924.<sup>44</sup> PCI was relatively small before the war: founded in 1921, it collected

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<sup>44</sup>PSI lost ground to PCI after 1947, when it split into 2 parties. One of these parties ran together with

4.6% of votes that year and 3.7% in 1924. We report pre-fascism elections results for the two specifications at 25 km, for both parties (Appendix Figure D.9). PSI vote shares exhibit parallel pre-trends in the specification controlling for a linear polynomial in distance, while the coefficients for 1921 and 1924 elections are negative and significant in the coordinate specification. PCI vote shares seem to grow faster in treatment towns, although pre-trends coefficients are not significantly different from zero in the specification controlling for a linear polynomial in distance. Growing support for left-wing parties pre-reform may bias the PCI treatment effect downward.

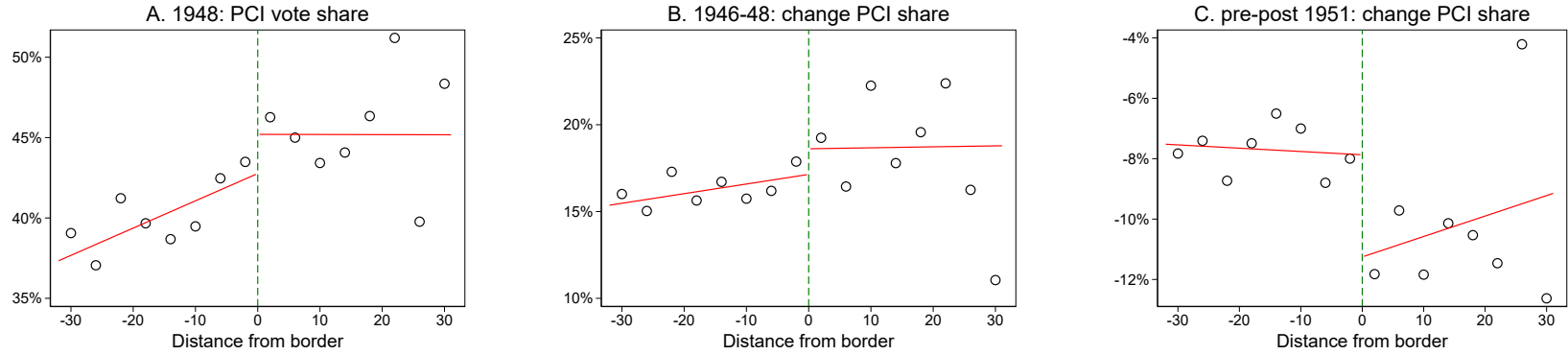
***Effect on other parties and turnout.*** In Appendix Figure D.10 we pool together all post-reform elections until 1992 and, using both our preferred specifications, we find that the reform did not have a significant effect on the vote share of other parties. This null effect is robust across different bandwidths. The only exception is a negative effect on the vote share of MSI (a very small neo-fascist party) when using the specification controlling for a quadratic polynomial in latitude and longitude. We conclude that around 85% of DC gains were at the expense of the Communist Party, while the remaining may have been at the expense of MSI. Additionally, in Appendix Figure D.11, we verify that the reform did not affect turnout.<sup>45</sup>

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PCI in 1948. After that year, PSI never received more than 15% of votes. When we look at the effect of the reform on PSI vote shares in 1946-92 we find no significant effect (see Appendix D.10).

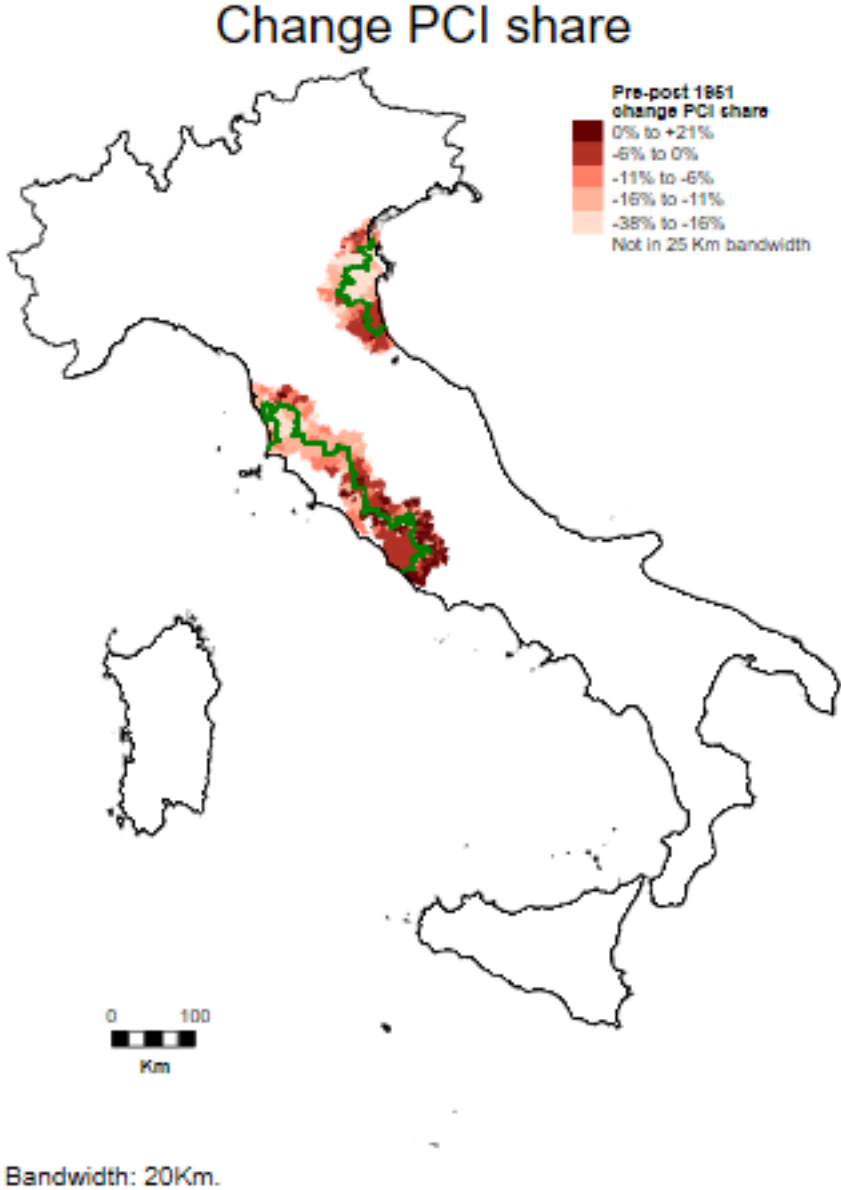
<sup>45</sup>1992 elections are an exception especially when using the specification controlling for distance from the border. However, the effect is very small given the high average turnout (95% in control towns across all years in the regression).

**Figure D.6:** Balance, pre-trends and effect of reform on PCI vote share: graphical evidence



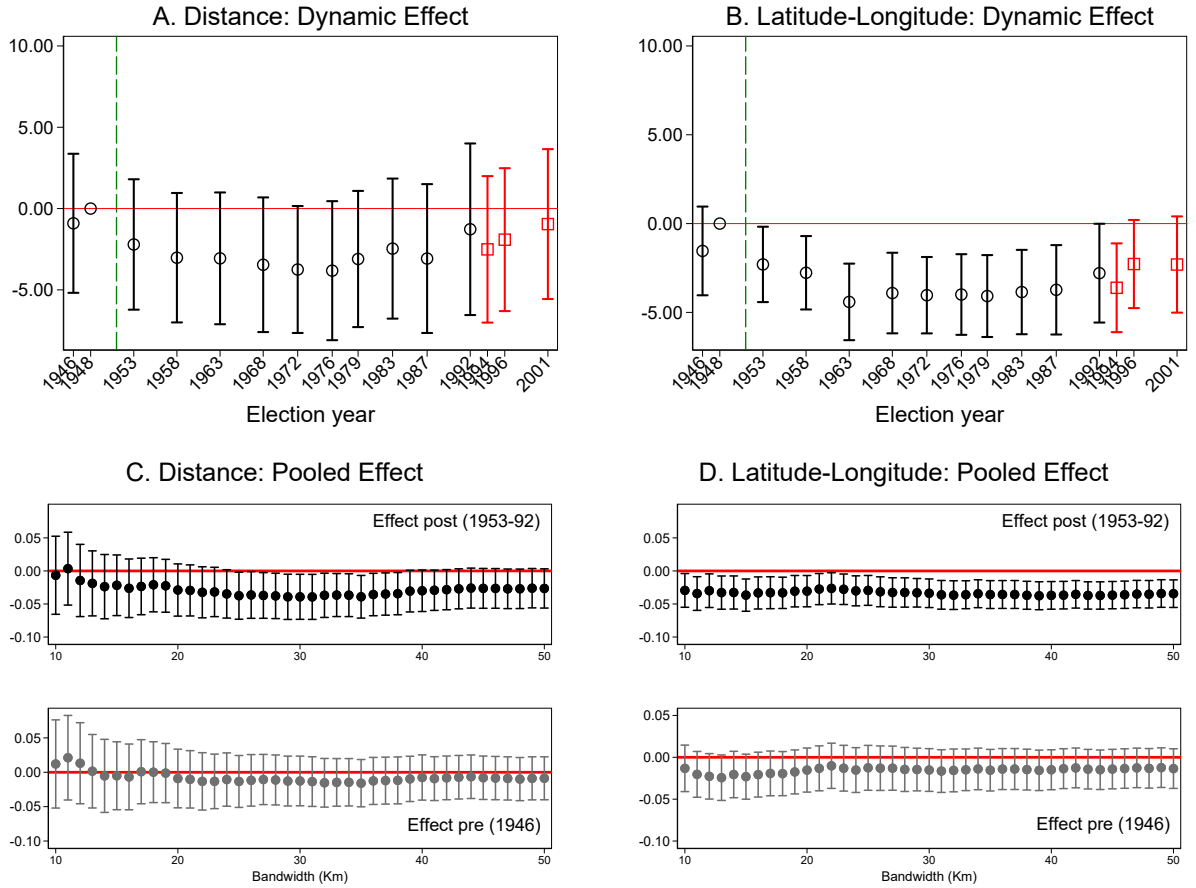
*Notes:* The Figure presents graphical evidence on the panel RDD. On the y-axes we plot electoral outcomes; on the x-axes the distance to the border. In each Panel, we bin data in 4 km intervals. Treated towns have positive distance and control towns have negative distance. The red lines report linear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

Figure D.7: Electoral effect of the reform: visualization of impact on a map



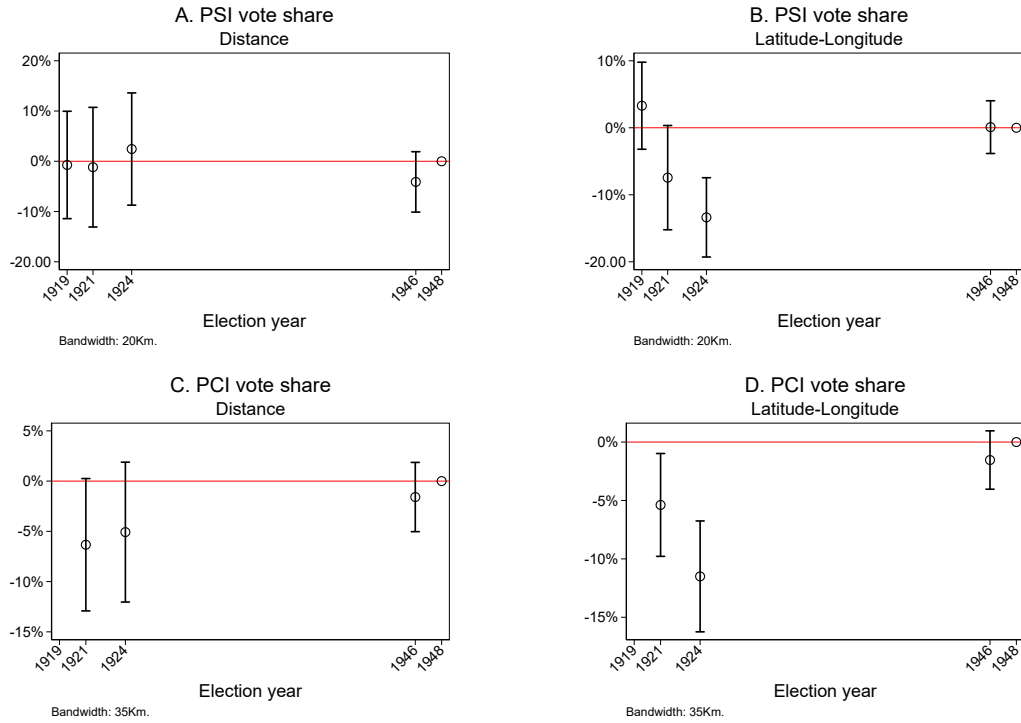
*Notes:* Change in PCI vote share between pre- (1946-48) and post-reform elections (1953-92) in the towns within 20 km from the reform borders of Delta Padano and Maremma.

**Figure D.8:** The electoral impact of the land reform – Communist Party (PCI)



*Notes:* Dependent variable is Communist Party (PCI) vote share. Panels A and B display coefficients  $\beta_t$  for each year from the panel RDD Equation (1). The omitted category is the  $\beta$  of 1948 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. The vertical dashed lines mark the 1951 land reform. 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). Panels C and D report pooled treatment effects for all electoral years between 1953 and 1992. We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. All regressions control for year  $\times$  reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

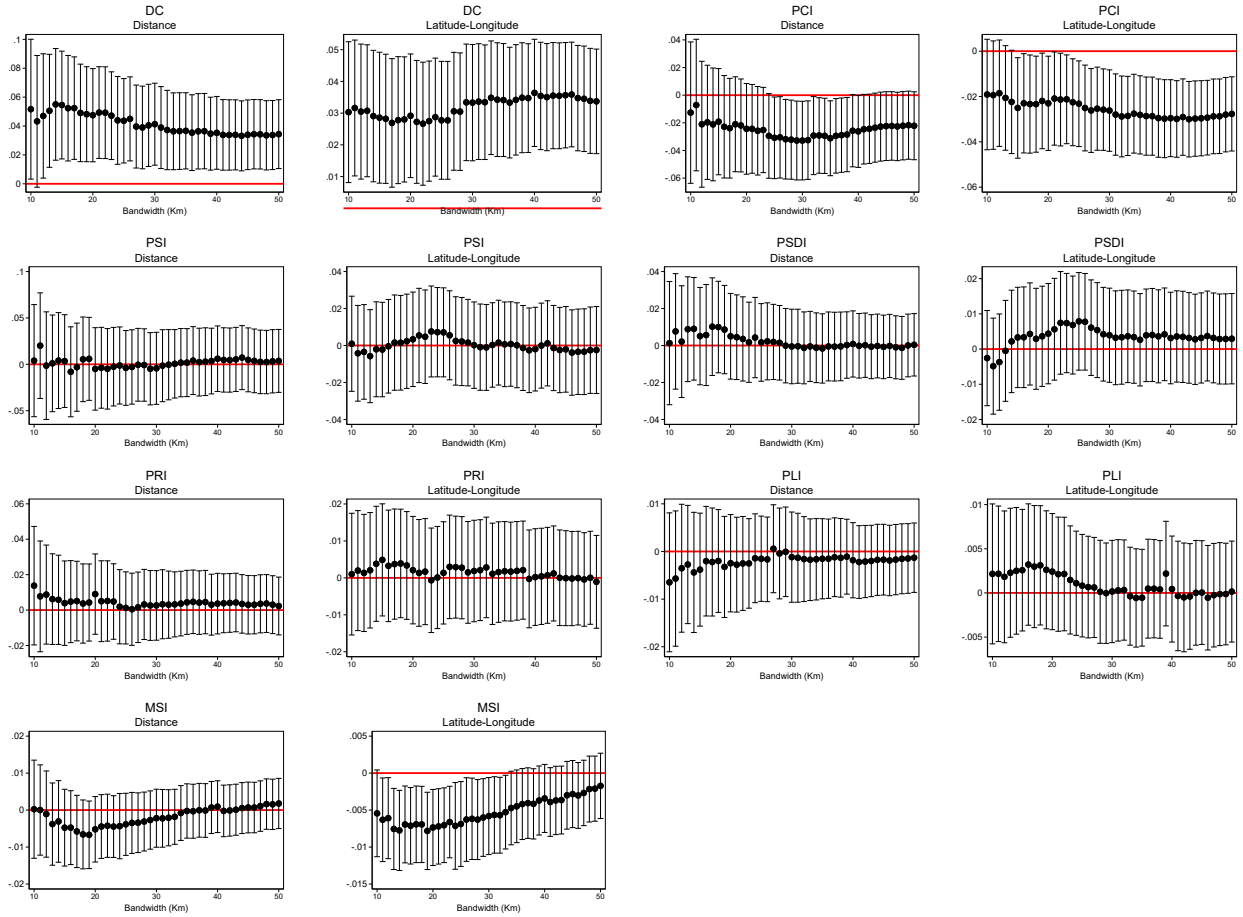
**Figure D.9:** 1919-1948 Pre-Trends PCI and PSI



*Notes:* Dependent variable is Socialist Party (PSI) vote share (Panel A and B) and Communist Party (PCI) vote share (Panel C and D). All panels display coefficients  $\beta_t$  for each year before the reform from the panel RDD Equation (1). The omitted category is the  $\beta$  of 1948. We include the 1992 election in the 1980s decade. The sample consists of all towns within 20 (Panels A and C) and 35 (Panels B and D) km from the reform borders of Delta Padano and Maremma. All regressions control for year  $\times$  reform area and town fixed effects. Regressions in Panels A and C control for a linear polynomial in distance, regressions in Panels B and D control for a quadratic polynomial in latitude and longitude. We use the vote share of the Popular Democratic Front (FDP) in 1948 for both PCI and PSI. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

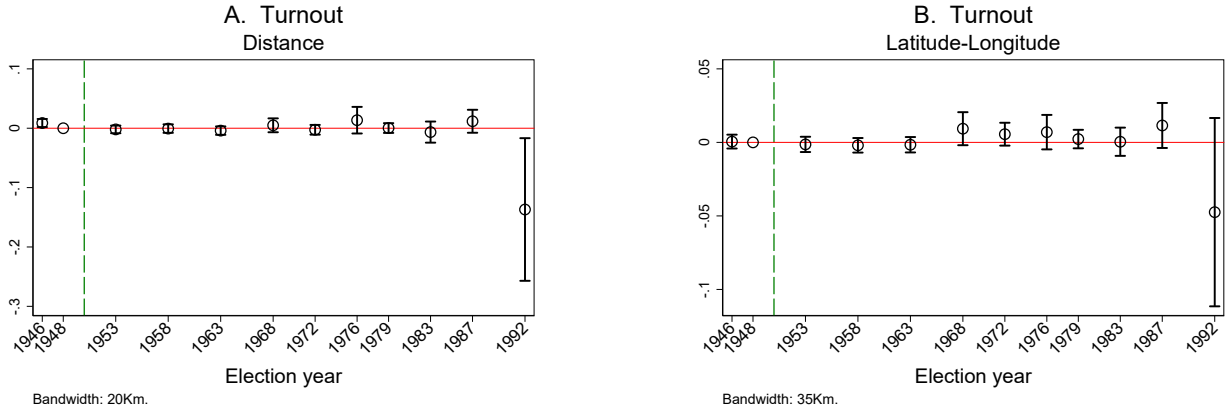


Figure D.10: The impact of the land reform on all major Italian parties



Notes: The graphs report the pooled treatment effects for all electoral years between 1953 and 1992 ((1)). We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. The dependent variables are the vote shares of the main Italian political parties in this period. The subtitle *Distance* indicates the RDD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RDD regressions controlling for a quadratic polynomial in latitude and longitude. All regressions control for year  $\times$  reform area and town fixed effects. Electoral data are from Corbetta and Piretti (2009). Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

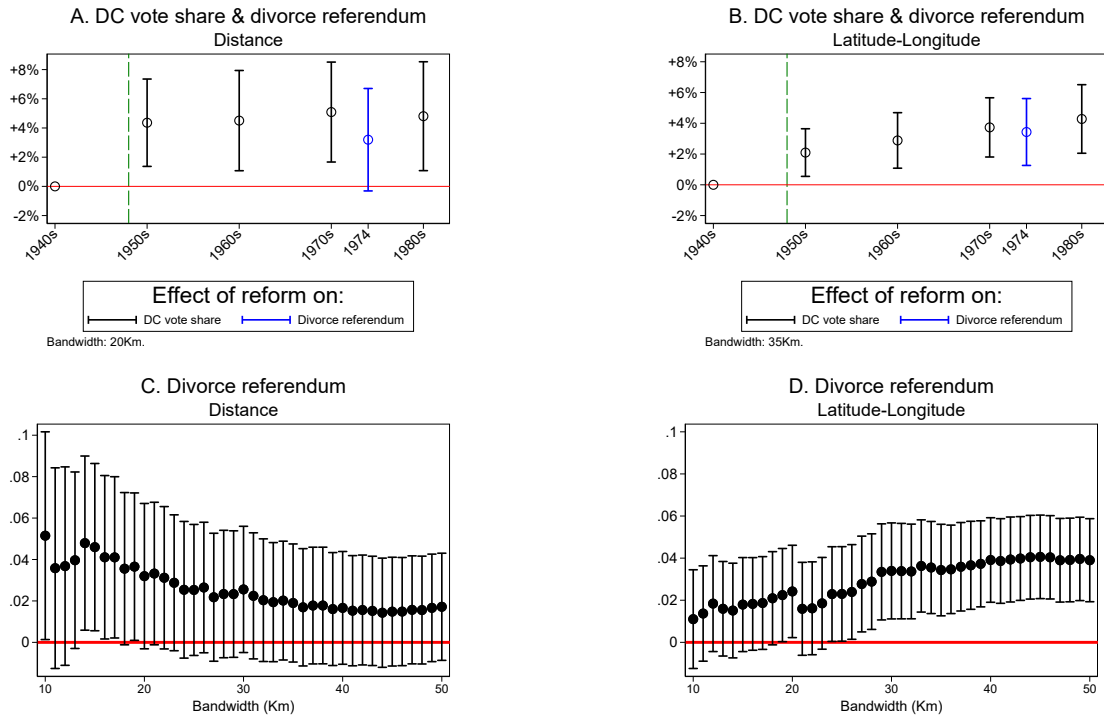
**Figure D.11: Turnout: Panel RDD Coefficients**



*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. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. Units of observation are town-years. The sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

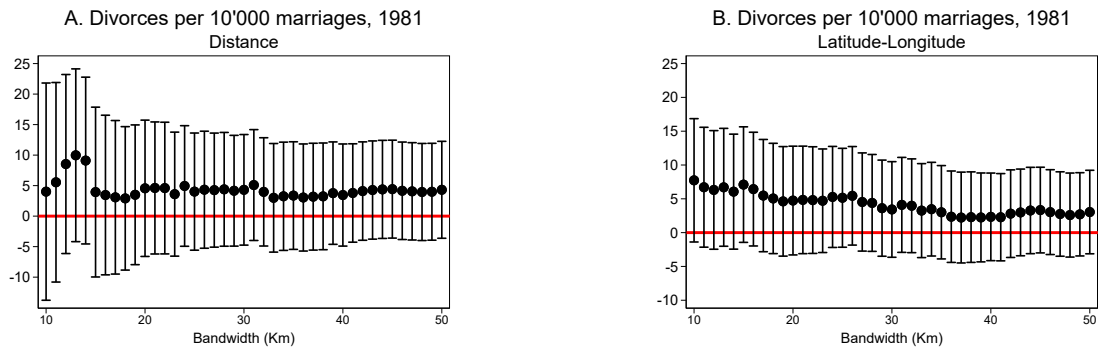
## D.5 Divorce Referendum

Figure D.12: 1974 Referendum on the Repeal of Divorce



Notes: Panel A and B display 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. The sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. Panel C and D report treatment effects for the 1974 divorce referendum. We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. Panel A and C control for a linear polynomial in distance. Panel B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

Figure D.13: Divorces per 10'000 marriages 1981



Notes: The graphs reports coefficients  $\beta_t$  of the RDD Equation (2). We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. All regressions control for reform area fixed effects. Dependent variable is the number of divorces per 10'000 marriages. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.

## D.6 Gratitude<sub>h</sub>

**Table D.1:** Gratitude: the impact of the reform by share of old people in 1951.

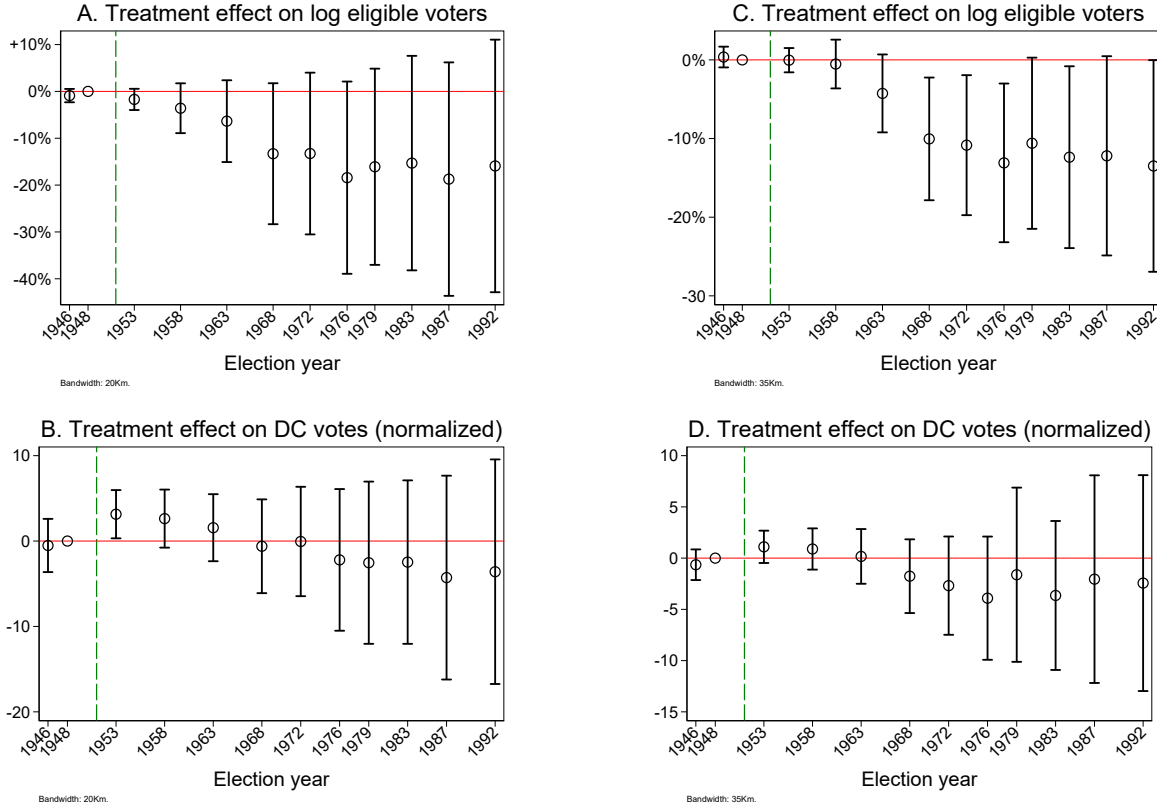
	DC vote share	
	(1)	(2)
Treatment × 1950s	0.031*** [0.011]	0.036*** [0.010]
Treatment × 1960s	0.031** [0.012]	0.040*** [0.011]
Treatment × 1970s	0.052*** [0.013]	0.060*** [0.012]
Treatment × 1980s	0.078*** [0.015]	0.090*** [0.013]
Old population in 1951 × Treatment × 1950s	0.005 [0.015]	-0.001 [0.013]
Old population in 1951 × Treatment × 1960s	0.001 [0.017]	-0.008 [0.015]
Old population in 1951 × Treatment × 1970s	-0.013 [0.018]	-0.024 [0.017]
Old population in 1951 × Treatment × 1980s	-0.040* [0.021]	-0.053*** [0.019]
Old population in 1951 × 1950s	0.011 [0.008]	0.010* [0.006]
Old population in 1951 × 1960s	0.009 [0.009]	0.009 [0.007]
Old population in 1951 × 1970s	0.028*** [0.010]	0.027*** [0.008]
Old population in 1951 × 1980s	0.042*** [0.010]	0.043*** [0.009]
Mean Y Control	0.36	0.37
Number of Towns	406	647
Bandwidth	20 Km	35 Km
Specification	DiD	DiD

Observations	4845	7696
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*Notes:* The Table reports coefficients from a difference-in-difference specification with heterogeneity in old population at the time of the reform. We measure old population with the share of people above 65 years old in the 1951 population census, and create a binary indicator by splitting the sample at the median. The regressions control for year  $\times$  reform area and town fixed effects. Dependent variable is Christian Democrat (DC) vote share. Units of observation are town-years. Column 1: the sample consists of all towns within 20 km to the reform borders of Delta Padano and Maremma. Column 2: the sample consists of all towns within 35 km to the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

## D.7 Migration

**Figure D.14:** Impact of reform on the number of eligible voters and DC votes



*Notes:* All 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 the election of 1948. Panels A and C: the dependent variable is the log of eligible voters. Panels B and D: the dependent variable is DC voters in year  $t$  normalized by eligible voters in 1948. Electoral data are from Corbetta and Piretti (2009). The vertical lines mark the 1951 land reform. The sample consists of all towns within 20 (Panels A and B) and 35 (Panel C and D) km from the reform borders of Delta Padano and Maremma. Panel A and B control for a linear polynomial in distance. Panel C and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

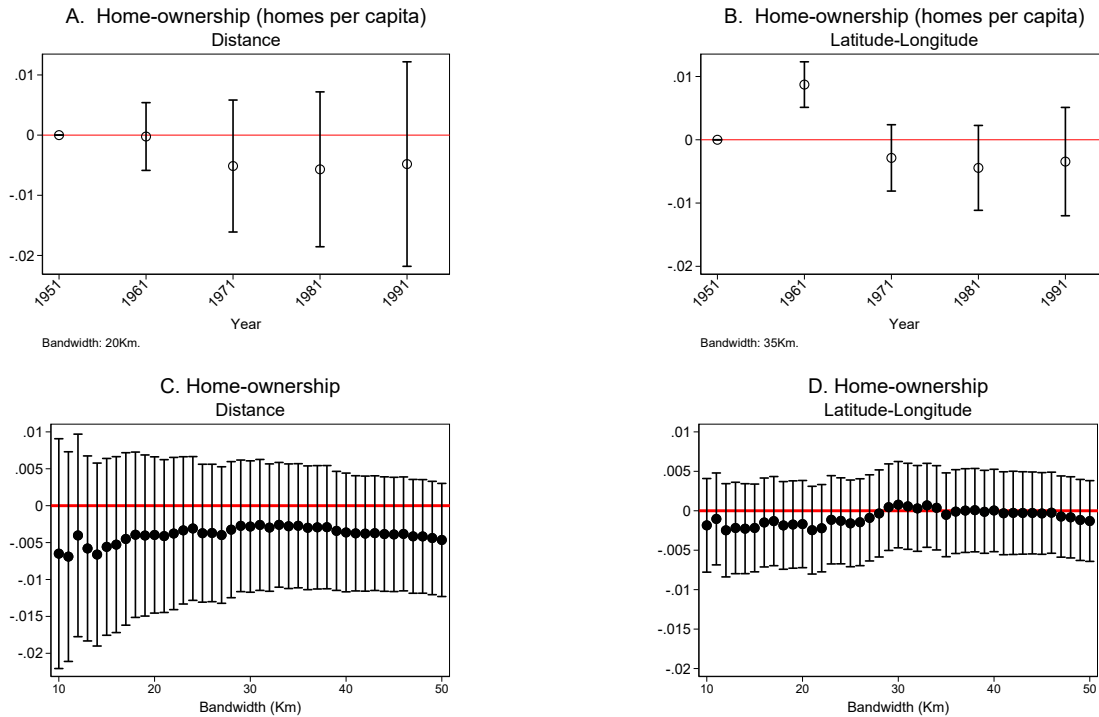
**Table D.2:** Impact of reform in the north on population structure.

	Distance					Latitude-Longitude				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share population aged				Males (%)	Share population aged				Males (%)
	0-20	21-45	46-65	>65		0-20	21-45	46-65	>65	
Treatment $\times$ 1961	-0.002	-0.004	0.002	0.004	-0.002	-0.006**	-0.001	0.004	0.003**	-0.001
	[0.005]	[0.004]	[0.004]	[0.003]	[0.002]	[0.003]	[0.003]	[0.002]	[0.002]	[0.001]
Treatment $\times$ 1971	-0.001	-0.001	0.004	0.005	-0.002	-0.012***	0.003	0.006	0.006*	-0.002
	[0.007]	[0.005]	[0.007]	[0.006]	[0.002]	[0.005]	[0.003]	[0.004]	[0.003]	[0.002]
Treatment $\times$ 1981	-0.007	-0.006	0.002	0.011	-0.003	-0.018***	-0.005	0.009**	0.013***	-0.003*
	[0.009]	[0.009]	[0.006]	[0.010]	[0.003]	[0.006]	[0.004]	[0.004]	[0.005]	[0.002]
Treatment $\times$ 1991	-0.003	-0.006	-0.001	0.012	-0.004	-0.019***	-0.006	0.010**	0.017***	-0.004*
	[0.007]	[0.010]	[0.006]	[0.011]	[0.003]	[0.005]	[0.005]	[0.004]	[0.006]	[0.002]
Mean Y Control Group	0.26	0.33	0.22	0.13	0.50	0.27	0.33	0.22	0.13	0.50
Number of Towns	406	406	406	406	406	647	647	647	647	647
Observations	2030	2030	2030	2030	2030	3235	3235	3235	3235	3235

*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year  $\times$  reform area and town fixed effects. Dependent variables are: share of people within specified age groups (Columns 1 to 4 and 6 to 9) and share of males in the population (Columns 5 and 10). Columns 1 to 5 control for a linear polynomial in distance. Columns 6 to 10 control for a quadratic polynomial in latitude and longitude. The omitted category is the  $\beta$  of 1951. Source is the decadal population censuses. Units of observation are town-years. The sample consists of all towns within 20 (columns 1 to 5) and 35 (columns 6 to 10) 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.

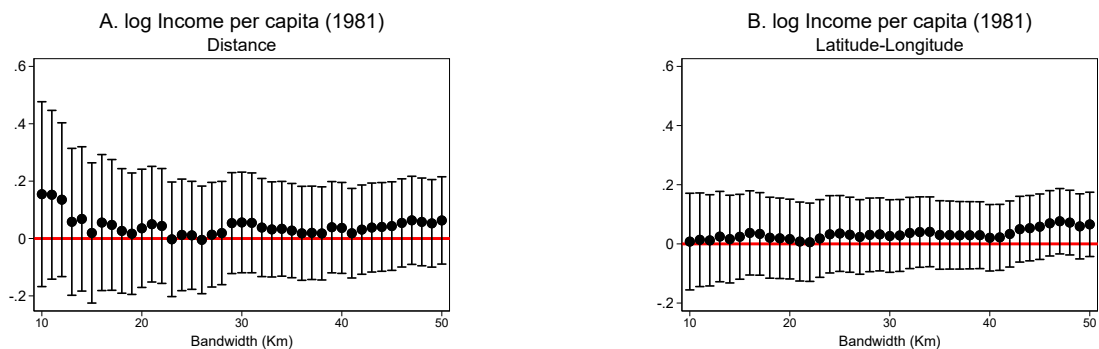
## D.8 Economic Growth and Development Patterns

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



*Notes:* Panel A and B display 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 census years. The omitted category is the  $\beta$  of the elections of 1951. The dependent variable is the number of households owning a house per capita. The sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. Panel C and D report pooled treatment effects for all census years between 1961 and 1991. We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. Panel A and C control for a linear polynomial in distance. Panel B and D control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. We estimate standard errors clustered at the town level and plot 95% confidence intervals as bars around the coefficients.

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



*Notes:* The graphs reports coefficients  $\beta_t$  of the RDD Equation (2). We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. All regressions control for reform area fixed effects. Dependent variable is the log income per capita in 1981. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.



**Table D.3:** Impact of reform in the north on active population and sectoral shares.

	Distance						Latitude-Longitude					
	(1) Active pop.	(2) Active men	(3) Active women	(4) Agriculture	(5) Manufacturing	(6) Services	(7) Active pop.	(8) Active men	(9) Active women	(10) Agriculture	(11) Manufacturing	(12) Services
Treatment × 1936	0.017 [0.017]			0.022 [0.018]	-0.010 [0.011]	-0.412 [0.435]	-0.022** [0.010]			0.007 [0.014]	-0.012 [0.009]	-0.767 [0.586]
Treatment × 1961	-0.020 [0.017]	-0.005 [0.013]	-0.035 [0.031]	-0.026 [0.021]	0.008 [0.010]	-0.008 [0.005]	-0.049*** [0.010]	-0.024*** [0.008]	-0.073*** [0.017]	-0.018 [0.012]	-0.017** [0.007]	-0.003 [0.004]
Treatment × 1971	-0.015 [0.018]	-0.008 [0.017]	-0.022 [0.031]	-0.027 [0.029]	0.003 [0.018]	0.015 [0.010]	-0.043*** [0.011]	-0.023** [0.011]	-0.059*** [0.019]	-0.031* [0.016]	-0.018 [0.011]	0.008 [0.007]
Treatment × 1981	-0.005 [0.020]	-0.012 [0.017]	0.005 [0.033]	-0.024 [0.032]	-0.004 [0.021]	0.012 [0.014]	-0.042*** [0.012]	-0.020* [0.011]	-0.058*** [0.019]	-0.019 [0.019]	-0.016 [0.014]	-0.007 [0.009]
Treatment × 1991	0.003 [0.021]	-0.016 [0.014]	0.024 [0.036]	-0.000 [0.036]	-0.020 [0.022]	0.029** [0.012]	-0.044*** [0.013]	-0.025** [0.013]	-0.059*** [0.021]	-0.013 [0.020]	-0.004 [0.015]	-0.007 [0.008]
Mean Y Control Group	0.56	0.85	0.31	0.42	0.20	0.22	0.56	0.85	0.32	0.40	0.23	0.20
Number of Towns	406	406	406	406	406	406	647	647	647	647	647	647
Observations	2435	2029	2029	2435	2435	2435	3881	3234	3234	3881	3881	3881

*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year × reform area and town fixed effects. Dependent variables are: share of people in the labor force (Columns 1 and 7), share of men in the labor force (2 and 8), share of women in the labor force (3 and 9), share of workers employed in agriculture (4 and 10), manufacturing (5 and 11) and services (6 and 12). Columns 1 to 6 control for a linear polynomial in distance. Columns 7 to 12 control for a quadratic polynomial in latitude and longitude. The omitted category is the  $\beta$  of 1951. Source is the decadal population censuses. Units of observation are town-years. The sample consists of all towns within 20 (columns 1 to 6) and 35 (columns 7 to 12) 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.9 Firms' growth

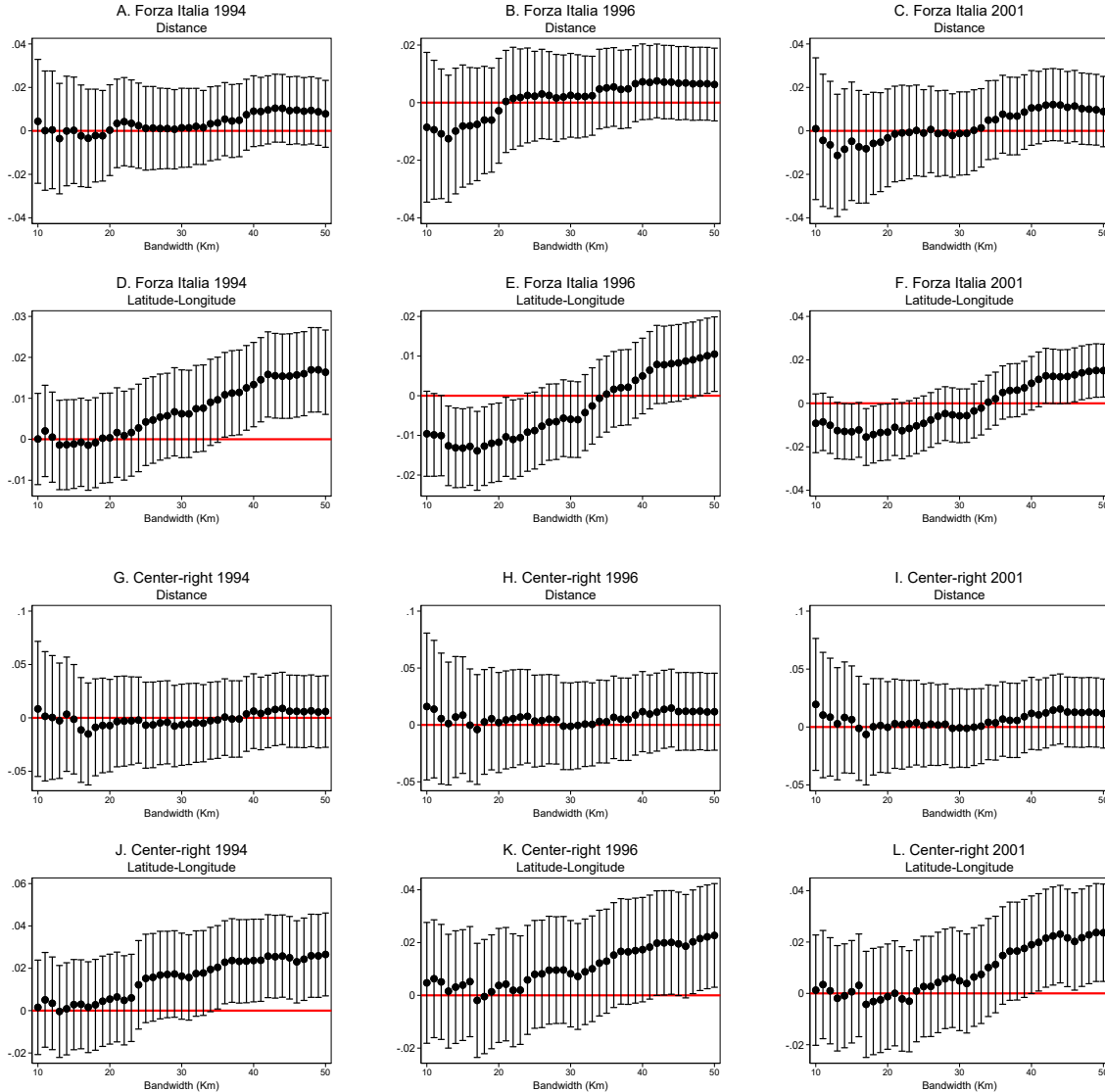
**Table D.4:** Impact of reform in the north on the number of workers per firm plant.

	Distance						Latitude-Longitude					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Plants per capita			Plant size			Plants per capita			Plant size		
	< 20km	< 10km	< 50km	< 20km	< 10km	< 50km	< 35km	< 10km	< 50km	< 35km	< 10km	< 50km
Treatment × 1961	-0.001 [0.004]	-0.000 [0.005]	-0.003 [0.003]	0.317 [0.267]	0.740* [0.419]	0.069 [0.198]	-0.002 [0.003]	-0.002 [0.003]	-0.003 [0.003]	-0.068 [0.173]	0.193 [0.232]	-0.110 [0.149]
Treatment × 1971	-0.000 [0.004]	0.002 [0.006]	-0.004 [0.004]	-0.056 [0.320]	0.061 [0.423]	-0.276 [0.223]	-0.001 [0.003]	-0.001 [0.003]	-0.002 [0.003]	-0.616** [0.242]	-0.303 [0.277]	-0.689*** [0.202]
Treatment × 1981	0.001 [0.005]	0.003 [0.007]	-0.002 [0.004]	-0.112 [0.368]	0.177 [0.506]	-0.382 [0.262]	-0.001 [0.004]	-0.003 [0.003]	-0.003 [0.004]	-0.451* [0.244]	-0.296 [0.309]	-0.407* [0.216]
Treatment × 1991	-0.001 [0.005]	-0.004 [0.007]	-0.004 [0.004]	-0.276 [0.352]	0.096 [0.485]	-0.481* [0.270]	-0.005 [0.004]	-0.006* [0.003]	-0.007* [0.004]	-0.504** [0.227]	-0.456* [0.272]	-0.472** [0.204]
Mean Y Control Group	0.04	0.04	0.05	2.86	2.79	3.14	0.04	0.04	0.05	3.06	2.79	3.14
Number of Towns	406	222	903	406	222	903	647	222	903	647	222	903
Observations	2024	1107	4508	2024	1107	4508	3228	1107	4508	3228	1107	4508

*Notes:* The Table reports coefficients  $\beta_t$  from the panel RDD Equation (1), which controls for year × reform area and town fixed effects. Because sectoral definitions vary somewhat across years, we ensure consistency by considering all firms in the following sectors: manufacturing, extraction, construction, commerce, transport, utilities, social services, banking and insurance. Columns 1-3 and 7-9 dependent variable is the number of plants per capita. Columns 4-6 and 10-12: dependent variable is the number of workers per plant. The omitted category is the  $\beta$  of 1951. Columns 1-6 control for a linear polynomial in distance. Columns 7-12 control for a quadratic polynomial in latitude and longitude. Source are the decadal economic and population censuses. Units of observation are town-years. The samples consists of all towns within 20 km (Column 1 and 4) 35 km (7 and 10), 10 km (Column 2, 5, 8, 11) and 50 km (Column 3, 6, 9, 12) to the reform borders of Delta Padano and Maremma. Standard errors clustered at the town level in parentheses. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

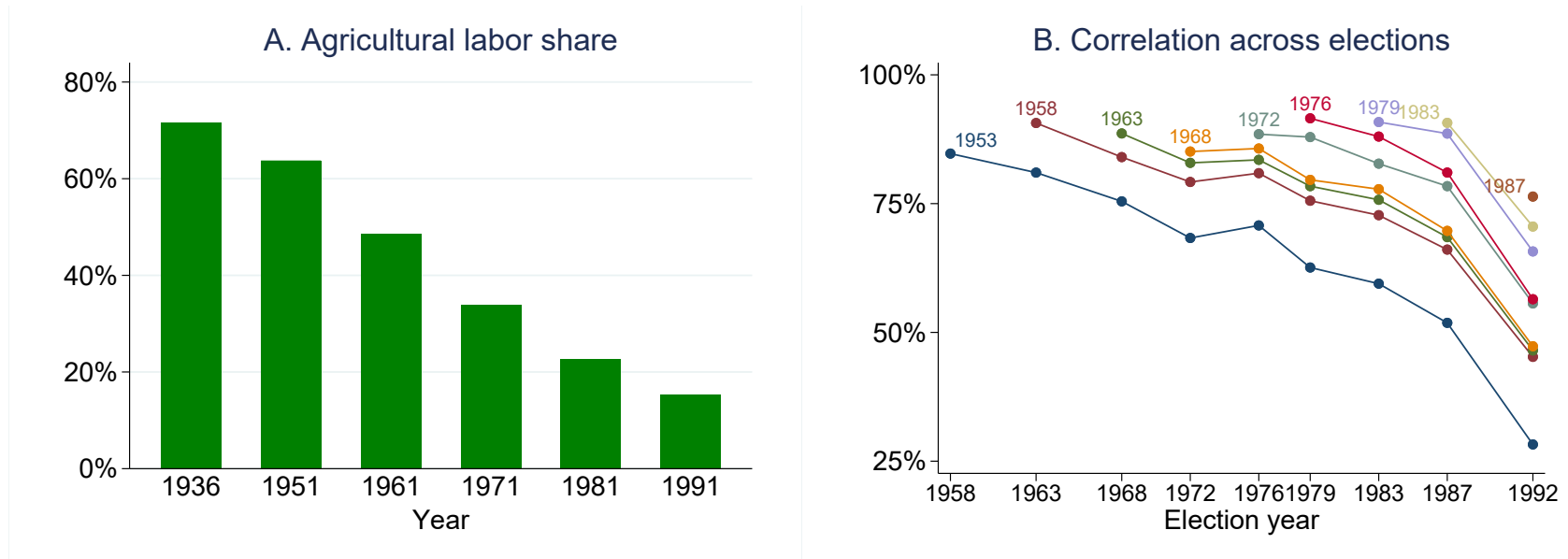
## D.10 Economic Conservatism and Changing society

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



*Notes:* The graphs reports coefficients  $\beta_t$  of the RDD Equation (2). We report separate estimates for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. All regressions control for reform area fixed effects. Dependent variables are: the vote share of Forza Italia (Berlusconi's party) in 1994 (Panel A and D), 1996 (Panel B and E) and 2001 (Panel C and F) and the vote share of major right-wing parties in 1994 (Panel G and J), 1996 (Panel H and K) and 2001 (Panel I and L). Panels A, B, C, G, H, and I control for a linear polynomial in distance. Panels D, E, F, J, K, and L control for a quadratic polynomial in latitude and longitude. Results controlling for Christian Democrat vote share in 1948 are very similar and available upon request. We include among the major post-1992 right-wing parties Forza Italia, Alleanza Nazionale, Lega Nord (all years) and Christian Democratic Center and the Christian Democratic Union (in 2001). Electoral data are from Corbetta and Piretti (2009). Units of observation are towns. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.

**Figure D.18:** 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.