

Harvesting Votes: The Electoral Effects of the Italian Land Reform ^{*}

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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. In a sample of towns close to the reform border, difference-in-differences estimates show that the reform generated large electoral gains for the incumbent Christian Democratic party. The electoral effects persist over four decades. We show that the reform initiated a sustained exchange between voters and politicians, rooted in clientelistic brokering and political patronage, which helped the Christian Democrats maintain the initial gains. We also examine alternative explanations – such as economic development, structural transformation, and shifts in political attitudes – but find little consistent evidence to support these channels.

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

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1 Introduction

The political objective of many large-scale redistribution policies is to establish political support (Acemoglu, 2001). In agrarian societies, land reforms – which entail the expropriation and redistribution of large land estates – represent a primary example of such policies. 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 happened in several European countries after World War II. Many Asian and Latin American countries also implemented reforms after the Chinese and the Cuban revolutions, respectively (Binswanger et al., 1995). Despite the importance of such reforms, the scarcity of natural experiments and the challenges in implementing credible research designs have constrained our understanding of the magnitude, persistence, and underlying mechanisms of their political effects.

In this paper, 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. The land reform only targeted well-defined reform areas. Therefore, we can study differential changes in voting outcomes between treated and control towns close to the reform areas’ borders, both right after the reform and over a long time horizon.¹ The focus on this *extensive margin* (i.e., comparing neighboring towns targeted or not targeted by the reform), represents a significant departure from previous literature, which has leveraged variation in the *intensive margin* of land reforms (i.e. variation across locations in the amount of redistributed land), and typically only for a few years, as we discuss in detail below.

We find that DC experienced immediate electoral gains. In the first election after the reform, treated towns increased support for DC by two to four percentage points (control mean: 37-38%). Christian Democrats’ gains are mirrored by the electoral losses of the Communist Party (*PCI*). Immediate electoral gains are consistent with journalistic reports and descriptive evidence from the time (Russo, 1955; Toldo, 1957). The results are robust to several checks, including alternative bandwidths and specifications, analysis of spillovers on voting in control towns, and inference approaches. The *persuasion rate* – the share of beneficiaries who start voting DC among beneficiaries who were not already DC supporters – is 60-80%. These effects are large if compared to the impact of pork-barrel

¹We focus on target reform areas in the center-north, as historical and statistical evidence indicates that the key assumption for our analysis (parallel trends at the border) likely holds there. Section 2 elaborates.

spending (Levitt and Snyder Jr, 1997) or other political interventions (DellaVigna and Gentzkow, 2010).

DC maintained an electoral advantage in reform towns 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. We explore several potential mechanisms behind this persistence. First, it is plausible that land redistribution induced gratitude among 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 over time many beneficiaries died, and memory of the reform faded. Consistent with this, we show that where population was older at the time of redistribution, DC gains diminished 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).

Next, we consider the hypothesis that the reform spurred a continued exchange between politicians and voters over time. This idea is motivated by the observation that, as is common in land reforms around the world, the Italian land reform had elements of discretionality and conditionality. These included favoritism in land allocation, limitations in transferability of the received land, and dependency on local brokers which controlled access to fundamental services like agricultural inputs, credit, and healthcare – including the *Enti di riforma* (Reform Boards), beneficiaries’ cooperatives and the farmers’ association *Coldiretti* (cf. Stokes et al., 2013; Larreguy et al., 2016 and Allum, 1974 for Italy).

We, therefore, examine the effects of the reform on political brokering and on targeted benefits through public sector employment, two important components of DC clientelistic system (see e.g. Hicken, 2011; Piattoni, 2001; Allum, 1974; Alesina et al., 2001).² 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. As these data do not exist before the reform, we analyze them with a spatial regression discontinuity (RD) that is directly comparable to the main difference-in-differences specification (same sample, same controls). RD estimates

²Targeted 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).

indicate 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. Analysis of public sector employment data also shows that treated towns employed a greater share of workers in the public sector. These effects are particularly large in the 1980s, when DC patronage became rampant (Allum, 1997).

These results suggest that the large-scale land reform induced a continued exchange between politicians and voters: in the first decades after the reform, DC controlled redistribution and the provision of additional services through the *Enti di riforma*. During the 1960s and 1970s, *Coldiretti* took root in reform towns, helping DC maintain consensus. In the 1980s and early 1990s, as the importance of both land reform and agriculture waned, DC adapted its methods and started providing public jobs. The long-term electoral advantages of DC, thus, compound the direct effects of the land reform with the reinforcing effect of the continued exchange. The crucial identification assumption is that the land reform induced the continued exchange. For example, it must be the reform that increased *Coldiretti*'s strength, not other unrelated factors. To the best of our knowledge, evidence that large-scale reforms can spur continued political exchange is novel in the literature.

We examined two potential additional drivers of the persistence of voting effects. First, the reform may also affect long-term electoral outcomes through its impact on economic development. We find no consistent evidence that land reform affected income, wealth, structural change, or firms' dynamics. Nonetheless, 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: *i)* The changes in population and age-groups shares are small in the first few years after the reform and grew over time; in contrast, DC support increased sharply in the first election after the reform and remained remarkably stable over four decades. *ii)* 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.

Second, 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 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 1971 and 1991. 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. As is often the case in analysis of long-term impacts (e.g. Dell, 2010), other channels, unobservable to us due to data limitations, may have also played a role. For example, while our results suggest a limited role for long-term gratitude and reciprocity *among beneficiaries*, we can not rule out the possibility that beneficiaries transmitted their positive attitudes towards DC to the next generation.

Credibly identifying the electoral impact of redistribution policies is challenging. Existing empirical studies mostly look 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 policies, several studies from Mexico (see, e.g., De Janvry et al., 2014, Larreguy et al., 2018) have looked at the gradual rollout of *land titling* programs – which formalize existing land claims without expropriating and redistributing land. González (2013) and Albertus and Schouela (2024) focus on land reform programs, respectively in Chile and Portugal, leveraging determinants of the intensity of the redistribution. We depart from these studies by exploiting sharp identification based on reform area borders and by looking at voting outcomes over several decades.³

The relationship between land redistribution policies and political exchange also contributes to the literature on clientelism. Scholars 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, the Italian land reform appears

³Several authors look at the political economy of land reforms (e.g., Bardhan and Mookherjee, 2010, Fergusson et al., 2022, Bhattacharya et al., 2019 and Bazzi et al., 2020). Dell (2012) uses an IV based on rainfall to study the impact of the Mexican revolution insurgency and subsequent land reform on long-run development. Several public policies contain elements of redistribution that may affect voting. See for example Acemoglu et al. (2021) analysis of the long-term electoral effects of redistribution policies, focusing on educational reforms in Norway.

to have strengthened clientelistic brokers, arguably because it facilitated new forms of control over beneficiaries. 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).⁴ 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 landless rural workers (Medici, 1948, cited by Gullo, 1950). This situation created expectations for major reforms. 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 to prevent a Communist coalition between rural and urban workers (Segni, 1952; Bernardi, 2006).⁵

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 that 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 1950.⁶ The *Legge Stralcio* targeted only the highest-priority regions in the original proposal⁷ and effectively delegated the exact definition of the reform borders to

⁴Important 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.

⁵Acemoglu and Robinson (2000) develops a model where *élites* extend the franchise to avoid a revolution.

⁶The *Legge Stralcio* was enacted as law 249 of 28th October 1950. A third bill regulated land redistribution on a small area covering the Sila plateau in the southern region of Calabria. The *Legge Sila* was enacted on 12th May 1950, and it was later superseded by the *Legge Stralcio*.

⁷Roughly “Zone B” of the *Legge di Riforma agraria*: see Figure A.1 (Salomone, 1950).

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.⁸

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 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 Agraria*: it excluded 40% of the 1.6 million hectares originally included in the proposal and included a new group of towns not contiguous to the rest of the reform (Ruini, 1951; Ministry of Agriculture’s Spokesman, 1951). The political motives of land redistribution in the South transpire in the letters sent to the then Ministry of Agriculture (e.g. Caglioti, 1950 informs the minister on how a town turned DC after the start of the redistribution).

By contrast, in Delta Padano and Maremma, political manipulation appears less important. Private and confidential records of the then Ministry of Agriculture suggest limited interest in adjusting borders for political gain, citing technical and financial concerns that did not feature in the discussions over southern borders. The same records also reveal the frustration of DC politicians for the *absence* of political manipulation in the North.⁹ 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

⁸The areas were: Delta Padano (north-east), Maremma (center-west), Fúicino (center), two separate areas in Campania (south-west, both managed by the *Opera Nazionale dei Combattenti*), a broad area that straddled across Molise, Puglia and Lucania (south-east), Sila (south) 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.

⁹Appendix Figure A.2 shows documents in which DC politicians complained to the Ministry about 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). Source: Mura (2017).

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 in the North.

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, specified in a table (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 redistributed it shortly thereafter: 84% of the land was assigned in 1952-56 and redistribution was essentially complete by December 1962 (Pezzino, 1972; see also law 901 of 1965).¹⁰

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, though a successive law in 1967 allowed sales after six years (King, 1973). For their part, expropriated landlords were not allowed to purchase land for six 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

¹⁰The one exception was land reclaimed in the late 1950s around the Comacchio Valley: this land was distributed to 340 families until 1967 (King, 1973). These families represent no more than 1.4% of beneficiaries in the study area. The main results are unaffected by the exclusion of the five towns in the Comacchio Valley.

eligibility criteria, applications exceeded available land, and agencies had discretion in selecting among many qualified applicants (Baldocchi, 1978; Prinzi, 1956; Capobianco, 1992; Marciani, 1966). Politics influenced these 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). This strategic land allocation may thus have created the pre-conditions to start a continued political exchange between DC and land reform beneficiaries. 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 lire (about 5 percent of 1951 Italy's GDP). Maremma and Delta received about 15% of these funds (Marciani, 1966; Barbero, 1960). The intervention represented a redistribution in favor of poor farmers and our empirical analysis estimates 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. Long tenures in office are not uncommon in democracies (e.g., Sweden's Social Democrats in 1932-1976, Japan's Liberal Democrats in 1955-1993, the Indian National Congress in 1947-1977, and Mexico's Partido Revolucionario Institucional in 1934-2000). In the Italian case, 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.

Reform Boards. The continued action of the *Enti di Riforma* may have helped perpetuate the political effects of the reform. After redistribution, the Reform Boards remained active in reform areas, providing training, improving land, and building irrigation canals, electricity networks, and warehouses. Because Boards' heads were appointed by DC governments, also these activities had sometimes political aims: Pezzino (1972) notes that "sometimes *Enti di riforma* turned into branch offices of the majority party

[DC].” In the first decade, Reform Boards staffed cooperatives of land beneficiaries with DC presidents and directors, exploiting the provision that land beneficiaries had to join a cooperative (King, 1973). After 1965 (law 901), the Reform Boards became responsible for agricultural development beyond reform areas and with law 386 of 1976, regional agricultural development bodies took up the functions (and the personnel) of the *Enti di Riforma*. Thus, their activity may help explain DC’s success at least until the mid-1960s, but possibly later if the Boards’ investment and hiring decisions kept prioritizing reform areas.

Political Brokers. Grassroots 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.

Two institutions provided *Coldiretti* with formidable tools to influence farmers: *Federconsorzi* and *Casse Mutue*. *Federconsorzi* coordinated a network of agricultural cooperatives providing services to farmers. It had the monopoly of crop stockpiles, and even farmers not directly linked to it had to interact with it. *Coldiretti* took over *Federconsorzi* in 1949, when *Coldiretti*’s founder Bonomi was elected president (Bernardi, 2020). *Federconsorzi* used monopoly profits and public funds to subsidize some farmers and agricultural cooperatives in ways that a Parliamentary commission found illegal (e.g. double payments or granting credit that was never repaid: Cirami, 2001, p.56, 69). Some of the resources *Federconsorzi* and *Coldiretti* managed on behalf of the Italian government were offered to farmers on the condition that they joined *Coldiretti*. For instance, when US food aid was shipped to Italy in 1956 through *Federconsorzi*, only *Coldiretti* members received it (Bernardi, 2020, p.146, 160). Similarly, during the 1968 drought, emergency water sent by the central government was delivered only to those who signed up to *Coldiretti* (L’Unità, 1968b). Thus, through *Federconsorzi* and its connections to DC governments, *Coldiretti* was able to administer benefits that, in contrast with the land redistributed in 1951, were easy to withdraw.

A second important tool in the hands of *Coldiretti* were the *Casse Mutue*, a system of more than eight thousand local healthcare providers serving farmers. *Casse Mutue* were introduced by DC in 1954: they provided state-subsidized healthcare to farmers and were financed by public funds and members’ contributions. Together with the right to receive healthcare, *Casse Mutue* members could also elect their boards. Communists repeatedly accused *Coldiretti* of rigging these elections, with tactics ranging from nullifying votes of non-*Coldiretti* farmers to faking or extorting proxy vote certificates (L’Unità, 1964b,a, 1967, 1968a). *Coldiretti* dominated *Casse Mutue*: 90% of *Casse Mutue* board seats were *Coldiretti* and the organization effectively controlled over 95% of the boards (Luzzi, 2004;

Lanza, 1991). Control over these healthcare providers gave real power: for instance, in 1964 a small landowner was reportedly denied healthcare as she refused to join *Coldiretti* (L’Unità, 1964c). To sum up, the control of healthcare provision gave *Coldiretti* another targeted, reversible benefit they could use to influence farmers. In Section 6.2 we examine *Casse Mutue*’s elections and budgets to gauge the local strength of *Coldiretti* in reform areas.

Coldiretti influence was particularly strong among land reform beneficiaries. Reform Boards’ managers were often *Coldiretti* leaders, and they often used their influence among land beneficiaries (Fano, 1981; Lanza, 1991). For instance, during the 1955 *Casse Mutue* elections, Reform Boards pressured land beneficiaries to give up their vote and let *Coldiretti* operatives proxy vote for them (L’Unità, 1955). Moreover, Reform Boards exclusively entrusted *Coldiretti*’s organization for the support of agricultural workers (EPACA) with the assistance of land-reform beneficiaries (Lanza, 1991; Ciranna, 1958). Possibly for this reason, we find that *Coldiretti* growth in treated towns was greater than the mechanical increase of small farms caused by the reform.

Patronage. Over the five decades of DC rule, patronage became widespread (Graziano, 1973; Ferrera, 1996; Shefter, 1977). Chubb (1982) writes that in Italy “when all is said and done, a job signifies a vote and vice versa.” We find anecdotal evidence suggesting that the Reform Boards and the Regional Agricultural Development Bodies that assumed their functions in 1976 were providing public sector jobs in the reform areas based on political alignment. An article in L’Unità (1983) highlights the need to restructure employment in the Regional Agricultural Development Bodies, arguing that their staff included “very few technicians and many bureaucrats (selected through patronage)” and that administrative personnel represented 75% of the employees in the body that replaced the Delta Padano Reform Board. Patronage became arguably more common during the 1980s (Alum, 1997) with public sector employees in Italy growing from 5.6% to 6.5% in 10 years (Bella, 2022). In Section 6.2 we use public sector employment as an indicator for local incidence of patronage.¹¹

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. In the 1994 election, Berlusconi’s newly formed *Forza Italia* ran on an economically conservative platform and became the largest right-wing party in Italy.

¹¹Anecdotal 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 (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.

3 Data Sources

In this section, we describe the main variables used in the analysis, and the respective data sources (see Appendix B for data construction and variable description).

Treatment: Reform Towns. The main town-level treatment indicator is a dummy for whether the town belongs to the reform areas. To define this variable, we rely on the lists of reform towns in the executive orders implementing 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). 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.

Main Outcome: Election Results. We measure the popularity of the party that implemented the reform using the DC vote share in national elections – our main outcome variable. We obtain this data from the *Istituto Cattaneo* (Corbetta and Piretti, 2009). Our main results focus on elections between 1946 and 1992. We define time-consistent geographic units based on the list of towns in 1951. To measure DC’s influence on voters’ attitudes and preferences, we also digitized town-level results from the 1974 “referendum on divorce” (Ministry of Interior, 1977) as an additional outcome variable. Our empirical strategy relies on parallel trends at the border and we study pre-trends extensively. A fundamental challenge in measuring pre-reform political trends is the absence of free political elections during the Fascist regime (1924-1945). We analyze the evolution of political attitudes during these years using a number of variables, including the elections results of 1919, 1921, and 1924 from Corbetta and Piretti (2009), political activists monitored by the Fascist regime through the “Casellario Politico Centrale” (Archivio Centrale dello Stato, nd) and episodes of Fascist violence in the 1920s (Acemoglu et al., 2022) and during World War II (Fulveti et al., 2016), and with the 1946 municipal elections (collected from two newspapers close to DC and PCI: *L’Avvenire* and *L’Unità*).

Data for the analysis of mechanisms. Section 6 discusses several potential mechanisms behind the long-run electoral effects of the reform: these analyses use additional variables capturing clientelism, migration, and economic outcomes. First, we focus on clientelistic practices. For this purpose, we digitize new data on beneficiaries cooperatives (Ente per la colonizzazione del Delta Padano, 1963; Ente Maremma, 1964). We also develop new measures to capture *Coldiretti*’s penetration. We use results of the board elections of farmers’ local healthcare providers (*Casse Mutue*), as well as the budgets of these providers (Alleanza Nazionale dei Contadini, 1970; Federazione Nazionale Casse Mutue, 1966)¹². In addition, we digitize new, town-level electoral preferences for

¹²Elections of *Casse Mutue*’s board of directors were generally held every three years and always saw

all candidates running in the parliamentary election of 1948 and 1958 from the Historical Archive of the lower chamber of the Italian Parliament (Archivio Storico Della Camera dei Deputati, 1948; 1958) and determine *Coldiretti* candidates based on endorsements by the association’s magazine (*Il Coltivatore*, 1948, 1953). To measure patronage, we analyze town-level census data on public-sector employment (ISTAT, 1937, 1955, 1965, 1974a, 1985, 1995).

Second, we examine treatment effects on population and population composition, using decadal population censuses from 1936 to 1991: this allows us to gauge the impact of the reform on migration (ISTAT, 1937, 1955, 1965, 1974a, 1985, 1995). Third, we look at the economic impact of the reform with data from the population and economic censuses (ISTAT, 1937, 1955, 1965, 1974a, 1985, 1995, 1954, 1964, 1974b, 1984, 1996), where we observe homeownership rates, labor force participation by sector, number of production plants, and number of workers per plant. In addition, we digitized town-level income in 1981 from Marbach and Ciapparelli (1983) which is the earliest collection of town-level income.

Auxiliary data. In addition to the primary data sources described above, we use several auxiliary sources to investigate balance, pre-trends, and the effects of the reform on other outcomes. First, to measure the effects of the reform on farm ownership (Section 5.1), we use data from the 1961 agricultural census (ISTAT, 1962a). The census includes tables with the number of farms by type of management (e.g. owner-operated, tenant farming, share-cropping), the outcome variable in Figure 3. We also found the same variable for a subset of our towns in the 1929 land registry, which we digitize (ISTAT, 1936) and use for balance. To look at land distribution before the reform, we also digitized data from Medici (1948)¹³. Second, for balance and pre-trend analysis, we use data on the potential yield of wheat and maize from IIASA/FAO (2012), elevation and slope from USGS (2005), and population and economic census data from 1936 and 1951 (ISTAT, 1937, 1955, 1954). Third, we test whether towns in land reform areas were also targeted by other contemporary public policies: 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

Coldiretti candidates running. We digitize all available election 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). In the 20 km sample, two towns have missing budgets, 86 have missing voting records and 87 have missing information on the votes to the *Coldiretti* list (the “Bonomiana”): these missings account for the smaller sample sizes in these regressions.

¹³The 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).

(2023); we digitize new data for the 1949 *Piano Casa* from INA Assitalia historical archive (Istituto Nazionale delle Assicurazioni (INA), 1956). Information on the importance of sectors affected by the GATT and ECSC agreements is from the newly digitized 1951 population and economic censuses (ISTAT, 1955, 1954). Fourth, to test whether the reform induced land invasions in non-targeted areas, we digitized documents from the archive of the Ministry of the Interior and from local police reports on land invasions in 1951-52 (Ministry of Interior, 1952).

4 Empirical Strategy

This section describes our empirical strategy. First, we illustrate our approach, which consists in a difference-in-differences on a sample of towns around the reform border. Second, we test our identification assumptions.

4.1 Research Design

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 run a difference-in-differences on a sample of towns close to the reform border, exploiting the longitudinal nature of our data.

Our empirical strategy is based on the following equation:

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

where y_{irt} is an outcome in town i , reform area r , election year t . Our parameters of interest are the β_t year-specific treatment coefficients. The model also includes town fixed effects, η_i , and reform area $\times$ year fixed effects: η_{rt} . We cluster standard errors by town and show robustness to spatial autocorrelation.

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 interacted with reform areas fixed effects. In both cases, we estimate these functions for each election year.¹⁴ The unidimensional model identifies the discontinuity in the most parsimonious

¹⁴Formally: 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. In Section 5.3 we also report results from specifications between these two extremes.

way, while the multidimensional model allows geography to have non-linear effects while avoiding overfitting (Dell, 2010; Gelman and Imbens, 2019). As we discuss later, the main results of the paper are robust to both approaches, reducing the chances of false positives. Conversely, throughout the paper, we interpret with caution any analysis in which the two specifications support different conclusions, for example when we examine mechanisms behind the persistence of the electoral effects.

To ensure comparability we restrict the sample to towns close to the reform border. This ensures similarity of treatment and control towns, as shown by balance and pre-trend analysis which we report later in this section. We follow the guidelines of the regression discontinuity literature when selecting the bandwidth. First, we consider distance to the reform border as running variable and select an optimal bandwidth of 20 km, following Calonico et al. (2014). Second, for the multidimensional regression discontinuity design, we select an optimal bandwidth of 35 km, following Keele and Titiunik (2015).¹⁵ In addition, since there is no consensus on how to compute optimal bandwidths in panel RD, we report for both specifications results for alternative bandwidths between 10 km and 50 km. Thus, the narrowest bandwidth we present (10km) is half the shortest optimal bandwidth and estimates effects on very small samples. Figure 1-Panel B shows 20 km buffers inside (dark red) and outside (orange) the reform areas we consider in the analysis, which include 417 towns.¹⁶

This empirical 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).

The difference-in-differences model 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, for example, if towns excluded from the reform voted against DC to punish

¹⁵For both approaches, we first derive optimal bandwidth (Calonico et al., 2014) separately for each election year. In the unidimensional case, 19 km is both median and mean across years (range: 16-26 km). When we pool observations for all years, we also obtain 19 km. We round it up to a 20 km bandwidth. For the multidimensional case, following Keele and Titiunik (2015), we estimate optimal bandwidths at different points on the border: across all points and years we obtain a median optimal bandwidth of 33 km. The range is wide and the average higher (84 km), as the optimal bandwidth is large in border points with low density of observations (as explained in Keele and Titiunik, 2015). We use the median as our preferred bandwidth, again to minimize bias, and round it up to 35 km.

¹⁶Section 2 suggests that our empirical strategy is not suitable in the South. Appendix D.1, shows that key identification assumptions fail in the South, as pre-trends in vote shares at the border are not parallel.

the government. We explore the validity of this assumption in Section 4.2, too.

Finally, for some outcomes (e.g. farm management and brokers' penetration) we have no pre-reform data and cannot estimate a diff-in-diff. For these outcomes we estimate a spatial RD, taking advantage of the continuity of most covariates at the border.

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.

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 model:

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

where the outcome variable y_{ir} 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, 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 0.013 in the distance specification (p -value: 0.435) and 0.005 in the coordinates specification (p -value: 0.594).¹⁷ Figure 4-Panel B presents a bin scatter and shows graphically the continuity at the border. DC representation inside Parliament also evolves similarly in treated and control towns: comparing DC politicians sitting in the 1948 Parliament with DC members of the 1946 Constitutional Assembly, we find no difference at the border (Appendix Figure C.1). In Section 5.3, we also examine robustness of our results to different degrees of pre-trend violations using the approach by [Rambachan and Roth \(2023\)](#).

¹⁷Other panels in the Figure 2 show parallel pre-trends for all other major parties, including the Communist party. Appendix D.3 discusses these results in detail.

One concern is that only two elections took place before the land reform and after World War II. To address this concern, we implement several approaches. First, we look at pre-Fascism elections in 1919, 1921 and 1924¹⁸. 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. Like DC, the PPI had a strong connection with the Church (its founder and leader was a priest) and aimed at organizing Catholic voters. Appendix Figure C.2 reports treatment coefficients from a panel 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-1948 elections for every bandwidth between 10 and 50 km. Across specifications and bandwidths, parallel trends hold over the 30 years preceding the land reform.

Second, we analyze the records of political opponents to the Fascist regime (1924-1945) preserved in the “Casellario Politico Centrale” ([Archivio Centrale dello Stato](#), nd). The database recorded subversive and politically dangerous individuals between 1894 and 1945 and was used extensively by Fascists: out of 153487 records, 110849 were added in 1925–45, when no free elections were held but when the regime closely monitored political activism. Appendix Figure C.1 shows that number of leftwing dissidents changed similarly at the border between the first (1924-34) and second (1935-45) decade of the Fascist regime. Because the registry records any political activism, these results provide additional evidence of parallel pre-trends in political attitudes.

Third, we study episodes of Fascist violence in the 1920s ([Acemoglu et al., 2022](#)) and during World War II ([Fulveti et al., 2016](#)). Mussolini’s Fascists met considerable opposition both in the early 1920s and during World War II. Because Fascists responded with violent reprisals, we can use Fascist violence as a proxy for political opposition at the beginning and at the end of the regime, asking whether the intensity of violence changed differentially at the reform border. World War II reprisals in our sample are unlikely to be driven by the location of major front lines, as the important Gothic line lies far from all control towns (Appendix Figure C.6). Appendix Figure C.1 shows that treated towns did not experience a differential change in violent episodes, and provides additional evidence that political attitudes were not trending differentially during the 20 years of Fascist rule.

While we acknowledge that there is no perfect solution to the lack of free elections in 1924-46, these additional tests, combined with lack of pre-trends on 1936-1951 census variables documented in the next paragraph, provide considerable support for the research design.

Census Variables. We digitized town-level data from the 1936 and 1951 population

¹⁸1924 being the country’s last multi-party election before the consolidation of the fascist dictatorship and until 1946.

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 no systematic differential evolution of observable outcomes across the border.

Balance. Since town fixed effects capture time-invariant differences, our diff-in-diff strategy does not require balance in levels. Nevertheless, in Appendix Figure C.3, we also test for balance on the two sides of the border at the time of the reform for the following variables: *i*) land distribution, including the share of expropriable estates and the share of farms and land managed by the owner (i.e., a proxy for small-scale cultivation);¹⁹ *ii*) DC MPs and vote shares for all the major parties in 1946 and 1948, the two national 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; *v*) political dissidents monitored during the first and second decade of the Fascist regime and Fascist violence in the early 1920s and early 1940s.

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.²⁰ It also provides support for the spatial RD analysis we implement on those outcomes for which we lack baseline data (e.g., farm management).²¹ We do observe unbalance in some geographical variables (e.g., town

¹⁹As 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).

²⁰To the best of our knowledge, town-level data on *Coldiretti* membership do not exist ([Lanza, 1991](#) p.58). In Section 6.2 we measure the presence of *Coldiretti* at town level indirectly, using the results of the elections of *Casse Mutue*. Four 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); 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.

²¹The spatial RD approach we use for some outcomes requires continuity. To our knowledge, there

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 diff-in-diff 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., 2025](#)), the Marshall plan (1948-51; see [Bianchi and Giorcelli, 2023](#); [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 cut through province borders and our results are robust to the inclusion of province $\times$ year fixed effects (Tables C.4 and C.5). Second, Appendix Figure C.5 shows that exposure to these policies did not change discontinuously at the borders of the land reform.²²

4.2.3 Stable Unit Treatment Value Assumption

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

We propose four approaches to mitigate this concern. First, we collect new data on land invasions in the years following the land reform. We ask whether control towns experienced more invasions following land redistribution, an extreme form of resentment. Second, we consider heterogeneity across control towns in potential 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

is no standard test to check the smoothness of the density when the discontinuity is multidimensional. Appendix C.4 discusses the result of the continuity test of [Cattaneo et al. \(2020\)](#) based on the distance to the border.

²²In 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*: 0.6% (4%) of control towns.

²³The reform would also affect outcomes in control towns mechanically if some of the beneficiaries of the land reform came from control areas. This type of spillover is not a concern because *in practice* almost all beneficiaries were resident of treated towns (cf. Section 2). Section 6.3 discusses migration in depth.

across control towns in the 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, we estimate Equation (1), but drop towns close to the reform border (this is reminiscent of Barreca et al., 2016’s “Donut RD”). Similar to the previous exercise, if voters in control towns resented the reform and punished DC after 1950, we would expect this effect to be larger close to the border, where voters were likely to be more aware of the reform. Appendix C.7 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 diff-in-diff hold: Pre-trends are parallel for voting, patronage, and other economic variables. In addition, it is reassuring that towns located just inside the reform area were broadly similar to those just outside: while diff-in-diff does not require balance, the similarity of border towns increases the credibility of our main strategy and also of spatial RD estimates for outcomes where we lack a panel dimension. Finally, we find no evidence that contemporary policies affected differentially treated and control towns at the border.

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: 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. 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). Because information for this variable is complete only for 1961, for this outcome we estimate treatment effects with the spatial RD.

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 RD

model in Equation (2). We report coefficients for bandwidths from 10 to 50 km. Treated towns have on average between 9 and 17 percentage points (p.p.) more owner-operated farms, from a control average of about 72%. The effect is significant and stable across bandwidths in the distance specification and grows at larger bandwidths in the coordinate specification. Similarly, in treated towns, the share of land in owner-operated farms is 9-18 p.p. higher than in control towns, from a control average of 40-46%. These results are not affected by controlling for the value of this variable in 1929 (Appendix Figure D.3), which is available for a smaller sample but was balanced between treatment and control (see Appendix Figure C.3). 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.7 shows these effects on a map of Italy. The map confirms that border towns inside of the reform area swung in favor of DC.

Panel Estimates. Next, we estimate Equation (1) for elections 1946-92. Figure 5-Panels A-B report point estimates and 95% confidence intervals for β_t : the effect of the land reform on DC vote shares in every election. Panel A presents estimates controlling for distance to the border and Panel B for the polynomial in coordinates. The sample in Panel A (B) consists of 417 (652) towns within 20 (35) km from the reform border in the study area, 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 (β : 0.005-0.013; p -value: 0.44-0.6). 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 37-38%: an increase of 5 to 11 percent. The effect is larger in the unidimensional specification with distance, but precisely estimated in both specifications. In sum, land redistribution moved votes: in the average town, out of one hundred voters ten benefited from the reform, and two to four switched to DC (treatment effects). Below we explain how we calculate these numbers and use them to estimate large persuasion rates.

The emergence of electoral effects already in 1953 is noteworthy, but entirely plausible. Two-thirds of reform town workers were employed in agriculture, and these workers had waited for land redistribution for generations. Timing is also plausible: expropriations in Maremma and Delta Padano were complete by the 27th of January 1953, and Reform Boards assigned land immediately after (Russo, 1955). Thus, when on the 7th of June 1953 Italians voted, the process of redistribution was well-advanced and likely to be in the minds of many voters. Indeed, our estimates of the impact of the reform on DC votes in the 1950s are in line with anecdotes and descriptive statistical evidence produced at the time. Amintore Fanfani, a DC leader, noted in 1956 that “in the reform areas, the *Scudo Crociato* [the DC symbol: a crusader shield] shines while the hammer and sickle rust” (Ufficio DC, 1956). A 1957 academic study records large DC gains across five reform towns after redistribution (Toldo, 1957). In one dramatic episode, 220 former Communists publicly tore down their PCI membership cards and joined DC after receiving their plot of land from the *Reform Board* (Il Mattino, 1951). More broadly, the impact in the first election is consistent with the literature on the short-term electoral impact of public policies (Manacorda et al., 2011; Zucco Jr, 2013).

DC gains remain large and stable until 1992 (Figure 5-Panels A-B; in 1953-92, the average DC vote share in control towns ranges between 29% and 40%). During the four decades following land redistribution, the land reform 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 reforms’ voting effects 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 report the pooled (1953-1992) treatment coefficients and pre-trends (1946) of the main specification with town fixed effects (base year 1948) for bandwidths between 10 and 50 km. The effect is stable for both specifications (top panels) and very similar between small and large bandwidths. Consistent with Figure 2, pre-trends are not significant at all bandwidths.²⁴

DC support in treated towns came mainly at the expense of the PCI and had minor impact on other parties and turnout. 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.3 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

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

already DC supporters.²⁵ With the distance (coordinates) specification we find a persuasion rate of 0.80 (0.60): out of five people who received land and were not already DC voters, four (three) started voting for DC as a result of the reform. The effect is large but plausible, given the magnitude of the asset transfer. We note that positive spillover on non-beneficiaries (e.g. on beneficiaries' relatives) would imply a lower persuasion rate. In contrast, if envy and resentment among non-beneficiaries *living in treated towns* lead them to stop voting for DC, this would imply a higher persuasion rate.

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

Using 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 diff-in-diff, and estimate the effect of the reform on the 1974 referendum along with its effect on Parliament elections, pooling Parliament election coefficients by decade. Appendix Figure D.10-Panels A-B shows that repeal received higher support in treated towns, with economically meaningful point estimates consistent across the distance and coordinate specifications (+2.3 p.p. and +3.1 p.p., respectively, from a control mean of 0.41), although the estimate is noisier in the distance one ($p=0.219$ vs. $p=0.004$). Appendix Figure D.10-Panels C-D shows that these effects are relatively stable across bandwidths and specifications, if anything, become larger as the bandwidth increases. Overall, these results suggest that treated towns did not vote with DC only out

²⁵For 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: $dc^T - dc^C = 0.044$ or 0.032 , the two 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 (1948) in the 20 and 35 km bandwidth; $b^C = 0$, the share of beneficiaries in control towns; $b^T = 0.096$, the share of net beneficiaries in treated towns. The share of net beneficiaries in treated towns is estimated with data of beneficiaries, expropriated landlords and total population. In the average reform town, 224 households received land expropriated from 7 landlords. We assume that every household casts three votes, so that the average town has $(224 - 7) \times 3 = 651$ net beneficiaries. There are 6784 (6782) voters in the average town within 20 (35) km from the border, so net beneficiaries over voters is about 0.096. Calculations with the median number of voters (2552 and 2597) still yield persuasion rates larger than in the literature: 0.30 and 0.23. Three voters per family is perhaps conservative: in 1951 the average family counted 4 members, and many families would not have more than two eligible voters. Nevertheless, families with more voters would imply smaller persuasion rates.

of economic interest and instead aligned themselves more broadly with the DC agenda.²⁶ 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 Appendix Figure D.12-Panels A-B we aggregate the votes of post-1993 DC parties and plot the treatment effect for 1994, 1996, and 2001. Coefficients on DC vote shares in post-1992 elections are noisy. The point estimates are smaller than in 1992, but the differences are not significant. Lack of precision and split of DC into multiple smaller parties, aligned with both major coalitions, prevent us from drawing conclusions from the post-1992 elections.²⁷ 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.

To summarize, the reform generated persistent political benefits for DC. Voters supported not only DC candidates, but also its policy agenda, as shown in the analysis of the divorce referendum.

5.3 Robustness

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

Alternative Specifications. In Appendix Tables C.4 and C.5, 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 in the unidimensional specification and 35 km in the multidimensional one). We then estimate variations of these specifications: Tables C.4 and C.5 drop all provincial seats including Rome (col. 2); control for a 2nd order polynomial in distance (col. 3 of C.4) or a linear polynomial in coordinates (col. 3 of C.5); interact the linear polynomial in distance with reform area dummies (col. 4 of C.4) or we no longer allow the effect of coordinates to vary by reform area (col. 4 of C.5). In the remaining columns of Tables C.4 and

²⁶Appendix Table C.3 presents results from a robust RD specification (Calonico et al., 2014 and Calonico et al., 2020): the point estimate is similar (0.03), but noisier (p -value: 0.137), consistent with the idea that the town fixed effects in the diff-in-diff absorb residual variation and improve precision. We also observe that divorce was still rare in 1981 (0.25% of marriages). Appendix Figure D.11 shows no significant differences at the border (if anything divorces were slightly higher in treated towns, which voted more *against* divorce).

²⁷For 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.

C.5, we include three different fixed effects $\times$ year: eight electoral districts (col. 5), ten segments (col. 6), and twenty-nine provinces (col. 7). Tables C.6 and C.7 demonstrate the robustness of our estimates to controlling for predetermined characteristics that are sometimes unbalanced: we control for each of these variables separately (cols. 2–10) and then add them in the same regression (col. 11). Tables C.8 and C.9 show results from nearest neighbor matching and CEM, matching treated and control towns based on observable characteristics. Results from these specifications are robust and similar to our baseline: if anything, more parsimonious specifications such as a linear polynomial in coordinates point to stronger effects.

We also split the reform border into 10 segments and assign every town to one of these segments (Appendix Figure C.8). We then estimate the effect of the reform after dropping towns close to each of these 10 segments. Figure C.9, 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.

Robustness to parallel-trend violations. We use [Rambachan and Roth \(2023\)](#)’s method for robust inference under potential pre-trends violation. We find that, even in the data-informed worst-case scenario where post-treatment deviation from parallel trends is as bad as the worst deviation in pre-treatment ($M=1$), the 90% confidence set on DC voter shares is $[-0.003, 0.084]$ in the distance specification and $[0.012, 0.049]$ in the coordinate specification. In Figure C.10, we report the confidence set of several values of M , therefore illustrating transparently the sensitivity of our results to potential violations of pre-trend assumptions.

Instrumental Variable: the Borders of the Original Legge di Riforma Agraria. Reform areas that redistributed land largely followed the definition of “Zone B” of the *Legge di Riforma Agraria* (Appendix Figure A.1). 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% exclusions, 7% additions). 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 Table C.10 shows the strength of the first stage. Panel A of the same table shows reduced form and IV estimates for DC shares and confirms the electoral effects of the reform on DC vote shares when using the proposed reform area. The table, however, also suggests 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.11 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.11-Panel A plots the 20 coefficients of these regressions (on the y-axes) against the location of the fictitious border (on the x-axes). In the same graphs we also report the real coefficient (in red). Figure C.11-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.11 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.11. Correcting for spatial correlation has no material impact on the significance of the reform on DC vote (col 1).

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. We consider five channels that could drive this persistence: i) gratitude and reciprocity (Healy and Malhotra, 2013), ii) continued exchange between voters and politicians, and clientelistic practices (Hicken, 2011); iii) voter migration (Cantoni and Pons, 2022); iv) economic transformation, (Lewis-Beck and Paldam, 2000); v) changes in voters' attitudes and preferences (Di Tella et al., 2007). In this section, we discuss evidence for each of these potential mechanisms.

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 over time, 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.²⁸ In the 1950s and 1960s age structure did not matter for DC gains. 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 gratitude driving some of the effects in the early decades, but becoming less important in the 1980s, when many of the voters who experienced the reform first-hand 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 and stable impact of the reform over 40 years. We now turn to one mechanism that may help explain such persistence: clientelism.

6.2 Continued Political Exchange and 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, in an attempt to increase political support (Robinson and Verdier, 2013). In the Italian case, several features of the land reform – discussed in Section 2 – limited its benefits and increased beneficiaries’ dependence on DC: 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 three ways in which DC provided reversible benefits after the reform: local cooperatives, political brokers and patronage through public employment.

Enti di riforma and cooperatives. After redistributing land, the *Enti di riforma* operated for over two decades, providing agricultural training and commercial assistance to beneficiaries, as well as local public goods. The heads of these agencies were political appointees, and scholars have noted the weaponization of these services for political gain (Pezzino, 1972). Beneficiaries were also required to join a cooperative, and the *Enti di riforma* staffed many of them with DC operatives (King, 1973). We collect new data on the location of these cooperatives, and in Appendix Table D.2 we show that the electoral effect of the reform is double in the 61%-65% of reform towns that are home to at least one cooperative. We are cautious to interpret these results in a causal way, as cooperatives are not randomly allocated and we lack information on their presence in control areas.

²⁸Age distribution of the general population is a reasonable proxy for the age distribution of beneficiaries. For instance, in reform towns in the Delta, age distribution of beneficiaries from Mazzaferro (1956) resembles that of the 1961 Census, and the average age of the beneficiaries is only 1.6 lower than the average age of the rest.

However these results provide suggestive evidence that beneficiaries' cooperatives helped DC maintain the initial electoral gain of the reform.

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 a role for DC. The presence of many DC members of parliament with ties to the *Coldiretti* speaks to the growing power of the organization. Using information provided in several publications – including *Coldiretti* magazine *Il Coltivatore* (1948, 1953), the parliamentary publication *La Navicella* (1949, 2000), Bernardi (2020), and Primavera (2018) – we document that the number of MPs linked to *Coldiretti* grew from 30 in 1948 to 40 in 1958. Newly digitized data on ballot preferences (Ministero dell'Interno, 1948, 1958) show that preferences for *Coldiretti* candidates grew in our study area from 383'326 in 1948 to 842'788 in 1958, from 11% to 21% of total DC preferences.

As we discuss 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 healthcare providers for 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. Because these data do not exist pre-reform, for these outcomes we estimate treatment effects with the spatial RD Equation (2).²⁹

We obtain two main results. First, Figure 6 shows RD estimates with data from the 1965 *Casse Mutue* budgets. We observe treatment effects of 26-38% in both revenues (Panels A–B) and expenses (Panels C–D) per inhabitant. Appendix Figure D.14 shows that revenues and expenses scaled by 1961 agricultural workers (Panels A–D) or 1970 farms (Panels E–H) are also higher in reform towns. *Casse Mutue*'s boards had some discretion in the use of these funds, and *Coldiretti* members sitting on the boards often used this discretion politically (Lanza, 1991). Larger *Coldiretti* budgets in treated areas suggest that these towns were more exposed to the influence of DC political brokers.

Second, Figure 6 shows RD results on the board elections in 1955-1970. Consistent with the previous results, the number of voters in the board elections is substantially higher in treated towns (by 1-1.2 voters per 100 inhabitants, or approximately 31-35%

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

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*.³⁰ These results indicate that *Coldiretti* could access a larger voter base in reform areas and thus could be more effective in brokering votes for DC.³¹

A back-of-the-envelope calculation suggests that the land reform not only increased the potential member base of *Coldiretti* (i.e. the number of small landowners), but also its actual support among them: approximately 40% of reform-induced small landowners supported *Coldiretti*, compared with a counterfactual share of 24.5%, implying approximately 63% higher support.³² Because beneficiaries may have been selected among most likely to support DC and its allies, this simple exercise is consistent with the historical research documenting favoritism in land allocation and suggests that the reform strengthened the role of DC brokers in reform areas.³³

To sum up, *Coldiretti*, an important DC ally, gained influence in reform towns. The association had a national reach, and all Italian landowners had incentives to join it. However, the reform further strengthened *Coldiretti*: *Casse Mutue* records indicate the organization had more resources and a greater base in reform towns. This is arguably both because land redistribution created more landowners and because beneficiaries were more likely to become *Coldiretti* members, allowing this political broker to target more voters in the treated areas.

Public Sector Employment. Governments’ patronage in public sector employment – a common practice around the world (Shefter, 1977; Acemoglu et al., 2011) – 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 Equation (1), we test whether reform towns experienced an increase in public sector employment. For all census outcomes the baseline (reference) year is 1951, since this census was taken before land expropriations started and captures population characteristics before the reform.

³⁰The reform had no significant effect on *Coldiretti* share, which was already very high at baseline (approximately 72-74%).

³¹Using preference vote data from parliamentary elections, we find no evidence that *Coldiretti* candidates received disproportionately more preferences than other DC candidates in reform towns compared to control towns (see Appendix Figure D.13). This is consistent with the idea that *Coldiretti* promoted all DC candidates in these areas, leading to aggregate gains for DC rather than a reallocation of votes among its candidates. Appendix Figure A.5 shows how *Coldiretti* magazine supported DC as a whole before the 1958 elections.

³²In 1961, there were 12.24 small farms per 100 inhabitants in the control group of 20 km sample. *Coldiretti* received on average 3 votes every 100 people: approximately 24.5% of small farmers supported *Coldiretti*. Treatment effects for small farmers per capita and *Coldiretti* votes per capita are 2.5 p.p. and 1 p.p. respectively, implying that approximately 40% of the new farmers supported *Coldiretti*.

³³For the case of Mexico, Larreguy (2013) shows that brokers in control of communal land overlapping entire electoral districts exerted greater control over voters.

Figure 7-Panels A-B presents the dynamic coefficients. In treated towns, the trend in the share of public sector employment was similar at the time of the reform ($\beta = +0.0047$ with the multidimensional, $\beta = +0.006$ with the unidimensional RD). Treated towns experience a differential increase in public sector employment in the three decades following the reform: in both specifications all coefficients are significant at the 5% level or better, in 1991 only the multidimensional RD estimate remains significant. When we pool the post-reform data, the treatment coefficient is around 1.5-1.8 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 are only marginally significant and disappear at larger bandwidths, where the main effect remains stable and significant. Pre-trends are absent in the coordinate specification. Importantly, the 1981 effect is particularly 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. Note, however, that the data does not allow us to distinguish local and national public workers.³⁴

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 to DC after the reform. This may have been 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. We note two caveats. First, the absolute number of public jobs did not grow, and the higher public employment share is driven by relative stable size of public sector employment under falling population (see next section). However, although public sector employment may be stickier than other sectors, retirements and separations of public employees surely happened over 40 years, and maintaining a stable number of public jobs against the backdrop of a falling population required at least replenishing these positions. Second, 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. While political brokers are specific to clientelistic systems, 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.³⁵

³⁴In the pre-trends analysis based on Rambachan and Roth (2023) the 90% confidence set with $M=1$ is $[-0.001, 0.034]$ in the distance specification and $[0.007, 0.031]$ in the coordinate specification. See also Appendix Figure C.10. Appendix C presents several exercises similar to those described in Section 5.3 for DC: we find that public sector estimates are generally robust to these checks.

³⁵We 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

Discussion. This section provides evidence that DC politicians continued to invest in treated towns decades after the reform. Cooperatives of beneficiaries magnify DC gains, and political brokering and patronage are more prevalent in reform areas. These results suggest that although land redistribution was irreversible, the presence of reversible benefits may have promoted the emergence of a system of continued exchange between politicians and voters based on these practices. The findings are consistent with models predicting that politicians transfer more resources to core voters (Dixit and Londregan, 1996).

This analysis has important implications for the interpretation of the results. The long-term electoral advantages of DC are likely to compound the direct effects of the land reform with the reinforcing effect of the continued exchange. The crucial identification assumption for this long-term analysis, supported by the pre-trend and balance analysis of Section 4, is that the initial large-scale land reform induced continued exchange, as opposed to other time-varying confounders jumping at the border.

6.3 Migration

The reform and its consequences may impact migration from or to reform areas. We first analyze population levels and composition 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.16-Panels A and C show the estimated β_t with our two specifications. There is little evidence of differential migration in the 1950s, the first decade after the reform: the point estimate is zero in the coordinates specification and negative but small when controlling for distance from the border. Starting from the 1960s treated towns appear to lose population. The effect is more precisely estimated in the coordinates specification and indicates that by the early 1990s treated towns were 21% less populous than control towns.

Population Composition. We next consider how out-migration affected the demographic composition of treated towns by looking at decadal censuses. Appendix Table D.3

publications available for town-level municipal budgets (years 1952, 1955, and 1959) and present results in the Appendix Figure D.15. 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), which becomes significant at bigger bandwidths (Panel C). In the coordinates specification, the coefficient remains positive and sizable, but it is smaller (7%: Panel B). 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.

presents the results with our two specifications. The table indicates that reform towns became older (cols. 1-4 and 6-9) and were home to fewer men (cols. 5 and 10). By 1991, treated towns had 0.4 p.p. lower share of males (cols. 5 and 10, base approximately 49%) and between 0.6 and 2.1 p.p. lower share of the population aged under-20 (cols. 1 and 6, 1991 control mean: 21%). 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 people and 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 (i.e. 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 experienced 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.16 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.3 indicates that the demography of treated and control towns slowly diverged over time. 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. Only balanced out-migration across major parties can explain a stable coefficient on DC vote share across decades. In contrast, if differential migration of PCI voters were the only mechanism behind persistence, we would expect treatment effects on vote shares to grow over time. Second, Appendix Figure D.16 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 p.p. in the distance specification (p -value = 0.03) and by 1.3 p.p. in the latitude-longitude specification (not significant: p -value = 0.106). These new DC voters were unlikely to be moving in from control towns, since the vast majority of beneficiaries were previous residents of treated towns. The increase in the number of DC votes is, therefore, consistent with the hypothesis that the land reform shifted some electoral choices within treated towns. It cannot be explained by the out-migration of PCI supporters, including the differential out-migration of PCI supporters in the first years after the reform.

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 in later decades 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.³⁶ 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 number of owner-occupied homes per capita. Because owning one's home is a primary form of household wealth in Italy (Rossi, 2019), the per-capita count of owner-occupied dwellings is a proxy of municipality-level housing wealth per capita. Second, we look at income per capita in 1981, the earliest available year.

Appendix Figure D.17 reports results for Equation (1) with home-ownership as dependent variable. The treatment coefficients in the four decades after the reform are small and almost never significant. The figures show that the reform had a small positive effect on home ownership in 1961 (only in the coordinates specification). This effect is likely associated with the construction of 13'417 new houses that reform boards provided to land beneficiaries (King, 1973). By the early 1960s all new houses were completed, and the subsequent housing boom reduced the importance of this program: by 1991 beneficiaries' houses represent less than 1% of the housing stock in reform areas. Thus, while the reform may have promoted higher home ownership in 1961, the effect appears small and short-lived. This speaks against permanent effects of the reform on wealth accumulation.

Appendix Figure D.18 reports RD estimates of Equation (2) with income per capita as dependent variable. Both specifications indicate that treated towns are not 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,

³⁶Much empirical work on land reform focuses on economic outcomes, such as agricultural productivity (Montero, 2022; Adamopoulos and Restuccia, 2020), poverty reduction (Besley and Burgess, 2000), and structural transformation (Galán, 2018).

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 over time, as agriculture lost importance during the second half of the twentieth century (Appendix Figure D.20-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.4 shows a decline in labor force participation in treatment areas, that already emerges in 1961 but only 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 agriculture 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). 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 support this hypothesis. First, these declines are two to three times larger among women than among men (column 9).³⁷ Second, the reduction in the share of agricultural workers appears to be a driving force in the decline of the labor force share (column 10).³⁸

We conclude that the reform neither hindered nor fostered structural transformation. Two subsequent papers, Albertus (2023) and Bianchi-Vimercati et al. (2026), use research designs that rely on different identification assumptions – respectively, matching of towns at the border and diff-in-diff on the full sample of Italian towns – and suggest that the reform led to specialization in agriculture. With our diff-in-diff analysis of towns close to the border we find generally negative, small and insignificant effects on agricultural employment share: for instance until 1991 the average point estimate is approximately -2.7 p.p and the 95% confidence intervals rule out positive effects of more than 3 p.p. in the multidimensional case and 4 p.p. in the unidimensional case.

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 plant size. Appendix Figure C.3 shows that these outcomes are balanced in 1951. Appendix Table D.5 reports estimates of Equation (1).

³⁷On the contrary, the results on public sector employment (Section 6.2) are not driven by women.

³⁸The reduction is stronger when agricultural employment is divided by working-age population – the denominator of the participation ratio, of which it is then a component – rather than by active population (-0.039 in 1961 and -0.035 in 1971, coordinates specification), accounting for roughly 82-85% of the participation decline in those decades.

There is no effect on the number of plants per capita. In the coordinate specification we find suggestive evidence of a reduction in plant size, starting in 1971. These results are consistent with the decline in population we discussed earlier (Appendix Figure D.16).

Discussion. Analysis in this section suggests a reduction in the share of active population, driven at least in part by mismeasurement of agricultural workers. 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 starts falling in treated towns two decades after the reform (and only in the coordinates specification), a timing that is inconsistent 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 land beneficiaries 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 intergenerational 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 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 Berlusconi into power, 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 turned conservative, we would expect greater support for Berlusconi in reform areas. Similarly, even if the reform did not improve economic conditions, as we have shown in Section 6.4, voters who changed the *perception* of their own socio-economic status because of the reform should remain right-wing even when DC is not the major conservative party. We test this idea with the RD model and data on Berlusconi's party vote shares in the elections of 1994, 1996 and 2001. Appendix Figure D.19-Panels A-F report the treatment coefficient on *Forza Italia* vote share for multiple bandwidths. Treatment coefficients are generally insignificant and small, with signs switching across specifications. 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.19-Panels G-L). This evidence speaks against the intergenerational transmission of conservative values as a driver of electoral persistence.

Discussion. 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 over time. Appendix Figure D.20-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 28%.

7 Conclusions

There is scarce evidence on persistent electoral gains of large-scale redistribution policies, although the political objective of many such policies is to establish long-lasting political support. 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 diff-in-diff around the reform border, 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 healthcare. Such continued exchange between voters and politicians underwrote the long-lasting relationship that these towns established with DC politicians. The relationship persisted for four decades during which DC remained in firm control of the Italian government. Long tenures in office are not unknown in democracies (e.g. Sweden 1932-1976, Japan 1955-1993, and India 1947-1977): thus, our results on continued exchange may apply more broadly. The way in which DC politicians continued to favor reform areas supports theories of political redistribution to core voters (Dixit and Londregan, 1996) and suggests that this type of redistribution may generate long-lasting political support.

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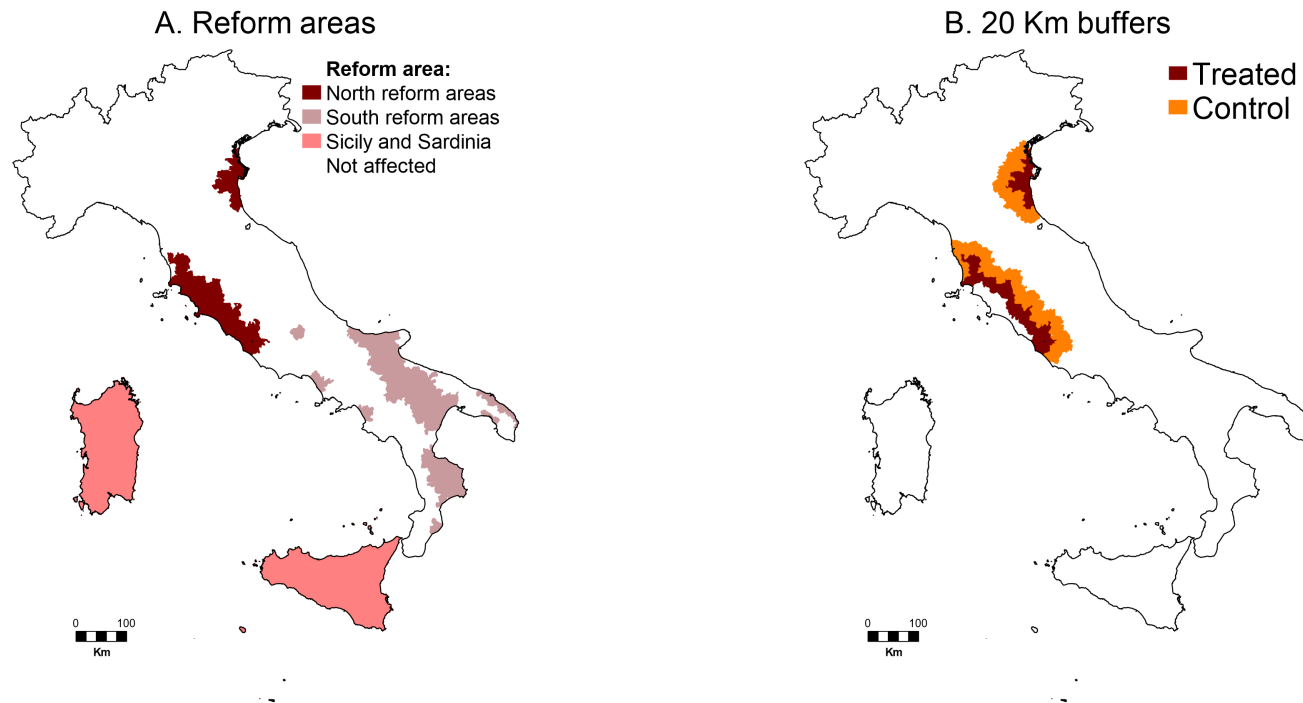
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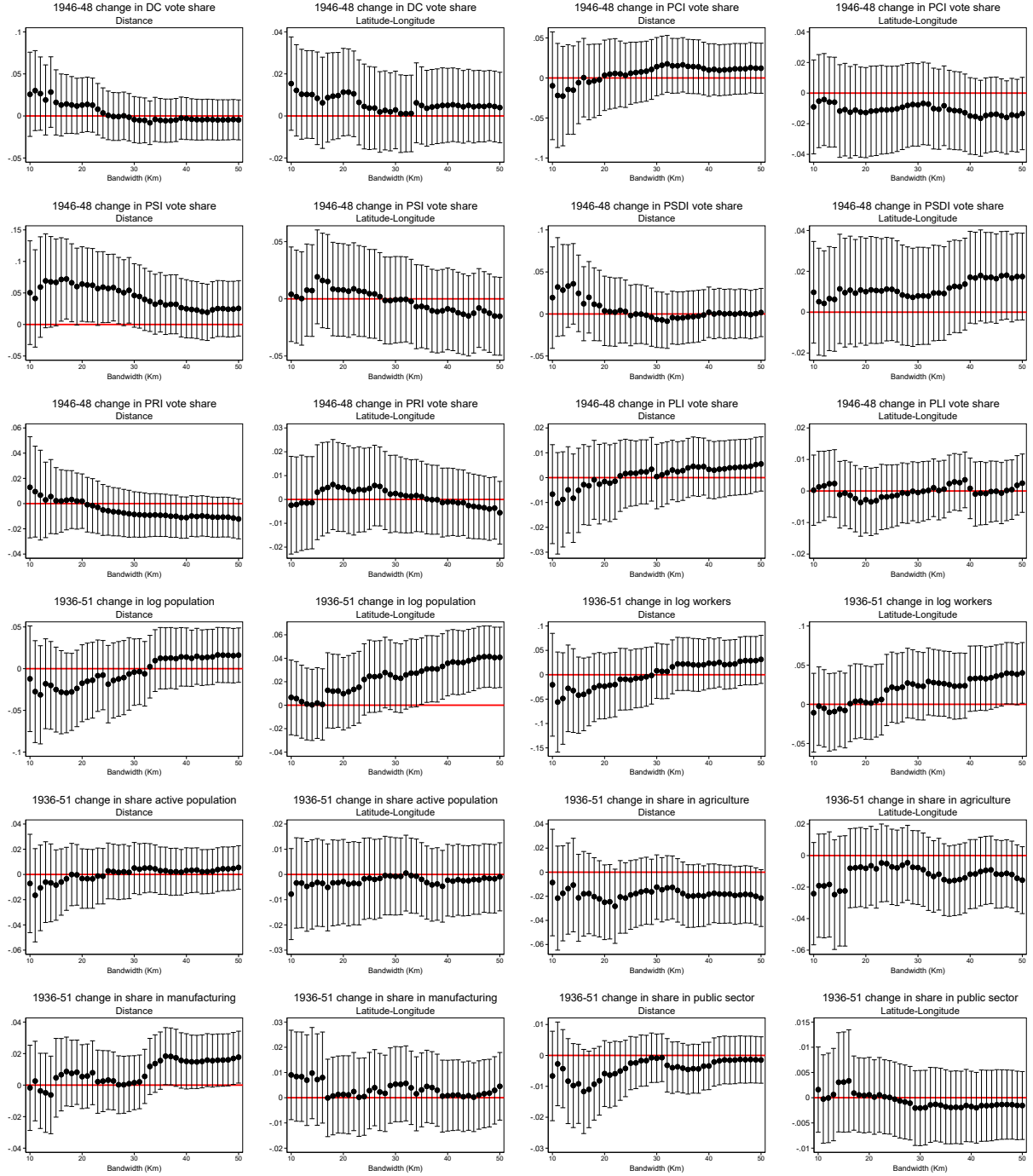
Figures

Figure 1: Reform Areas and Buffers



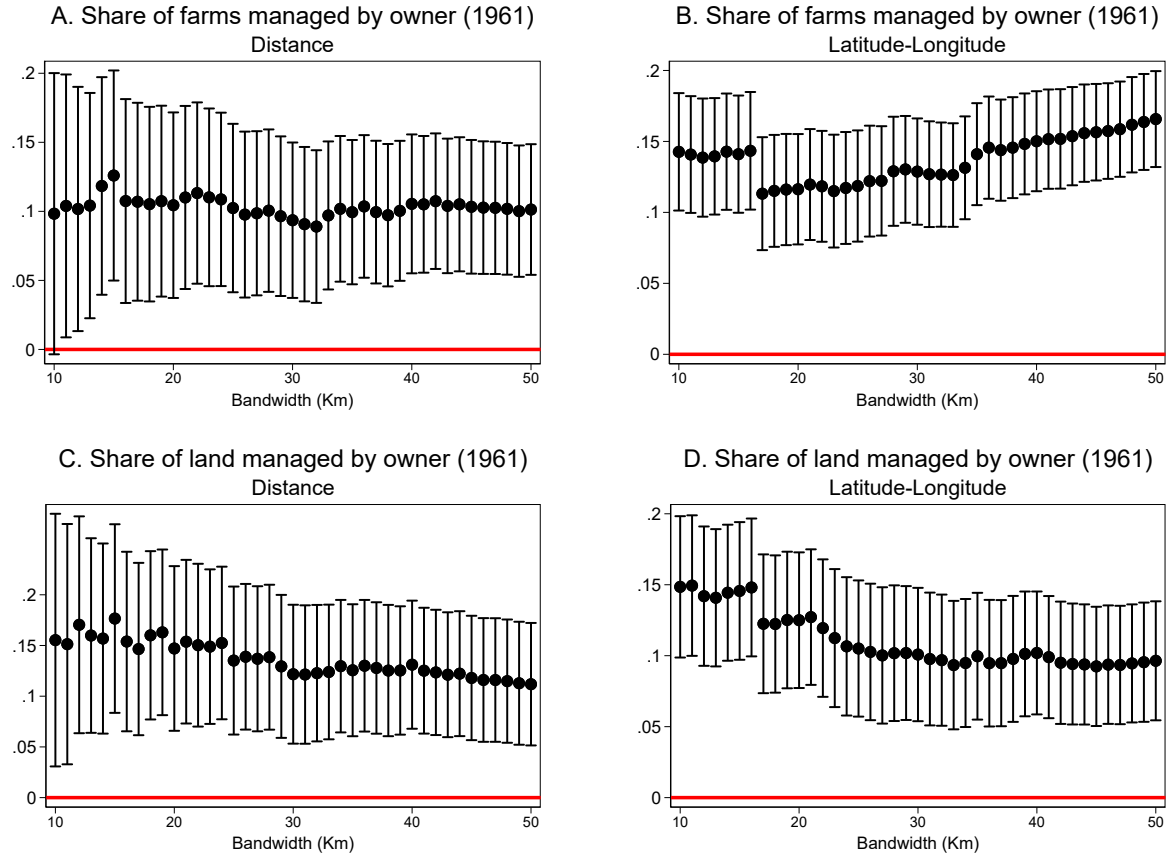
Notes: Panel A: land reform areas as defined in the 1951 executive orders ([D.P.R.66/1951](#); [D.P.R.67/1951](#); [D.P.R.68/1951](#); [D.P.R.69/1951](#); [D.P.R.70/1951](#); [D.P.R.264/1951](#); [D.P.R.265/1951](#)). In dark red the areas of Delta Padano (north-east) and Maremma (center-west). In light 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



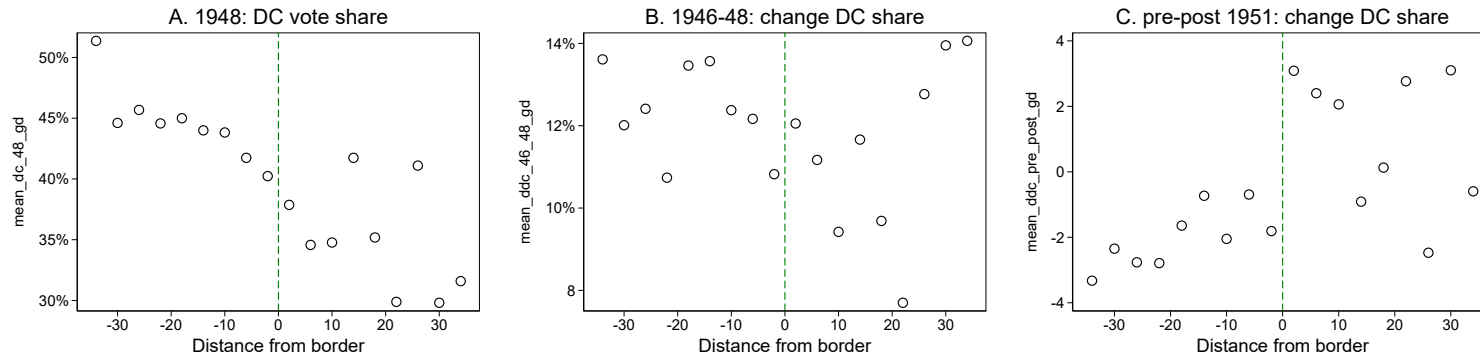
Notes: The graphs report the coefficients of regressions of the specification in Equation (2) along with 95% C.I. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. Dependent variables are changes in the outcome variable between 1946 and 1948 for electoral variables 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.



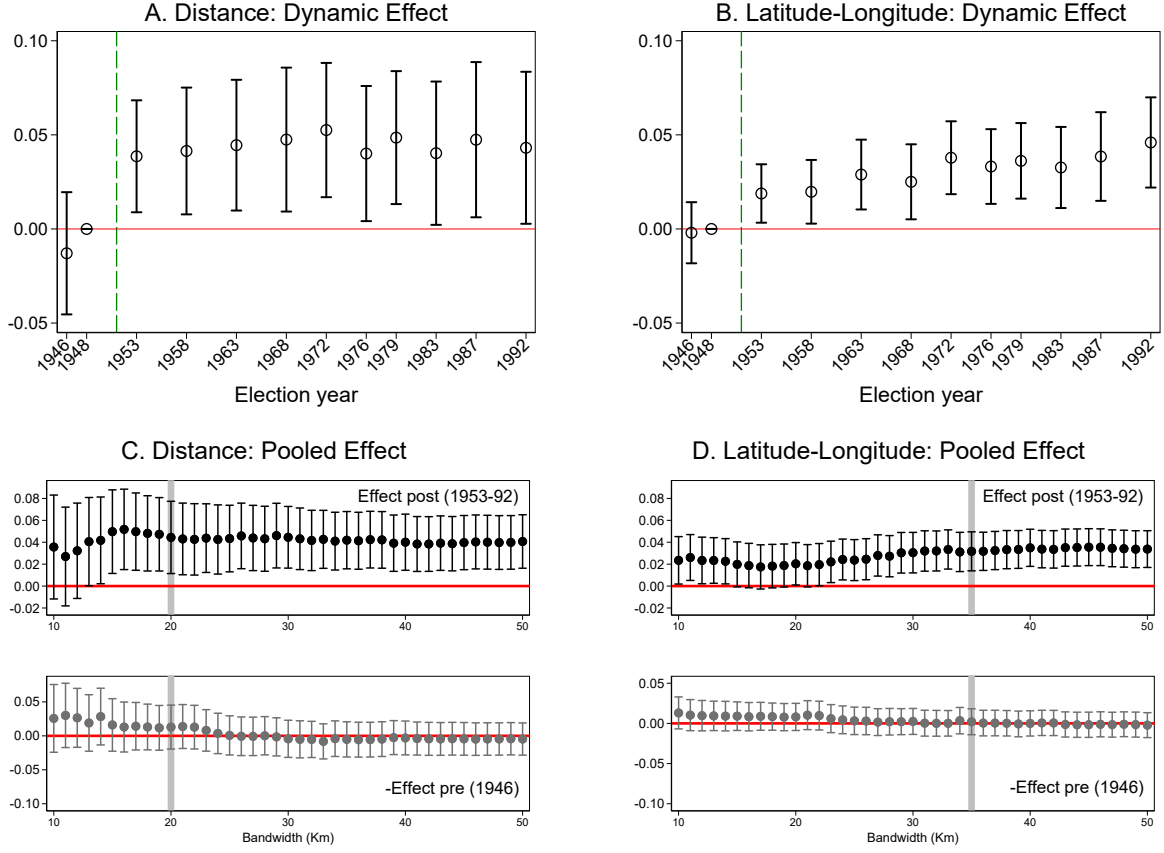
Notes: The graphs report β of regression in Equation (2) and 95% confidence intervals, for 41 different bandwidths between 10 and 50 km from the reform borders of Delta Padano and Maremma. Panels A and B: dependent variable is share of *farms* managed by the farm owner in 1961. Panels C and D: dependent 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 heteroskedasticity 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



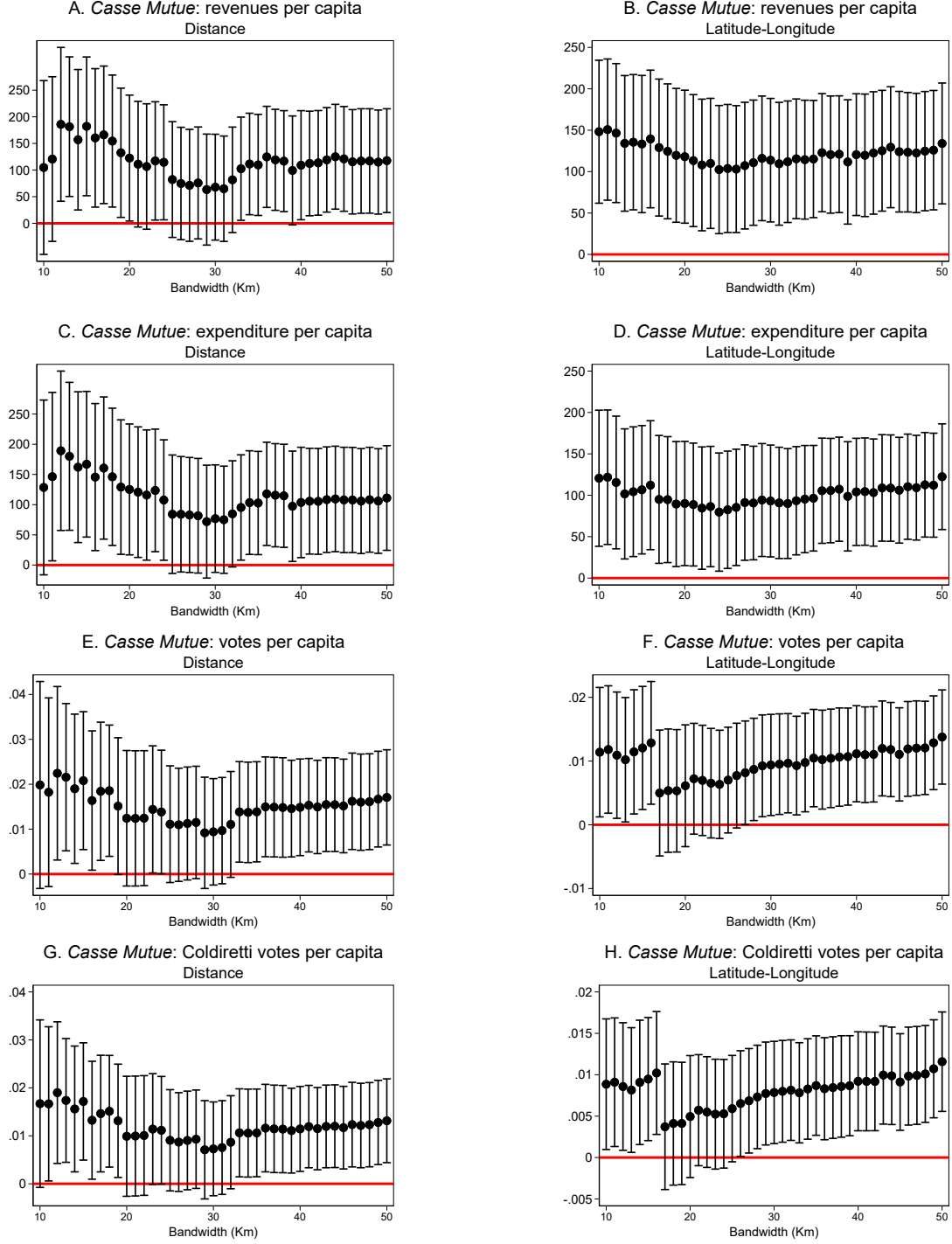
Notes: The Figure presents graphical evidence on the election results of DC at the border. On the y-axes we plot electoral outcomes; on the x-axes the distance to the border. In each Panel, we bin data in 4 km intervals. Treated towns have positive distance and control towns have negative distance. Panel 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



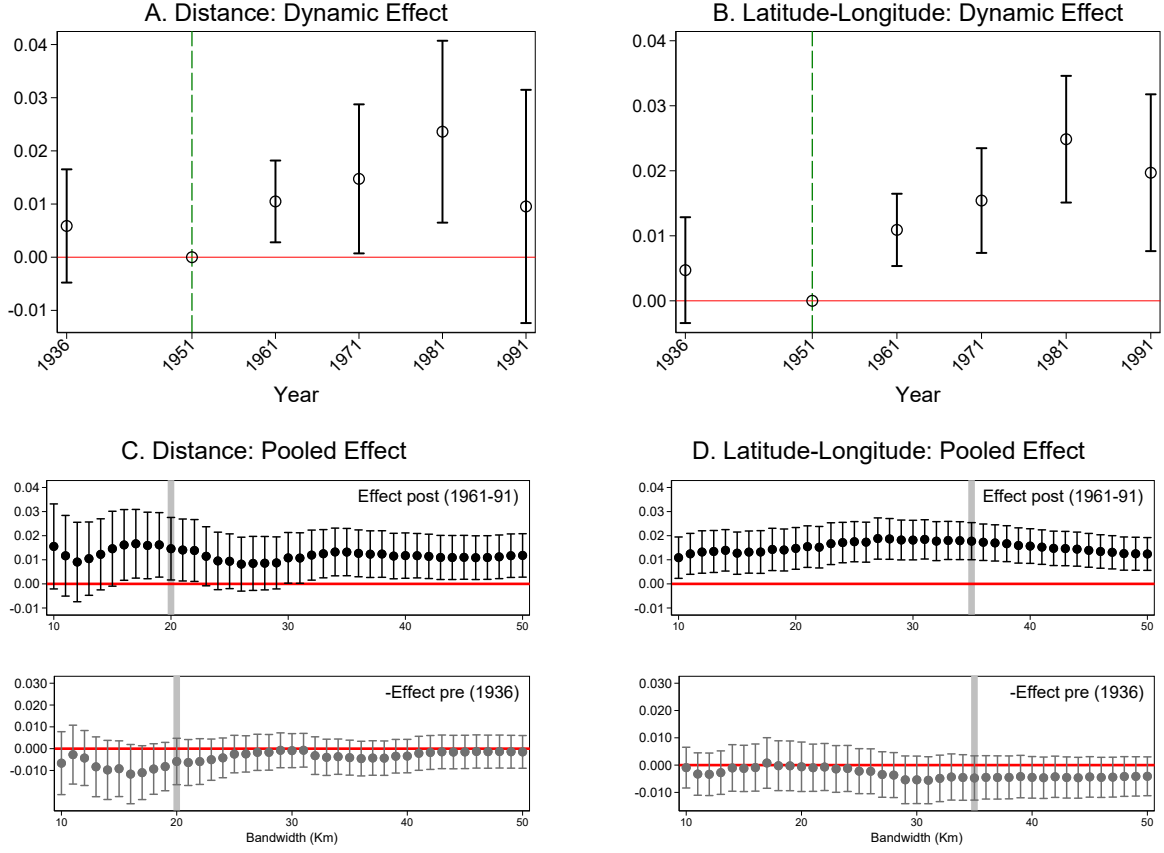
Notes: Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients β_t from Equation (1), $t = \{1946, \dots, 1992\}$. The omitted category is the β of 1948 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano and Maremma. The vertical dashed lines mark the 1951 land reform. Panels C and D report pooled 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 β of 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 (we add reform area fixed effects). 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 f.e.). Standard errors heteroskedasticity robust (Panel A to D) and clustered at the town level (Panel E to H).

Figure 7: The impact of the reform on patronage.



Notes: The dependent variable is the share of active workers employed in the public sector, a form of patronage. Panels A and B display coefficients β_t from Equation (1), $t = \{1936, \dots, 1991\}$. The omitted category is the β 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



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 (center), Opera Combattenti (south-west), Puglia and Lucania (south-east), Sila (south). In pink the islands of Sicily and Sardinia. Remaining municipalities are left blank.

Figure A.2

CAMERA DEI DEPUTATI 28/11/50

Signori,
mi dicono che non
parlerai? Perché?
Mi fusti prima
dimenticato
Condulotti per
l'anno scorso -
Non parlerò perché in effetti
non ho le cose; dopo un quarto
d'ora doveri mettere.
Perciò, non ti sariano che
non puoi valutarci. Le sono
sono soddisfatto sulla tua
"fondazione" (per le condizioni
del nord che avevano nascosto
le 1, non sono 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, Piancastagnaio, 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.

(Panel B) Letter to Segni from DC Siena
Committee

REPUBBLICA ITALIANA
 Il Presidente
 del Consiglio dei Ministri

Per Segni

Rodinò mi dice che agri-
 cultori ribellano d. Negro
 contro estensione compen-
 sorio (Cavarzere
 Cidolonia ecc.)

Comunque ho promesso
 incontro Segn. Segni
 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 Rodinò, 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 e agraria e ri-
 cadono nella legge) sulla zona del Delta (Coppara, Volano di
 Savoia ecc.) sono ora iniziati a cura e spese dello Stato, ope-
 re pubbliche di irrigazione, che importa 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 seethe in resentment for the extension of the reform area (Cavarzere [in Delta Padano, eventually included in the reform area] ecc.). In any case I promised a meeting Segni Rodinò.” Panel D. Letter to the PM De Gasperi from Ministry of Agriculture Segni (1951). Highlighted text: “Dear President. [...] The entire area [he refers here to towns in Delta initially not included in the reform area] is cut by the *Po di Volano* and *Po Morto di Primaro*, today an irrigation canal, thus the area belongs geographically to the Po estuary [Delta Padano].” Panel E: Technical report prepared for the Ministry of Agriculture to help him respond to criticism for modified reform borders (Bandini, 1951). Highlighted text: “Maremma. The final border was smaller than what we ought have done – for the well-known financial reasons [...]” Images ©Fondazione Antonio Segni: Serie IV (Attività politica), Sottoserie 2 (anni 1947-51 - ministro MAF), Fascicolo 1. All rights reserved. With thanks to Fondazione Antonio Segni and Salvatore Mura.

Figure A.3: Expropriation Criteria

SCAGLIONI DI REDDITO IMPONIBILE TOTALE		Imponibile medio per Ha.									
		Lire									
		1000 e oltre	900	800	700	600	500	400	300	200	100 e meno
Lire											
Fino a	30.000	—	—	—	—	—	—	—	—	—	—
Da oltre 30.000 a	60.000	—	—	—	—	—	0	15	30	55	70
»	60.000 a 100.000	—	—	—	—	0	10	30	60	70	85
»	100.000 a 200.000	35	40	47	55	60	65	70	75	84	90
»	200.000 a 300.000	45	50	55	60	65	70	75	80	87	95
»	300.000 a 400.000	52	57	60	65	70	75	80	85	90	95
»	400.000 a 500.000	60	64	66	71	76	80	85	90	95	95
»	500.000 a 600.000	64	70	76	78	80	85	90	95	95	95
»	600.000 a 700.000	68	74	79	82	85	90	95	95	95	95
»	700.000 a 800.000	72	78	82	85	90	95	95	95	95	95
»	800.000 a 900.000	76	82	86	90	93	95	95	95	95	95
»	900.000 a 1.000.000	82	86	90	93	95	95	95	95	95	95
»	1.000.000 a 1.200.000	90	92	95	95	95	95	95	95	95	95
Oltre	1.200.000	95	95	95	95	95	95	95	95	95	95

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

Figure A.4: Original applications

Mod. D. A. T. **4.4.2.60** *non agricola* **Unità Regg.**

Rosso!!! **B** *aziende coop. Edile*

F/M	N. di categoria	N. di registrazione
		7382

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

OGGETTO: Domanda di assegnazione di terreni.

Il sottoscritto Maurizio Maurino di Corinno
e di Rolli Lucrezio nato a Mantelbaum T3 e residente
in Mantelbaum Lomieu Via Enclia 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
pono a verità per cui ne assume piena responsabilità.

Mantelbaum, li 6-5-1952

FIRMA DEL RICHIEDENTE

RISERVATA 1 FEB 1957

ALLA SEZ. SPEC. RIF. FOND.
SERVIZIO SEGR. ed Aff. Gen.

648 **248**

MORELLI Rinaldo
Via Filangieri 15
BARALDA

Riferimento nota n. 3459/229/Segr. A.G. del
17/1/1957 di pari oggetto, si comunica che da ec
certamenti esperiti la MORELLI è risultata non pos
sedere alcun bene di fortuna.

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

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

Si richiama l'attenzione a ciò che si legge

IL DIRETTORE DEL CENTRO
(Dr. Carlo C. Orsola)

Dr. Carlo C. Orsola

TOTALE L.

Con la presente contestualmente espresso mandato alla Cooperativa
trasferire a questo Centro la predetta somma di L.
ritenendola dal rice
ba ottenuto dalla vendita dei prodotti del mio podere.

L'ASSEGNETTARIO

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: Electoral propaganda by *Coldiretti* (1955-1962).



Notes: Electoral propaganda by *Coldiretti*, supporting DC (1955-1962). Left: electoral poster stating: “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*. Right: advertisement published on *Coldiretti*’s official magazine “Il Coltivatore” on the 10th May 1958, before the Parliamentary elections. It explains “how to vote.” Sources: 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 2001,³⁹ with a shapefile of 2001 Italian towns⁴⁰. 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 7788 towns. We drop 8 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 1951.⁴¹ 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, agricultural workers or area and we match the weighted measures to the relevant 1951 towns. This procedure sometimes 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

³⁹We find these lists on <http://www.elesh.it>.

⁴⁰From ISTAT. ISTAT provides a shapefile for 1991 towns, but ELES website does not have a 1991 list of towns.

⁴¹Nicastro, Sambiasi 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; Sorbano was joined into Sarsina. 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.

actual reform borders, although these were closer to the proposal in the North than in the South. Overall, in the South the Government followed only loosely the indications contained in the *Legge di Riforma 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) as shown in Appendix Figure A.1. By contrast, in Delta Padano and Maremma the final borders of the reform followed closely the definition spelled out by the agronomy experts in the original plan: out of the proposed 1.4 million hectares, only 8% were eventually excluded from the reform. Additions accounted for 7% of the final area and they concentrated at the edges of Delta Padano.

Distance to actual reform border. We define continuous reform borders by conflating all contiguous towns inside reform areas. We then compute the distance between the centroid of each town and the closest reform area border.⁴²

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 compute the distance between the centroid of each town and the closest proposed reform area border.⁴²

Electoral outcomes and political attitudes

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 and 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

⁴²The main distance variables (actual, *dist*, and proposed, *distlaw*) in the dataset are from the Northern reform borders (*Delta* and *Maremma*)

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.

Fascist violence: 1920s and WWII. We take violent Fascist actions in the early 1920s from [Acemoglu et al. \(2022\)](#) and construct a similar measure for World War II with massacres recorded in [Fulvetti et al. \(2016\)](#). The variables are normalized with 1921 population and 1936 population respectively. To study pre-trends we construct the change between these two variables.

Political dissidents: 1924-1945. We collect data on political activism during the Fascist regime from “Casellario Politico Centrale” ([Archivio Centrale dello Stato, nd](#)),

which recorded dissidents during the two Fascist decades. We construct two town-level measures of opposition to Fascism: total number of dissidents and dissidents classified as leftwing (socialist, communists, and anarchists). We normalize by total 1936 population and construct the change in the number of dissidents recorded in the first (1924-34) and second (1935-45) decade of Fascist rule.

Preferences for Coldiretti candidates: 1948, 1958. We digitize from the *Archivio Storico della Camera dei Deputati* town-level preferences received by DC candidates at the Parliamentary elections of 1948 and 1958 ([Ministero dell'Interno, 1948, 1958](#)). We identify the DC candidates connected to *Coldiretti* from the endorsement lists published in the association's magazine *Il Coltivatore* ([Il Coltivatore, 1948, 1953](#)), complemented by additional sources on Coldiretti's parliamentary representation ([La Navicella, 1949, 2000](#); [Bernardi, 2020](#); [Primavera, 2018](#)). We normalize the number of preferences received by these candidates with the total number of preferences received by all DC candidates.

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'Unità* and *La Voce Repubblicana*.

DC members of parliament: 1946, 1948. For each town, we count the Christian Democrat (DC) members of parliament born there. Party affiliation and place of birth are from the biographical records of the *Camera dei deputati* – which also cover the members of the Constituent Assembly – and of the *Senato della Repubblica*, together with the volume *I deputati e senatori del primo Parlamento repubblicano* ([La Navicella, 1949](#)).⁴³ Each member is assigned to their town of birth, matched to its 1951 municipality; members born abroad or in towns we cannot match are excluded, and towns with no DC member are coded zero.

Land distribution

Share of expropriable estates: 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 is the number of expropriable estates divided the total number of estates.

⁴³Camera dei deputati: <https://storia.camera.it/deputati>; Senato della Repubblica: <https://www.senato.it/leg/01/BGT/Schede/Attsen/Sena.html>.

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 1929 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 \(1966\)](#). Our baseline measure of the relative size of *Casse Mutue* is total revenues and total expenditure per inhabitant (total population from the 1961 census). In Appendix Figure D.14 we use two alternative normalizations: per agricultural worker (1961 census) and per farm (1970 agricultural census).

Other public policies

Transfers per capita: 1952, 1955, 1959. Municipal budgets are from [ISTAT \(1962b\)](#)⁴⁴. The variable is the inverse hyperbolic sine of per-capita transfers from the central government. We use the 1951 total population.

⁴⁴At 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. Other years are either lost or recorded only at the province level.

Piano Casa *dummy*. Information on *Piano Casa* housing projects built between 1949 and 1956 is from *Piano INA-CASA, Primo Setteennio, Gli Artefici* (Istituto Nazionale delle Assicurazioni (INA), 1956). The variable is an indicator for the presence of at least one project.

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 (2023): 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 the public sector divided total active population. In 1981 public sector is economic sector 9.A.

Share of workers in GATT affected sectors: 1950-51. 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-metallic 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. 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 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.

Divorces per 10'000 marriages: 1981. Civil status of the population is from Table 3 of the 1981 census. The variable is number of divorced individuals divided by the number of married individuals in a municipality, 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 coordinate system.

Distance to coast. We compute the distance to the coast of towns’ 1951 centroid.

Distance to Rome. We compute the distance between Rome’s centroid and towns’ 1951 centroid.

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 30-arc seconds grid covering the entire planet (approximately 0.9 km). 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 30-arc seconds grid covering the entire planet (approximately 0.9 km). 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 are from IIASA/FAO (2012). This data is defined on a 5-arc minute grid (approximately 9 km) 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.

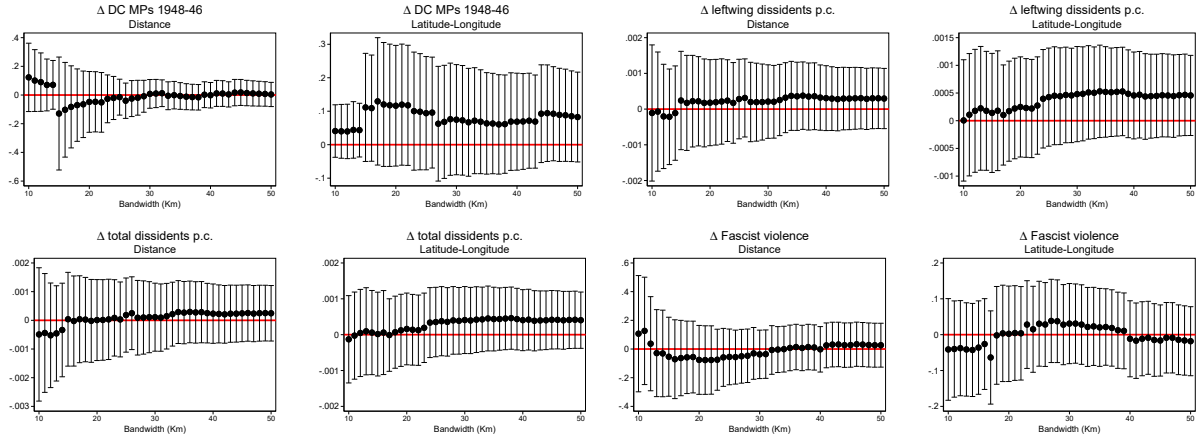
Potential yield: maize. Potential yield data are from IIASA/FAO (2012). This data is defined on a 5-arc minute grid (approximately 9 km) covering the entire planet. We join the raster to the map of 1951 Italian towns and assign to every town the average potential yield of maize with medium-level of inputs of all grid cells falling inside the town limits.

Share of border exposed to treatment (or control). The variable is the share of the town’s perimeter (length of the perimeter of the municipality’s shapefile polygon) lying on the treated–control frontier.

C Robustness and Alternative Specifications

C.1 Additional pre-trends at the border – Northern Italy

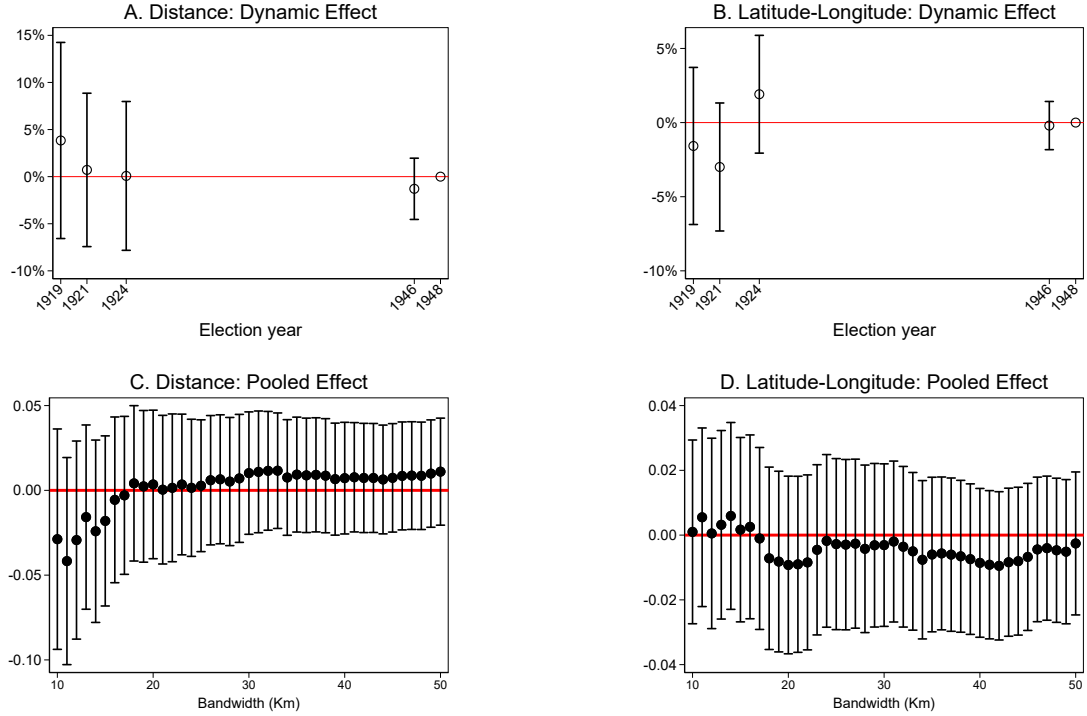
Figure C.1: Pre-Trends at the border



Notes: The graphs report the coefficients of regressions of the RD 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 pre-reform changes in the outcome variable. 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.

C.2 1919-1948 Pre-Trends

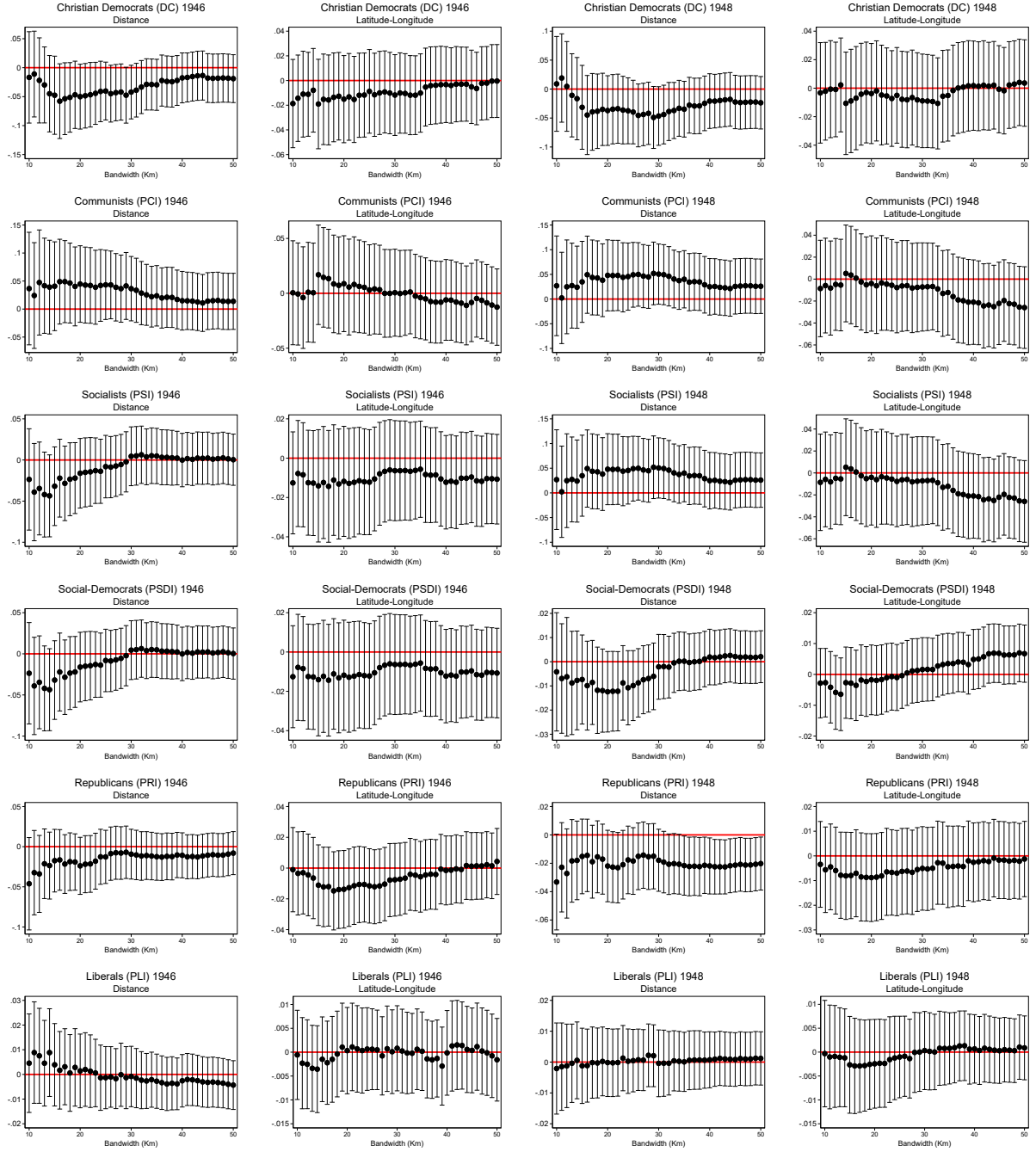
Figure C.2: Pre-Fascism Elections

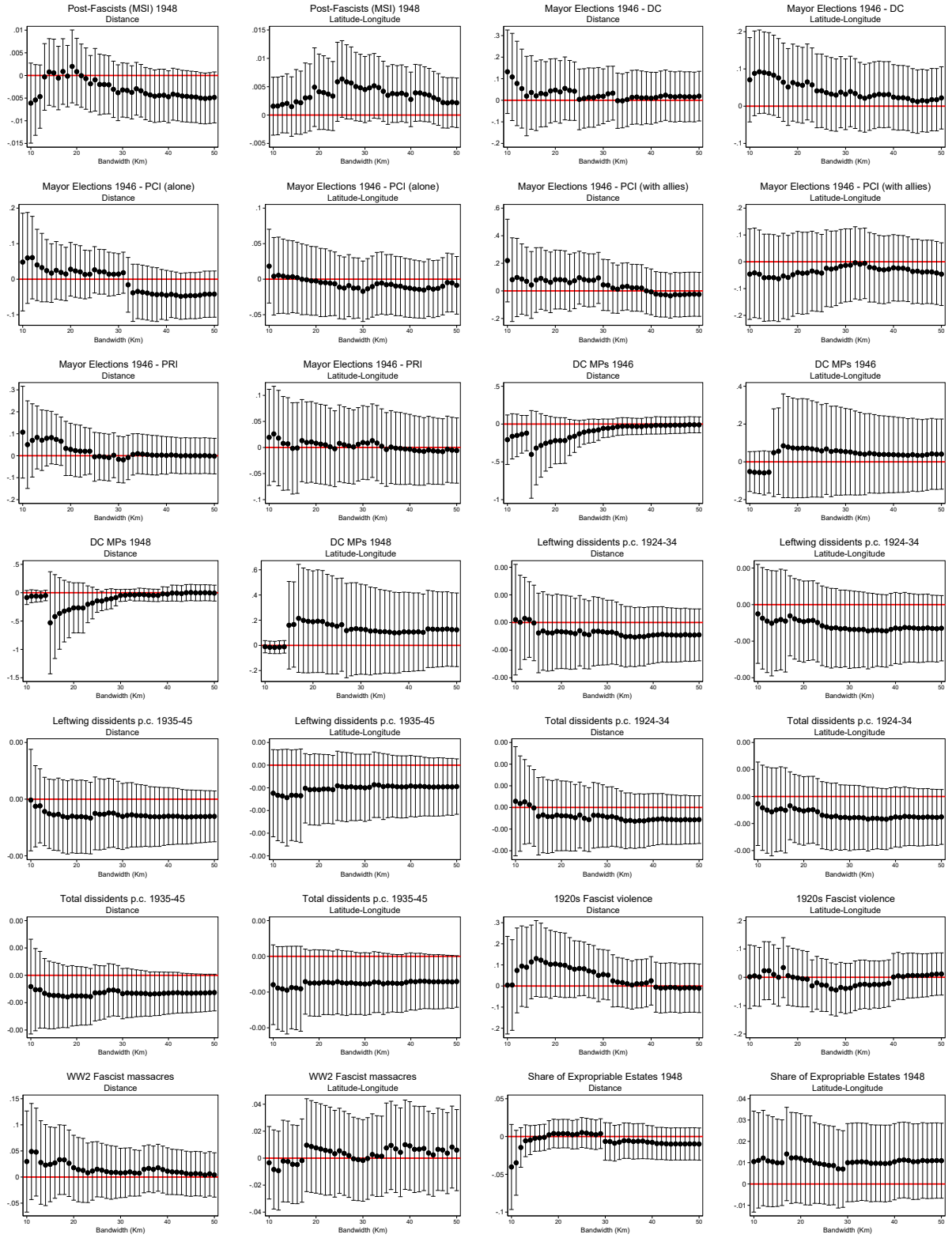


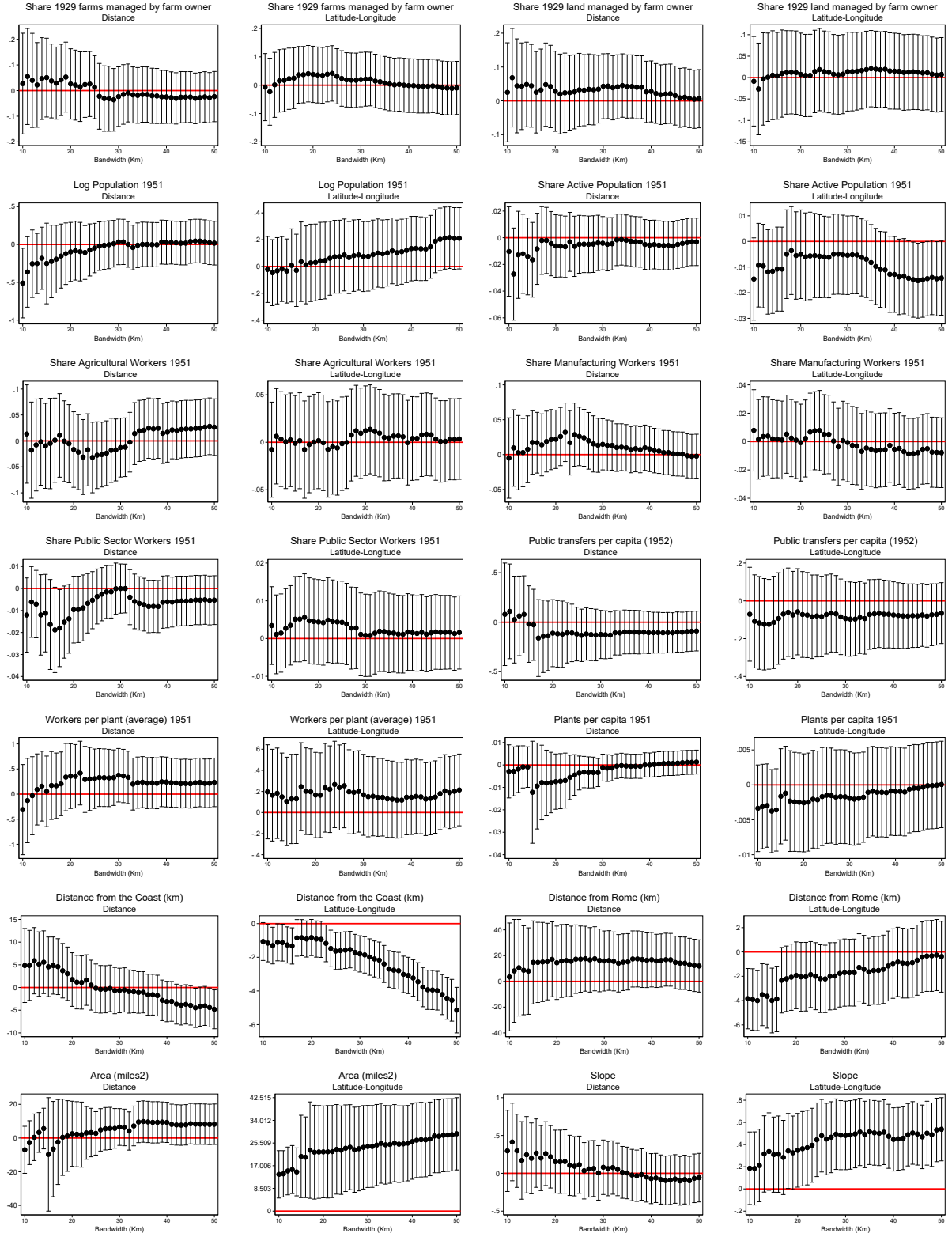
Notes: Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients β_t for each year before the reform from Equation (1). The omitted category is the β of 1948 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta 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.

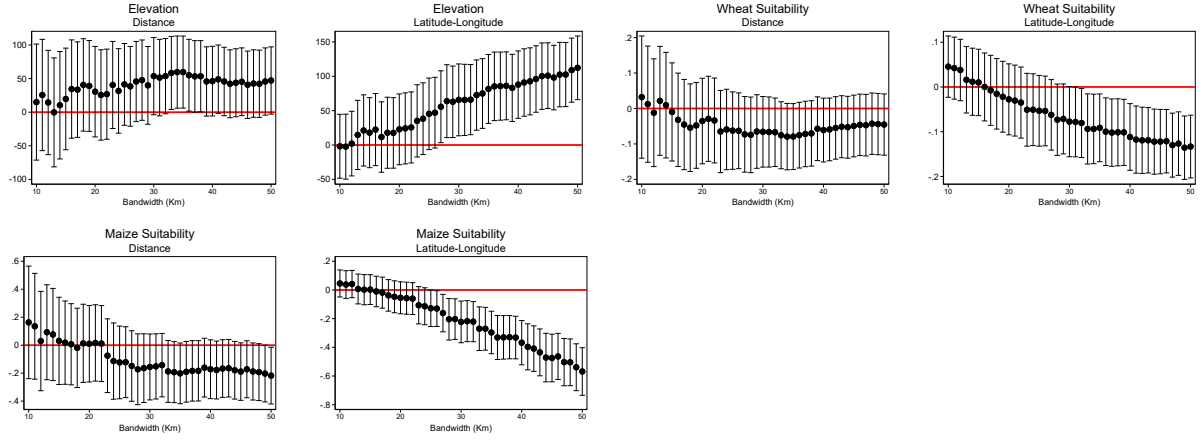
C.3 Balance – Northern Italy

Figure C.3: Balance Northern Italy









Notes: The graphs report the coefficients of separate regressions of the RD specification in Equation (2). We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals from the reform borders of Delta Padano and Maremma. The subtitle *Distance* indicates the RD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RD 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.4 Continuity of the Running Variable

To our knowledge, there is no standard manipulation test for two-dimensional RD 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).

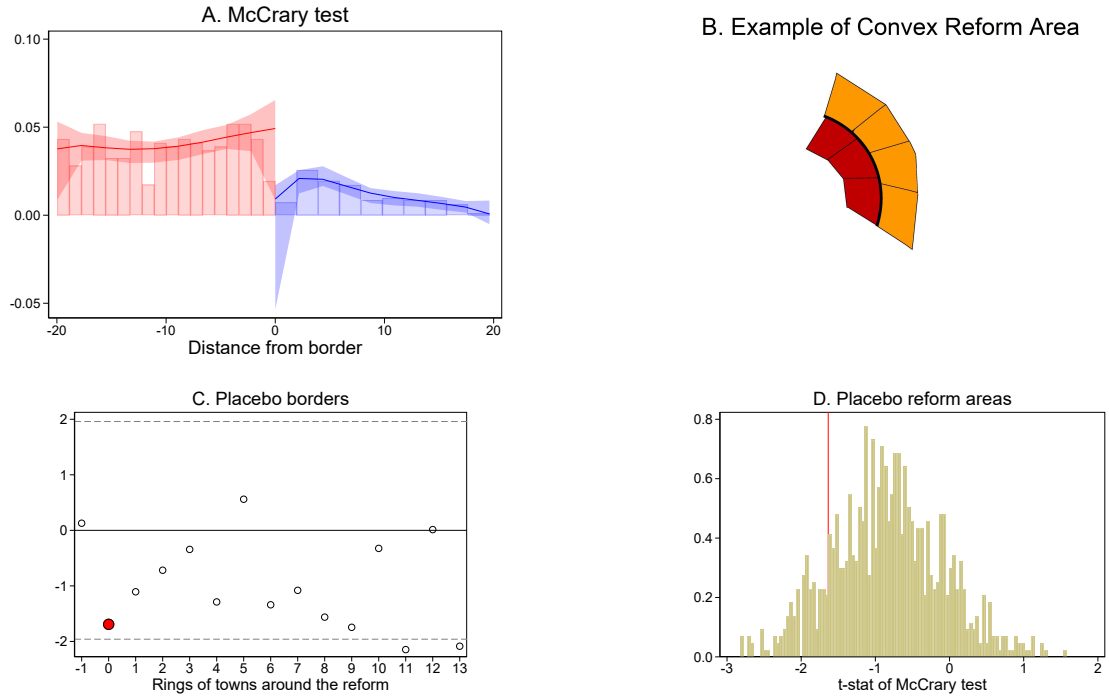
Appendix Figure C.4-Panel A presents the density approximation of the number of towns in the North (y-axis) as a function of the distance to the border (x-axis). The formal test has a t -statistic of -2.376 and rejects the null of no jump at the border. We believe that the jump in the density is a result 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 41% more area than treated towns within the same distance (roughly 15'000 vs 10'600 km²). Thus, it must be the case that there are more towns outside than inside the reform area (see Appendix Figure C.4-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.4-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.4-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 (-1.64).

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.4-Panel D. The average t -statistic is -0.84 and our observed t -statistic (-1.64) lies at the 14th 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.

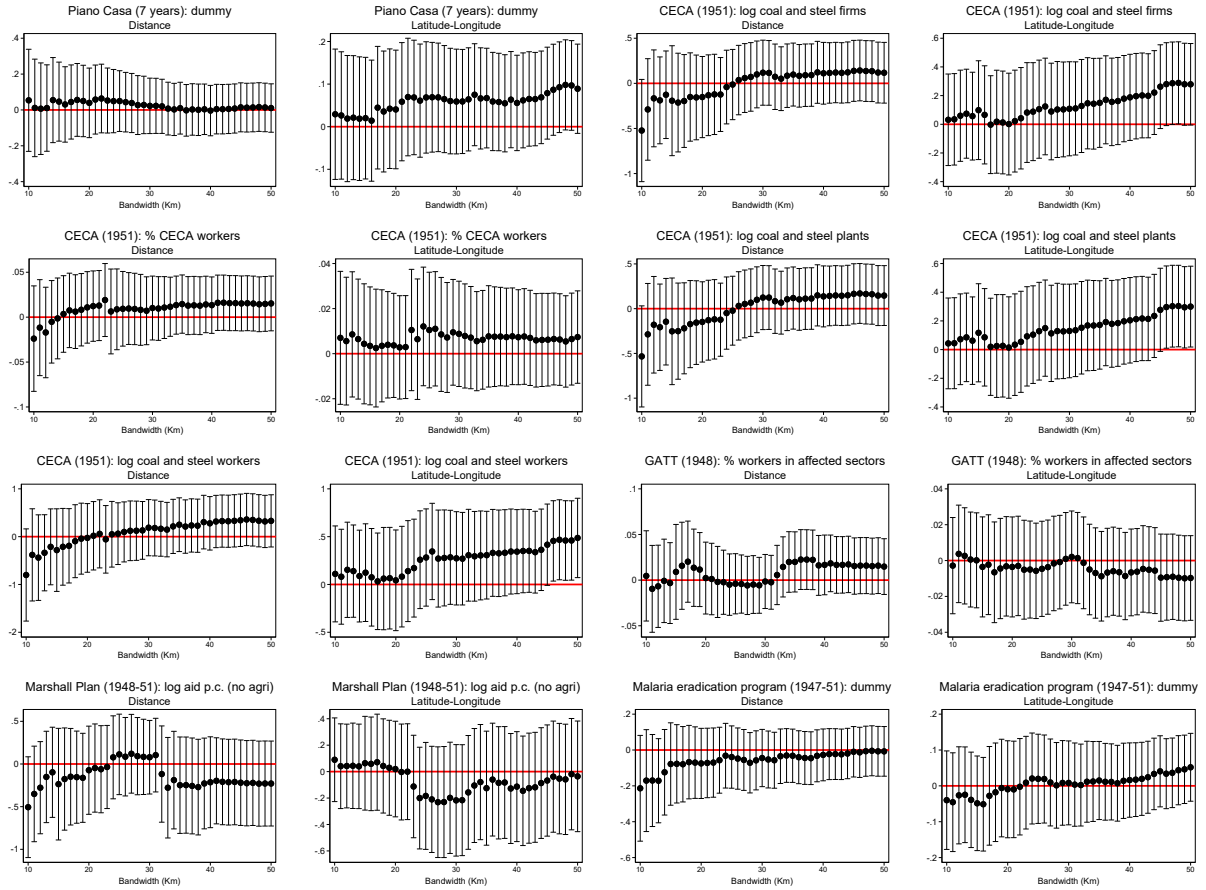
Figure C.4: McCrary test, Conjecture and Simulation Exercises



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 -2.376. 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 20 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.5 Contemporaneous Policies

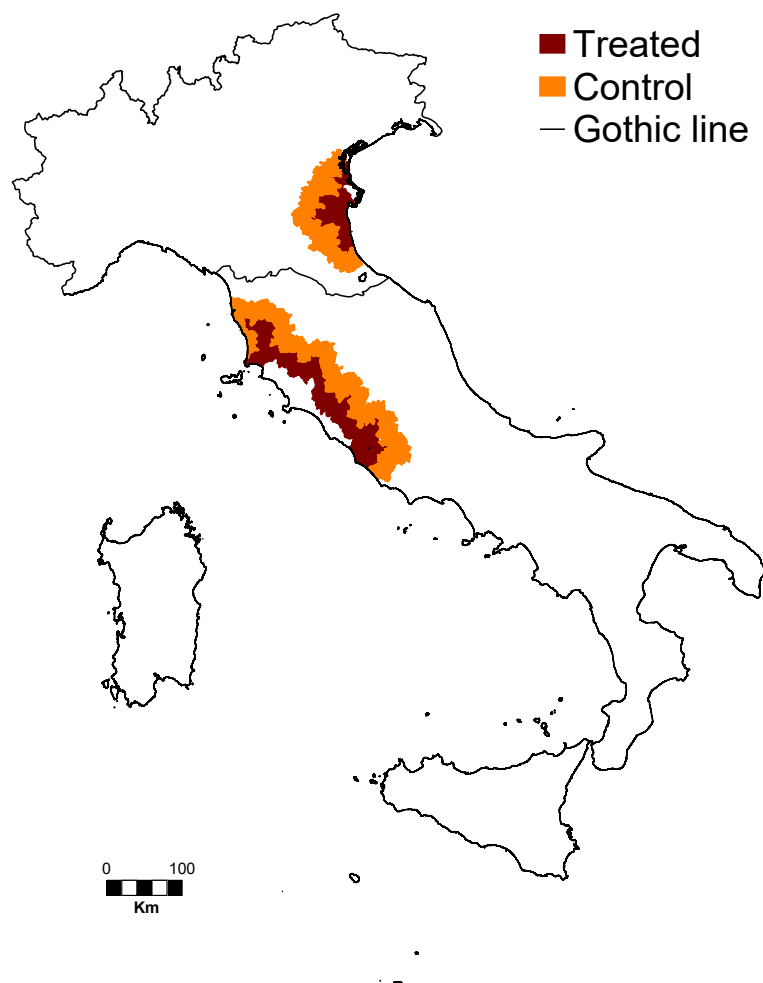
Figure C.5: Contemporaneous policies



Notes: The graphs report the coefficients of separate regressions 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 regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RD 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.6 Gothic Line

Figure C.6: Gothic Line



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

C.7 SUTVA

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

Land invasions after the reform. In the years following the reform, groups of rural workers invaded land across Italy to voice discontent and sometimes in the hope to obtain land. We digitize new data on land invasions ([Ministry of Interior, 1952](#)) in the two years following the reform and ask whether discontent was greater in control towns. Results with the two specifications are in Columns 1 and 2 of Table C.1: 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-differences specification.⁴⁶

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

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

In Table C.1 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 for DC is higher in towns with more agricultural workers: $\gamma=0.019$, s.e.=0.019).⁴⁸

Heterogeneity by exposure to the reform. Next, we explore a different source of heterogeneity. If resentment were driving our results, we would expect a higher reduction in DC vote share in those control towns where the reform is very visible. We proxy visibility with the *portion* of the town border that overlaps with the reform area. For this exercise, we restrict the sample to those towns with at least a portion of their town limit

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

⁴⁶We include towns in a 20 km bandwidth. We performed the same exercise with a more complex panel RD specification, which requires more power than the difference-in-differences. However, the specification controlling for latitude and longitude suffers from overfitting. Results for the RD controlling for a linear polynomial in distance are similar and available upon request.

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

⁴⁸In addition, in treatment areas, towns with a strong presence of agricultural workers experience a differential increase in the electoral support for DC, though this is not significant ($\delta=0.055$, s.e.=0.042).

on the border of the reform areas. Table C.1-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 RD. In the last exercise, we estimate Equation (1), but drop towns close to the reform border.⁴⁹ Similar to the previous exercise, if voters in control towns resented the reform and punished DC after 1950, we expect this effect to be larger close to the border, where voters were likely to be more aware of the reform. If this were true, dropping towns close to the border should shrink the estimated effect of the reform, because it would remove those towns where punishment against DC was stronger. In contrast, if the coefficient remains stable after dropping towns close to the border, it would be evidence that this mechanism is not important. Table C.2-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.2, we estimate the same regressions after dropping towns that are within 1.5, 2 and 2.5 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.

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

Table C.1: Tests of resentment.

	Land Invasions		Christian Democrats	
	(1)	(2)	(3)	(4)
Treatment	-0.014 [0.107]	0.083 [0.074]		
Treatment $\times$ Post			-0.007 [0.025]	0.029** [0.015]
Share agricultural workers $\times$ Post			0.019 [0.019]	
Share agricultural workers $\times$ Treatment $\times$ Post			0.055 [0.042]	
Share of town limit on reform border $\times$ Post				0.010 [0.025]
Share of town limit on reform border $\times$ Treatment $\times$ Post				-0.014 [0.048]
Mean Y Control	0.06	0.05	0.35	0.31
Number of Towns	417	652	412	161
Bandwidth	20 Km	35 Km	20 Km	20 Km
Specification	Distance	Lat-Long	DiD	DiD
Observations	417	652	1645	644

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 RD controlling for a linear polynomial in distance. Column 2: two-dimensional RD 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

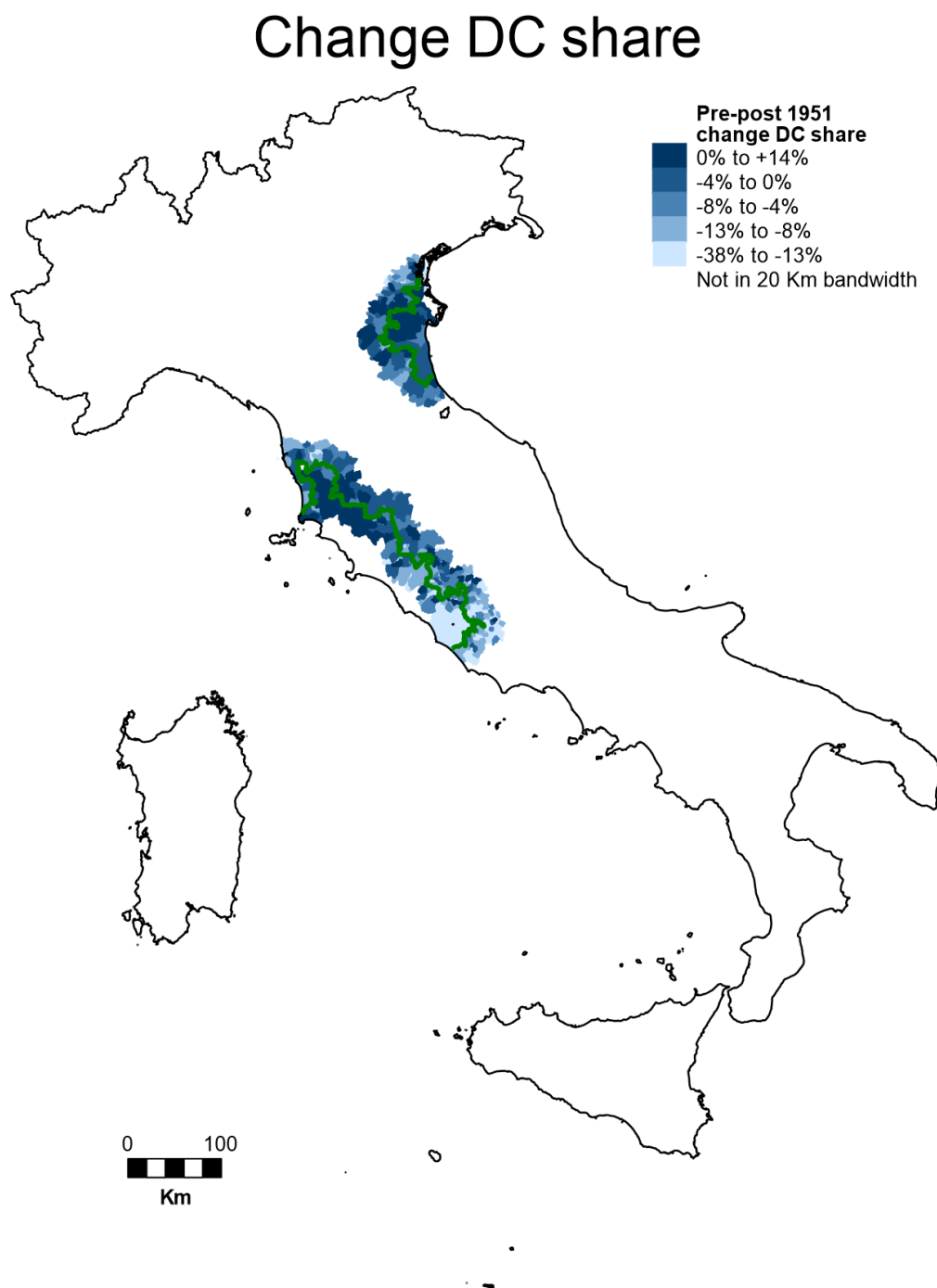
Table C.2: Donut Panel RDD. Effect on Christian Democrats vote share.

	Distance				Latitude-Longitude			
	(1) All	(2) Donut: 1.5 km	(3) Donut: 2 km	(4) Donut: 2.5 km	(5) All	(6) Donut: 1.5 km	(7) Donut: 2 km	(8) Donut: 2.5 km
Treatment x 1950s	0.046*** [0.015]	0.037*** [0.013]	0.034** [0.014]	0.044*** [0.015]	0.020** [0.008]	0.016** [0.007]	0.015** [0.007]	0.018** [0.008]
Treatment x 1960s	0.052*** [0.017]	0.046*** [0.017]	0.043** [0.017]	0.051*** [0.018]	0.028*** [0.009]	0.025*** [0.009]	0.025*** [0.009]	0.027*** [0.009]
Treatment x 1970s	0.054*** [0.018]	0.052*** [0.018]	0.049*** [0.019]	0.050** [0.020]	0.037*** [0.010]	0.036*** [0.010]	0.036*** [0.010]	0.035*** [0.010]
Treatment x 1980s	0.050*** [0.019]	0.053*** [0.019]	0.050** [0.020]	0.054** [0.022]	0.040*** [0.011]	0.041*** [0.012]	0.041*** [0.012]	0.042*** [0.012]
Mean Y Control Group	0.36	0.36	0.36	0.36	0.36	0.36	0.36	0.37
Number of Towns	417	398	388	369	652	633	623	604
Observations	4972	4749	4629	4401	7756	7533	7413	7185

Notes: The Table reports coefficients β_t from the panel RD Equation (1), which controls for year $\times$ reform area and town fixed effects. We include the 1992 election in the 1980s decade. The omitted category is the β of the elections of 1946 and 1948. The dependent variable is Christian Democrat (DC) vote share. Columns 1-4 control for a linear polynomial in distance, columns 5-8 control for a quadratic polynomial in latitude and longitude. Units of observation are town-years. Column 1 and 5: baseline; the sample consists of all towns within 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” RD; 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.8 Electoral effect of the reform: visualization on a map

Figure C.7: 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.9 RD-robust inference

Table C.3: Robust RDD inference.

Dep. Var.:	Divorce referendum (1)	<i>Casse Mutue</i>			
		Revenues p.c. (2)	Expenditure p.c. (3)	Votes p.c. (4)	Votes <i>Coldiretti</i> p.c. (5)
Treatment	0.0294 [-0.0171, 0.1244]	158.0 [-28.1, 381.1]*	156.4 [-5.3, 333.1]*	0.0367 [0.0086, 0.0849]**	0.0329 [0.0105, 0.0737]***
Robust p-value	0.137	0.091	0.058	0.016	0.009
Total obs.	6959	1660	1660	1788	1780
Effective obs.	319	261	262	220	204
Covariates	Reform FEs	Reform FEs	Reform FEs	Reform FEs	Reform FEs
Bandwidth	14.9	11.8	11.9	12.6	11.6

Notes: The Table reports coefficients β 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: vote share against divorce in referendum (col. 1); *Casse Mutue* per capita revenues (col. 2), expenses (col. 3) and per capita votes (col. 4); *Coldiretti* per capita votes (col. 5). Units of observation are towns. The sample (effective observations) consists of all towns close to the reform borders of Delta Padano and Maremma, within a bandwidth specified at the bottom of the table. Total observations are the number of municipalities for which data is available and are used to compute the optimal bandwidth. The displayed coefficients are the conventional local-polynomial point estimates; square brackets below report the 95% robust bias-corrected confidence intervals (Calonico et al., 2014), and the *Robust p-value* row reports the corresponding robust bias-corrected p-value. Stars indicate significance according to the robust p-value: *p<0.1, **p<0.05, ***p<0.01.

C.10 Robustness to alternative specifications

Table C.4: Robustness to alternative specifications. Distance.

	Distance						
	(1) Baseline	(2) No prov. seats	(3) Dist ²	(4) Reform area	(5) Elec. dist. FEs	(6) Segment FEs	(7) Prov. FE
A. DC vote share							
Treatment × 1946	-0.013 [0.017]	-0.015 [0.017]	-0.034 [0.029]	-0.015 [0.017]	-0.024 [0.015]	-0.009 [0.014]	-0.021 [0.016]
Treatment × Post	0.044*** [0.017]	0.043** [0.017]	0.041 [0.028]	0.045*** [0.017]	0.040** [0.016]	0.037** [0.016]	0.045*** [0.016]
N	4972	4876	4972	4972	4972	4972	4972
R ²	0.383	0.381	0.383	0.384	0.440	0.448	0.498
No. param.	445	437	449	449	501	533	588
d.o.f.	4527	4439	4523	4523	4471	4439	4384
B. <i>Casse Mutue</i> revenues p.c.							
Treatment	122.536** [60.319]	122.158** [60.264]	228.147** [88.387]	116.508** [58.739]	150.631** [61.367]	125.660** [61.751]	135.609** [60.540]
N	415	407	415	415	415	415	415
R ²	0.104	0.113	0.110	0.114	0.151	0.161	0.220
No. param.	5	5	7	7	11	13	19
d.o.f.	410	402	408	408	404	402	396
C. <i>Casse Mutue</i> expenditures p.c.							
Treatment	125.202** [55.283]	124.931** [55.484]	207.318*** [79.184]	120.840** [54.582]	125.375** [54.564]	121.056** [55.484]	110.826** [52.709]
N	415	407	415	415	415	415	415
R ²	0.095	0.103	0.101	0.098	0.117	0.156	0.192
No. param.	5	5	7	7	11	13	19
d.o.f.	410	402	408	408	404	402	396
D. <i>Casse Mutue</i> votes p.c.							
Treatment	0.012 [0.008]	0.012 [0.008]	0.036*** [0.014]	0.009 [0.007]	0.017** [0.008]	0.014** [0.007]	0.015** [0.007]
N	1216	1183	1216	1216	1216	1216	1216
R ²	0.120	0.124	0.135	0.168	0.263	0.352	0.363
No. param.	15	15	17	17	44	62	79
d.o.f.	1201	1168	1199	1199	1172	1154	1137
E. <i>Coldiretti</i> votes p.c.							
Treatment	0.010 [0.006]	0.010 [0.006]	0.030*** [0.011]	0.007 [0.006]	0.014** [0.006]	0.011** [0.006]	0.012** [0.006]
N	1188	1155	1188	1188	1188	1188	1188
R ²	0.101	0.102	0.119	0.169	0.275	0.361	0.404
No. param.	15	15	17	17	44	62	80
d.o.f.	1173	1140	1171	1171	1144	1126	1108
F. Public sector employment							
Treatment × 1936	0.006 [0.005]	0.006 [0.005]	0.012 [0.009]	0.006 [0.006]	0.006 [0.005]	0.004 [0.005]	0.007 [0.005]
Treatment × Post	0.015** [0.007]	0.013** [0.006]	0.016 [0.011]	0.016** [0.007]	0.019*** [0.006]	0.011* [0.006]	0.019*** [0.007]
N	2502	2454	2502	2502	2502	2502	2502
R ²	0.559	0.573	0.560	0.562	0.634	0.660	0.664
No. param.	433	425	437	437	462	473	502
d.o.f.	2069	2029	2065	2065	2040	2029	2000

Notes: The Table reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area × year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area × year fixed effects. Panel A: The omitted category is the β of the elections of 1948. Panel F: omitted category is the β of 1951. Col. 1: baseline specification controlling for a linear polynomial in distance. Col. 2: drop 10 provincial seats (including Rome). Col. 3: control for 2nd order polynomial in distance interacted with decades and reform area on both side of the border. Col. 4: controls for a linear polynomial in distance interacting it with reform area identifiers. Col. 5: controls for electoral districts × year fixed effects. Col. 6: control for ten segments × year fixed effects (see Figure C.8). 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 heteroskedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table C.5: Robustness to alternative specifications. Latitude-Longitude.

	Latitude-Longitude						
	(1) Baseline	(2) No prov. seats	(3) Linear	(4) No reform area	(5) Elec. dist. FEs	(6) Segment FEs	(7) Prov. FE
A. DC vote share							
Treatment $\times$ 1946	-0.002 [0.008]	-0.003 [0.008]	0.005 [0.007]	-0.000 [0.008]	-0.007 [0.008]	0.008 [0.009]	0.002 [0.012]
Treatment $\times$ Post	0.032*** [0.009]	0.031*** [0.009]	0.056*** [0.008]	0.038*** [0.009]	0.029*** [0.010]	0.026** [0.010]	0.020* [0.011]
N	7756	7564	7756	7756	7756	7756	7756
R ²	0.429	0.428	0.422	0.427	0.483	0.477	0.539
No. param.	692	676	684	679	727	756	811
d.o.f.	7064	6888	7072	7077	7029	7000	6945
B. Casse Mutue revenues p.c.							
Treatment	115.201*** [36.120]	124.258*** [36.248]	160.067*** [30.431]	150.590*** [32.793]	124.038*** [35.647]	122.395*** [36.819]	90.062** [38.557]
N	650	634	650	650	650	650	650
R ²	0.219	0.230	0.157	0.121	0.254	0.237	0.305
No. param.	12	12	7	6	16	19	29
d.o.f.	638	622	643	644	634	631	621
C. Casse Mutue expenditures p.c.							
Treatment	96.407*** [32.531]	101.386*** [32.743]	146.539*** [26.144]	151.948*** [27.616]	105.534*** [33.272]	104.751*** [34.647]	77.163** [34.297]
N	650	634	650	650	650	650	650
R ²	0.199	0.210	0.160	0.138	0.219	0.228	0.289
No. param.	12	13	7	6	18	19	29
d.o.f.	638	621	643	644	632	631	621
D. Casse Mutue votes p.c.							
Treatment	0.010*** [0.004]	0.011*** [0.004]	0.014*** [0.004]	0.015*** [0.004]	0.011*** [0.004]	0.009** [0.004]	0.007 [0.004]
N	1922	1855	1922	1922	1922	1922	1922
R ²	0.262	0.276	0.166	0.150	0.302	0.326	0.381
No. param.	23	23	17	16	59	68	102
d.o.f.	1899	1832	1905	1906	1863	1854	1820
E. Coldiretti votes p.c.							
Treatment	0.009*** [0.003]	0.009*** [0.003]	0.009*** [0.003]	0.011*** [0.003]	0.009*** [0.003]	0.007** [0.003]	0.003 [0.003]
N	1868	1802	1868	1868	1868	1868	1868
R ²	0.295	0.308	0.160	0.128	0.336	0.361	0.420
No. param.	22	23	17	16	56	68	103
d.o.f.	1846	1779	1851	1852	1812	1800	1765
F. Public sector employment							
Treatment $\times$ 1936	0.005 [0.004]	0.005 [0.004]	0.004 [0.004]	0.005 [0.004]	0.001 [0.004]	0.003 [0.003]	0.003 [0.005]
Treatment $\times$ Post	0.018*** [0.004]	0.018*** [0.004]	0.013*** [0.004]	0.019*** [0.004]	0.014*** [0.004]	0.010*** [0.004]	0.014*** [0.005]
N	3912	3816	3912	3912	3912	3912	3912
R ²	0.569	0.590	0.561	0.563	0.616	0.629	0.646
No. param.	681	664	672	672	717	714	739
d.o.f.	3231	3152	3240	3240	3195	3198	3173

Notes: The Table reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area $\times$ year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area $\times$ year fixed effects. Panel A: The omitted category is the β of the elections of 1948. Panel F: omitted category is the β of 1951. Col. 1: baseline specification controlling for a quadratic polynomial in latitude and longitude, interacted with reform area identifiers. Col. 2 drop 10 provincial seats (including Rome). Col. 3: control for 1st order polynomial in latitude and longitude interacted with decades and reform area on both side of the border. Col. 4: controls for a quadratic polynomial in latitude and longitude without interacting it with reform area identifiers. Col. 5: controls for electoral districts $\times$ year fixed effects. Col. 6: control for ten segments $\times$ year fixed effects (see Figure C.8). 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 heteroskedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

C.11 Robustness to controlling for predetermined characteristics

Table C.6: Robustness to controlling for predetermined characteristics. Distance.

	Distance										
	(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
A. DC vote share											
Treatment $\times$ 1946	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]	-0.013 [0.017]
Treatment $\times$ Post	0.044*** [0.017]	0.046*** [0.017]	0.046*** [0.017]	0.043** [0.017]	0.042*** [0.016]	0.044*** [0.017]	0.045*** [0.017]	0.046*** [0.017]	0.044*** [0.017]	0.044*** [0.017]	0.047*** [0.016]
B. Casse Mutue revenues p.c.											
Treatment	122.536** [60.319]	99.441* [59.765]	124.857** [59.789]	117.598* [61.036]	123.046** [60.316]	125.640** [59.744]	119.493* [60.803]	118.516* [60.769]	120.453** [60.636]	123.066** [60.712]	102.375* [57.124]
C. Casse Mutue expenses p.c.											
Treatment	125.202** [55.283]	93.211* [50.760]	128.113** [54.767]	123.310** [56.358]	118.642** [53.502]	127.026** [55.221]	124.787** [55.365]	125.458** [55.404]	125.339** [55.449]	125.253** [55.393]	96.348** [48.469]
D. Casse Mutue votes p.c.											
Treatment	0.012 [0.008]	0.011 [0.008]	0.012 [0.008]	0.013* [0.008]	0.015* [0.008]	0.021*** [0.008]	0.010 [0.008]	0.011 [0.008]	0.012 [0.008]	0.012* [0.007]	0.018** [0.007]
E. Coldiretti votes p.c.											
Treatment	0.010 [0.006]	0.009 [0.006]	0.009 [0.006]	0.010 [0.006]	0.012* [0.006]	0.017*** [0.006]	0.008 [0.006]	0.008 [0.006]	0.010 [0.006]	0.010 [0.006]	0.014** [0.006]
F. Public Sector Employment											
Treatment $\times$ 1936	0.006 [0.005]	0.004 [0.005]	0.006 [0.005]	0.006 [0.005]	0.003 [0.005]	0.005 [0.006]	0.007 [0.006]	0.008 [0.006]	0.006 [0.005]	0.006 [0.006]	0.004 [0.005]
Treatment $\times$ Post	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]	0.015** [0.007]

Notes: The Table reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area $\times$ year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area $\times$ year fixed effects. Panel A: The omitted category is the β of the elections of 1948. Panel F: omitted category is the β 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 heteroskedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table C.7: 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
A. DC vote share											
Treatment $\times$ 1946	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]	-0.002 [0.008]
Treatment $\times$ Post	0.032*** [0.009]	0.033*** [0.009]	0.030*** [0.009]	0.027*** [0.010]	0.033*** [0.009]	0.033*** [0.009]	0.033*** [0.009]	0.035*** [0.009]	0.034*** [0.009]	0.034*** [0.009]	0.037*** [0.010]
B. Casse Mutue revenues p.c.											
Treatment	115.201*** [36.120]	104.037*** [35.711]	119.762*** [35.847]	103.715*** [36.851]	116.888*** [35.899]	139.159*** [37.329]	110.046*** [36.599]	115.104*** [36.380]	117.442*** [36.146]	117.537*** [36.450]	118.072*** [37.441]
C. Casse Mutue expenses p.c.											
Treatment	96.407*** [32.531]	83.647*** [30.451]	101.120*** [32.152]	92.200*** [32.426]	98.780*** [32.250]	111.175*** [33.777]	96.849*** [33.166]	99.375*** [33.258]	98.996*** [32.562]	100.090*** [33.042]	105.356*** [31.457]
D. Casse Mutue votes p.c.											
Treatment	0.010*** [0.004]	0.009** [0.004]	0.012*** [0.004]	0.009** [0.004]	0.010*** [0.004]	0.014*** [0.004]	0.011*** [0.004]	0.010*** [0.004]	0.011*** [0.004]	0.011*** [0.004]	0.012*** [0.004]
E. Coldiretti votes p.c.											
Treatment	0.009*** [0.003]	0.007** [0.003]	0.009*** [0.003]	0.008** [0.003]	0.008*** [0.003]	0.012*** [0.003]	0.008** [0.003]	0.008*** [0.003]	0.009*** [0.003]	0.009*** [0.003]	0.009*** [0.003]
F. Public Sector Employment											
Treatment $\times$ 1936	0.005 [0.004]	0.004 [0.004]	0.005 [0.004]	0.005 [0.004]	0.007* [0.004]	0.003 [0.004]	0.005 [0.004]	0.006 [0.004]	0.005 [0.004]	0.004 [0.004]	0.005 [0.004]
Treatment $\times$ Post	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]	0.018*** [0.004]

Notes: The Table reports coefficients β_t and β from alternative specifications of Equations (1) (Panels A, F) and (2) (Panels B, C, D, E). Panels A and F: regressions control for reform area $\times$ year and town fixed effects. Panel B and C: regressions control for reform area fixed effects. Panels D and E: regressions control for reform area $\times$ year fixed effects. Panel A: The omitted category is the β of the elections of 1948. Panel F: omitted category is the β of 1951. Col. 1: baseline specification controlling for a quadratic polynomial in latitude and longitude, interacted with reform area identifiers. Col. 2-10: 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 heteroskedastic robust (Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

C.12 Nearest neighbor Matching and Coarsened Exact Matching

Table C.8: Impact of reform matched sample (1 nearest neighbor), 20km bandwidth

	Change in DC vote share (1)	Casse Mutue revenues p.c. (2)	Casse Mutue expenses p.c. (3)	Casse Mutue votes p.c. (4)	Coldiretti votes p.c. (5)	Change in public sector employment (6)
Treatment	0.035*** [0.010]	147.188*** [37.555]	120.972*** [34.058]	0.013** [0.005]	0.011** [0.004]	0.014*** [0.004]
N	180	179	179	138	138	180
Mean Y Control Group	-0.06	379.27	292.30	0.04	0.03	0.02
Reform area fe	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The Table reports coefficients β estimated within the 20 km bandwidth on the matched sample obtained with nearest neighbor matching. In columns 1 and 6, β is a difference-in-differences estimate, in columns 2 to 5, β is a matched cross-sectional comparison. The variables used in the matching procedure are: Republican Party vote share in 1948, share of active population in 1951, distance from the coast and to Rome, area of the municipality, slope, elevation, wheat suitability, and maize suitability (same variables used in Tables C.6 and C.7). All regressions control for reform area fixed effects. Dependent variables: change in DC vote share (before-after the reform, col. 1); *Casse Mutue* per capita revenues (col. 2), expenses (col. 3) and per capita votes (col. 4); *Coldiretti* per capita votes (col. 5); change in public sector employment (before-after the reform, col. 6). Units of observation are towns. The sample consists of matched pairs of towns within a 20 km bandwidth to the reform borders of Delta Padano and Maremma. Robust standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

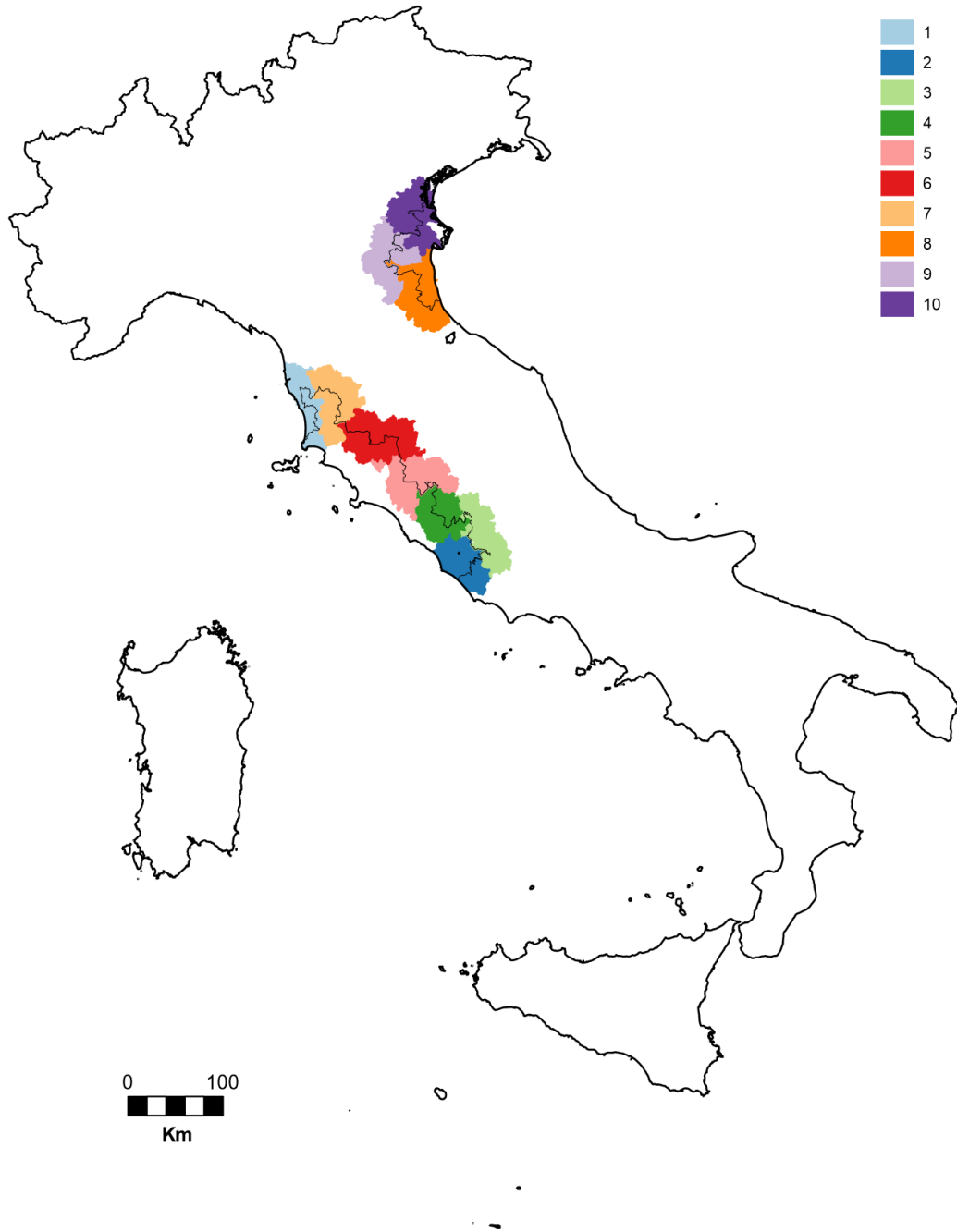
Table C.9: Impact of reform CEM, 20km bandwidth

	Change in DC vote share (1)	Casse Mutue revenues p.c. (2)	Casse Mutue expenses p.c. (3)	Casse Mutue votes p.c. (4)	Coldiretti votes p.c. (5)	Change in public sector employment (6)
Treatment	0.052*** [0.010]	184.196*** [39.385]	167.249*** [33.054]	0.017*** [0.006]	0.012** [0.005]	0.016*** [0.005]
N	267	267	267	218	217	267
Mean Y Control Group	-0.02	435.82	311.86	0.04	0.03	0.02
Reform area fe	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The Table reports coefficients β estimated within the 20 km bandwidth using Coarsened Exact Matching (CEM). In columns 1 and 6, β is a difference-in-differences estimate, in columns 2 to 5, β is a matched cross-sectional comparison. The variables used in the matching procedure are: Republican Party vote share in 1948, share of active population in 1951, distance from the coast and to Rome, area of the municipality, slope, elevation, wheat suitability, and maize suitability (same variables used in Tables C.6 and C.7). All regressions control for reform area fixed effects. Dependent variables: change in DC vote share (before-after the reform, col. 1); *Casse Mutue* per capita revenues (col. 2), expenses (col. 3) and per capita votes (col. 4); *Coldiretti* per capita votes (col. 5); change in public sector employment (before-after the reform, col. 6). Units of observation are towns. The sample consists of towns within a 20 km bandwidth to the reform borders of Delta Padano and Maremma. Robust standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

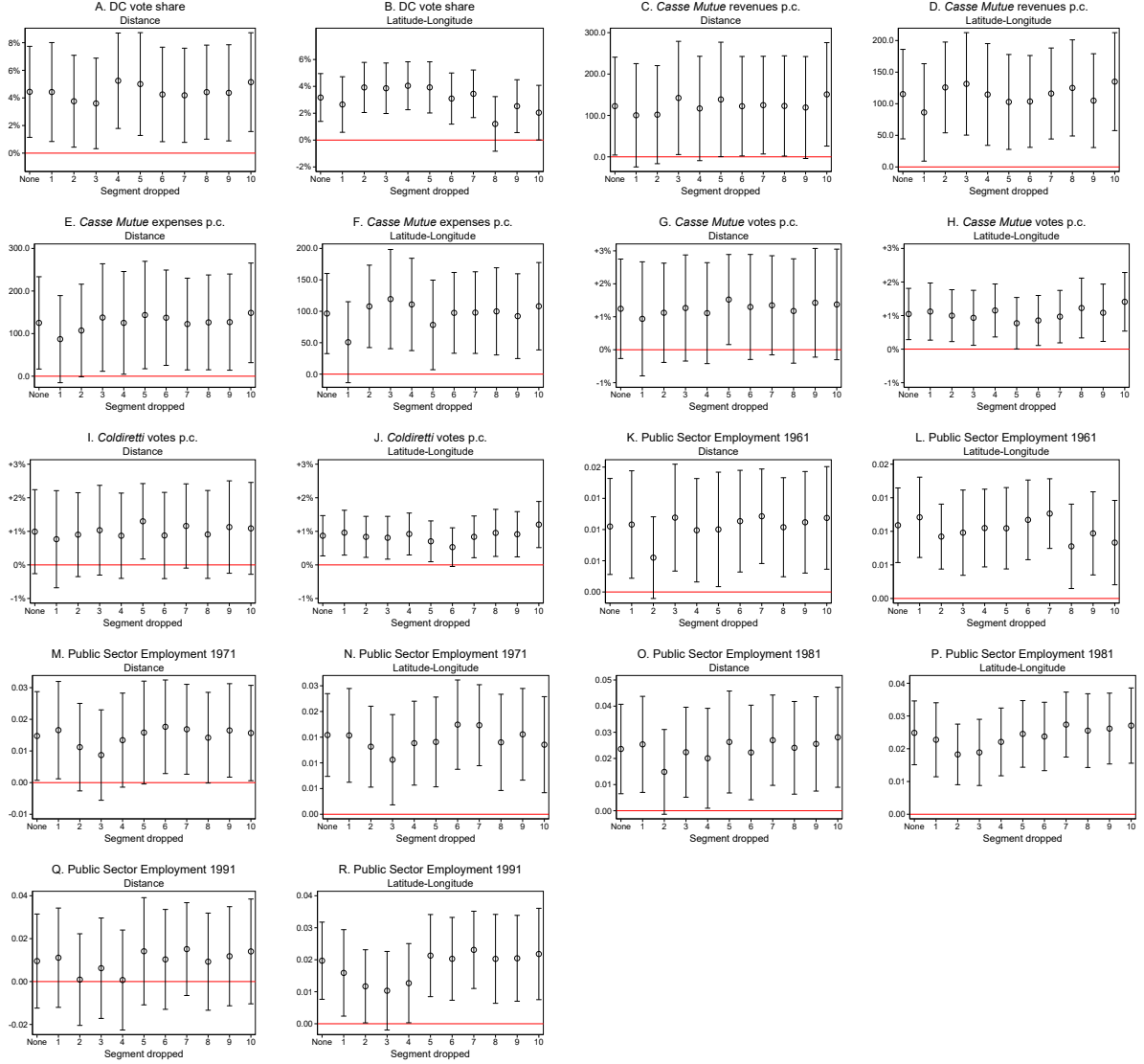
C.13 Robustness to Dropping Portions of the Reform Border

Figure C.8: 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.9.

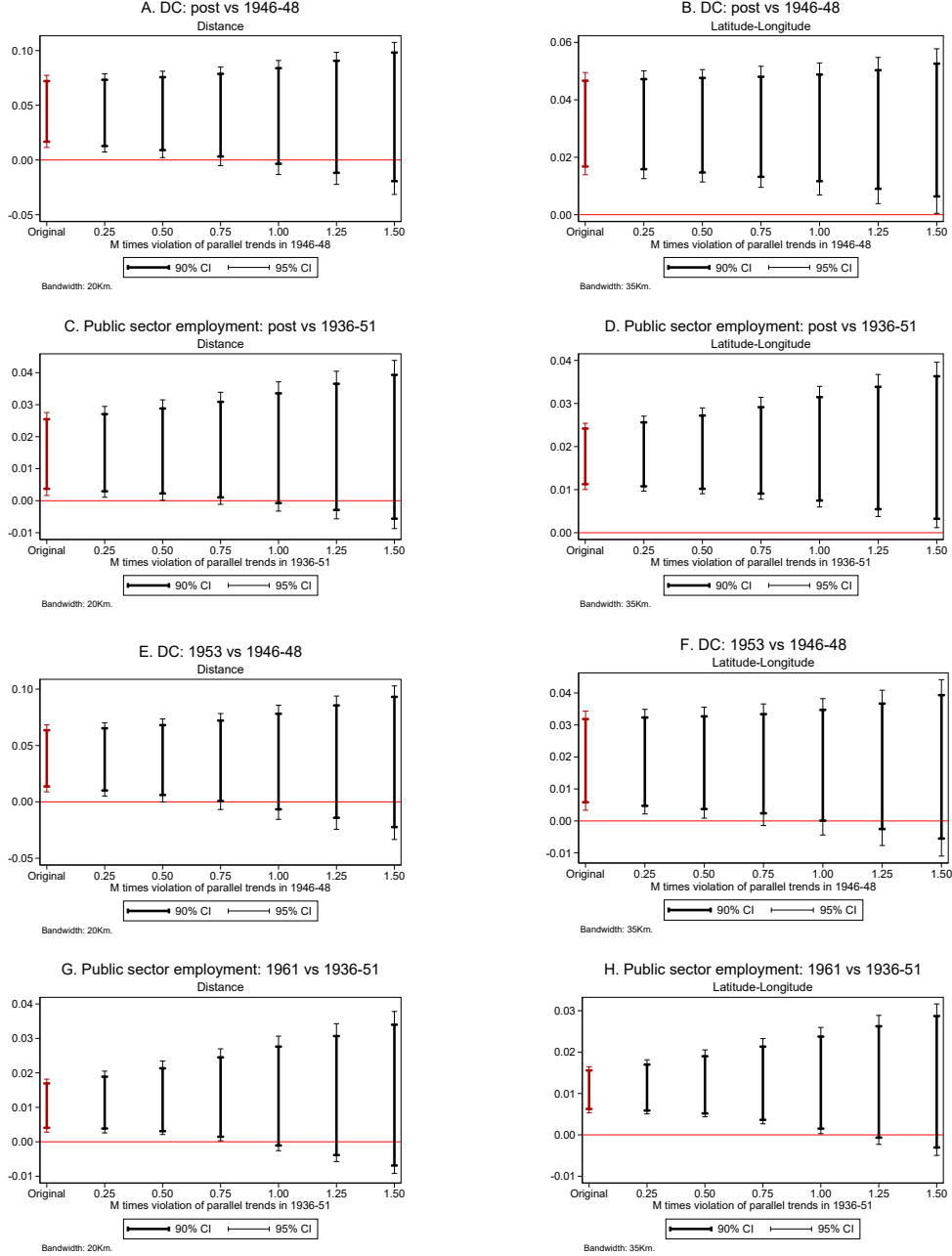
Figure C.9: Treatment coefficients when dropping portions of the sample.



Notes: The Figure reports coefficients β_t and β 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.8. 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 heteroskedasticity robust (Panels C to F) and plot 95% confidence intervals as bars around the coefficients.

C.14 *Honest pre-trends*

Figure C.10: Robustness to violations of the pre-trends assumption.



Notes: The Figures reports bounds for the coefficients β_t from Equation (1) calculated with the method of [Rambachan and Roth \(2023\)](#) to account for possible violations of the parallel trends assumption. Panels A, B, and E and F: dependent variable is DC vote share. Panels C, D and G and H: dependent variable is public sector employment. Panels A, B and C and D: bounds for treatment effect for the full “post” period (1953-1992 for vote share, 1961-1991 for public sector employment). Panels E, F and G and H: bounds for the treatment effect of the first post period (1953 for vote share, 1961 for public sector employment). 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 allow non parallel trends with the Relative Magnitude method (RM) and report bounds for deviations from the parallel trend assumption that are equal to M times observed pre-trends in 1946-48 (vote shares) and 1936-51 (public sector employment), with $M \in \{0.25, 0.50, 0.75, 1.00, 1.25, 1.50\}$. We also report in red the bounds when the parallel trends assumption holds ($M = 0$). Thick lines show 95% confidence bounds, thin lines 90% confidence bounds.

C.15 Instrumental Variable

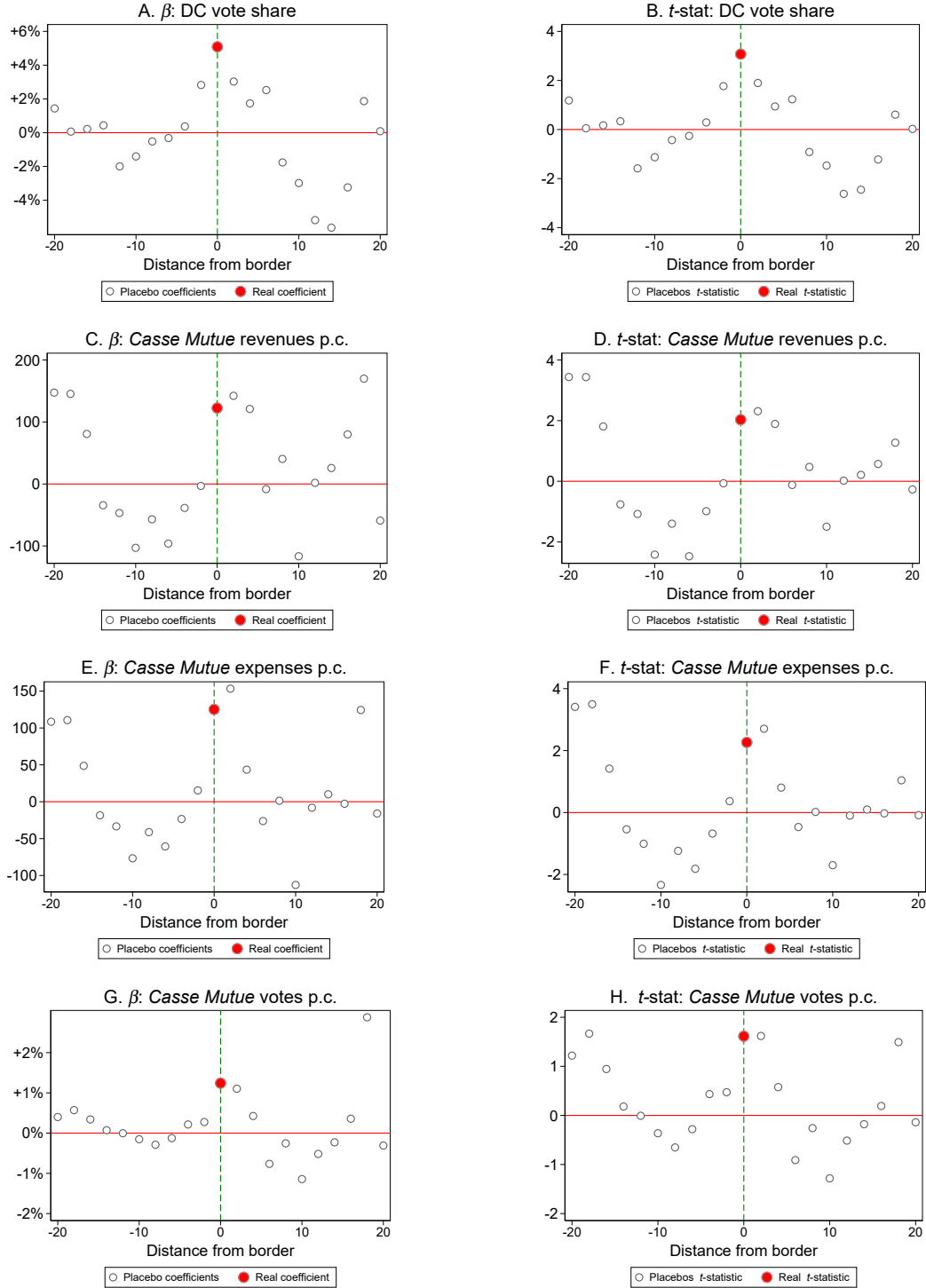
Table C.10: Impact of reform. Instrumental variable.

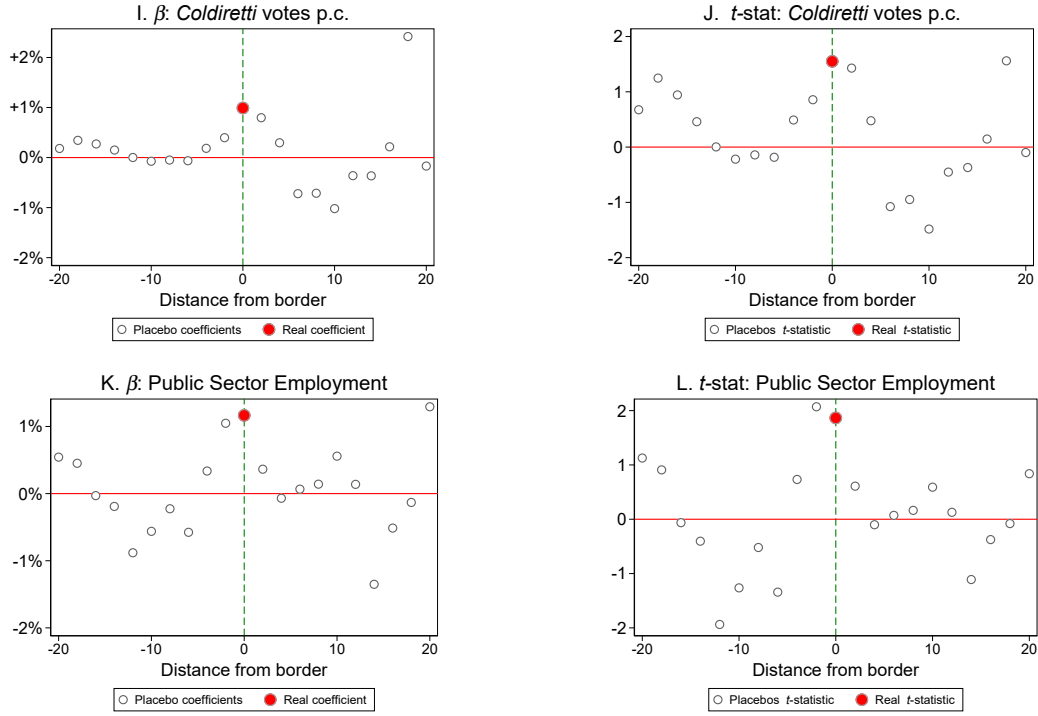
	Distance (20 Km)			Latitude-Longitude (35 Km)		
	(1) First Stage	(2) Reduced form	(3) IV	(4) First Stage	(5) Reduced form	(6) IV
First stage (cross-section)						
Treatment	0.556*** [0.073]			0.697*** [0.042]		
Joint F-test	276.38			281.09		
Observations	426			644		
A. DC vote share						
Treatment 1946		-0.013 [0.014]	-0.023 [0.024]		-0.012* [0.007]	-0.018* [0.011]
Treatment 1950s		0.033*** [0.013]	0.056** [0.022]		0.018*** [0.007]	0.026** [0.010]
Treatment 1960s		0.043*** [0.015]	0.075*** [0.027]		0.029*** [0.009]	0.042*** [0.013]
Treatment 1970s		0.049*** [0.015]	0.086*** [0.027]		0.038*** [0.009]	0.056*** [0.014]
Treatment 1980s		0.032* [0.017]	0.056* [0.029]		0.039*** [0.011]	0.057*** [0.016]
AR test (p-value)			0.000			0.000
B. Casse Mutue revenues p.c.						
Treatment		111.740** [50.677]	203.424** [80.793]		89.864*** [32.027]	177.248*** [42.743]
AR test (p-value)			0.002			0.005
C. Casse Mutue expenditures p.c.						
Treatment		139.880*** [44.731]	242.428*** [75.613]		83.521*** [27.968]	177.521*** [39.482]
AR test (p-value)			0.000			0.003
D. Casse Mutue votes p.c.						
Treatment		0.008 [0.006]	0.022* [0.012]		0.011*** [0.004]	0.020*** [0.005]
AR test (p-value)			0.002			0.003
E. Coldiretti votes p.c.						
Treatment		0.009** [0.005]	0.024** [0.010]		0.010*** [0.003]	0.017*** [0.004]
AR test (p-value)			0.001			0.001
F. Public Sector Employment						
Treatment 1936		0.013*** [0.005]	0.022** [0.008]		0.007** [0.004]	0.007 [0.005]
Treatment 1961		0.008** [0.004]	0.014** [0.006]		0.009*** [0.002]	0.009*** [0.003]
Treatment 1971		0.010 [0.006]	0.016 [0.010]		0.014*** [0.003]	0.015*** [0.005]
Treatment 1981		0.023*** [0.008]	0.039*** [0.014]		0.024*** [0.004]	0.026*** [0.007]
Treatment 1991		0.012 [0.010]	0.021 [0.017]		0.019*** [0.005]	0.017** [0.008]
AR test (p-value)			0.003			0.000

Notes: First stage estimated with Equation (2). Reduced form and IV estimated with Equations (1) (Panels A, D, E, F) and (2) (Panels B, C). The borders of “Zone B” in the *legge di riforma agraria* were proposed and never-enacted, but formed the basis of the actual reform. We instrument actual treatment with proposed treatment (an indicator for the “Zone B”). In cols 1-3 the endogenous regressors are actual treatment, distance to the actual border, and their interaction, just-identified by proposed treatment, distance to the proposed border, and their interaction; in cols 4-6 the only endogenous regressor is actual treatment, instrumented by proposed treatment. The first stage reported at the top is the town-level cross-sectional first stage for actual treatment, predicted by proposed treatment. AR test (p-value) reports the p-value of the Anderson–Rubin weak-identification-robust test of the null that the endogenous coefficients are jointly zero (cluster-robust). Units of observation: town-years (Panels A, D, E, F) or towns (first stage and 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 robust (first stage and Panels B, C) in parentheses. *p<0.1, **p<0.05, ***p<0.01.

C.16 Placebo Borders

Figure C.11: 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 β . Panels B, D, F, H, J and L: t -statistics calculated from standard errors clustered at the town level (Panels B, H, J and L) or heteroskedastic 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.17 Spatial Standard Errors

Table C.11: Standard error robust to temporal and spatial correlation.

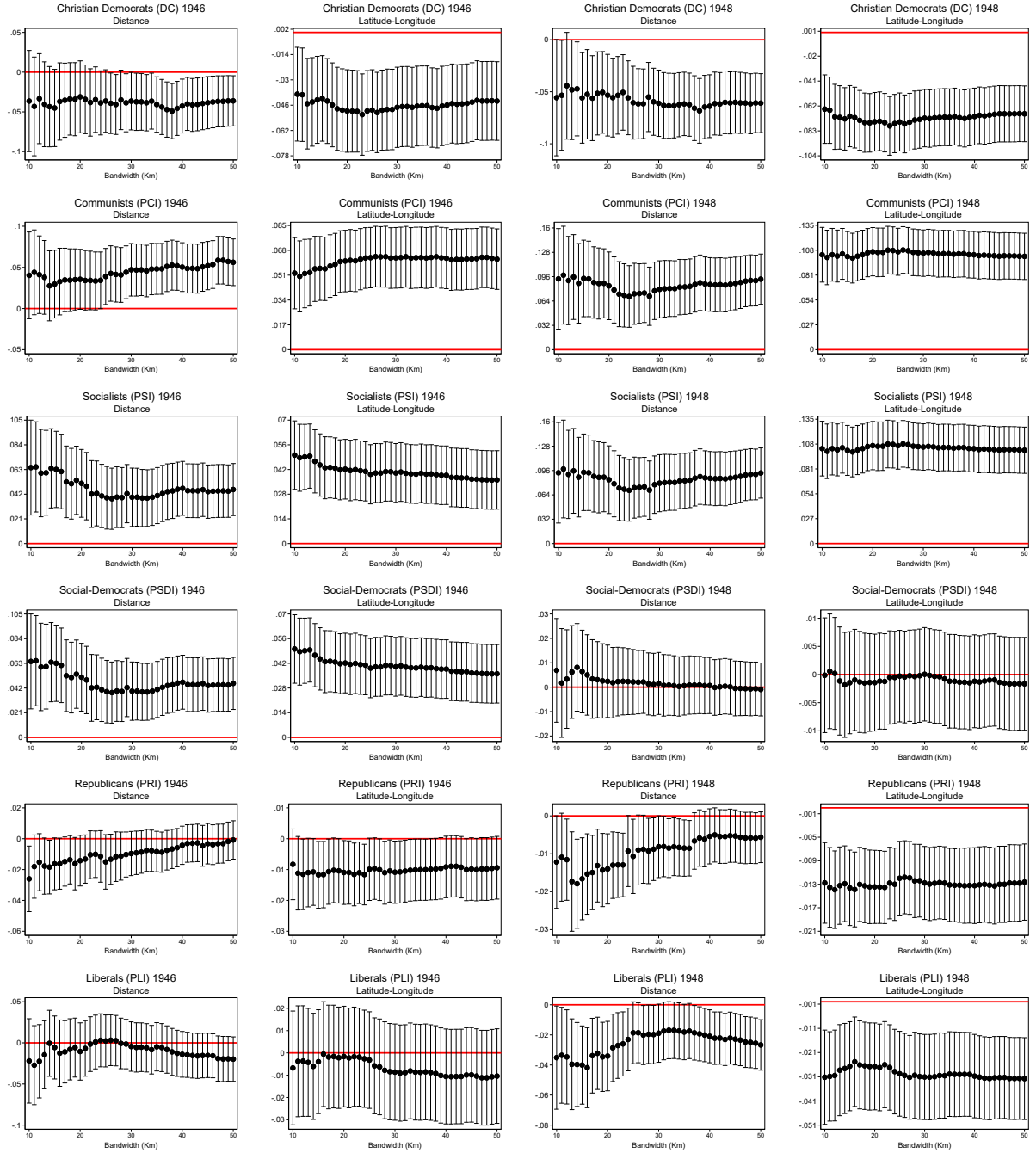
	Casse Mutue					
Dep. var.:	DC (1)	Revenues p.c. (2)	Expenditure p.c. (3)	Votes p.c. (4)	Coldiretti votes p.c. (5)	Public sector employment (6)
Panel A. Distance						
Treatment × Post	0.044	122.536	125.202	0.012	0.010	0.015
Cluster: town	(0.017)***	(60.319)**	(55.283)**	(0.008)	(0.006)	(0.007)**
Conley s.e.: cutoff = 5 km	(0.017)***	(59.805)**	(55.645)**	(0.008)	(0.006)	(0.007)**
Conley s.e.: cutoff = 10 km	(0.017)***	(60.082)**	(56.612)**	(0.008)	(0.006)	(0.007)**
Conley s.e.: cutoff = 25 km	(0.019)**	(59.414)**	(58.197)**	(0.008)	(0.006)	(0.007)**
Conley s.e.: cutoff = 50 km	(0.019)**	(53.997)**	(56.000)**	(0.007)*	(0.005)*	(0.007)**
Conley s.e.: cutoff = 100 km	(0.016)***	(45.234)***	(48.926)**	(0.006)**	(0.004)**	(0.009)
Mean Y Control Group	0.36	452.16	329.59	0.04	0.03	0.06
Number of Towns	417	415	415	331	330	417
Observations	4972	415	415	1216	1188	2502
Panel B. Latitude-Longitude						
Treatment × Post	0.032	115.201	96.407	0.010	0.009	0.018
Cluster: town	(0.009)***	(36.120)***	(32.531)***	(0.004)***	(0.003)***	(0.004)***
Conley s.e.: cutoff = 5 km	(0.009)***	(35.944)***	(32.652)**	(0.004)***	(0.003)***	(0.004)***
Conley s.e.: cutoff = 10 km	(0.009)***	(37.231)***	(33.544)***	(0.004)***	(0.003)***	(0.004)***
Conley s.e.: cutoff = 25 km	(0.010)***	(39.206)***	(35.816)***	(0.004)**	(0.003)**	(0.005)***
Conley s.e.: cutoff = 50 km	(0.011)***	(38.674)***	(36.886)***	(0.005)**	(0.004)**	(0.005)***
Conley s.e.: cutoff = 100 km	(0.013)**	(34.447)***	(35.180)***	(0.005)*	(0.005)*	(0.005)***
Mean Y Control Group	0.36	450.65	319.04	0.03	0.03	0.05
Number of Towns	652	650	650	536	534	652
Observations	7756	650	650	1922	1868	3912

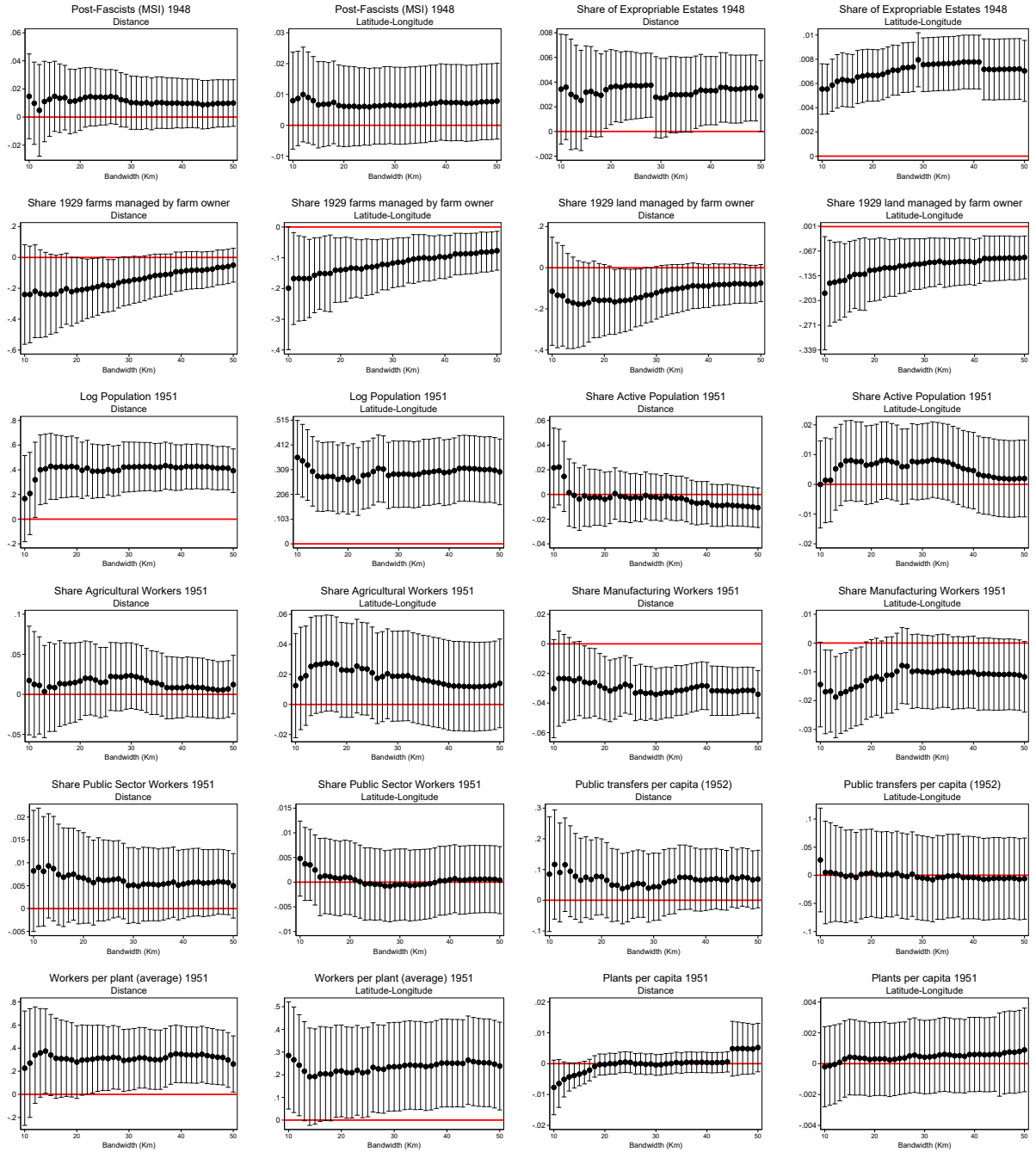
Notes: The Table reports on the first row coefficients β_t and β from alternative specifications of Equations (1) (Cols. 1, 6) and (2) (Cols. 2, 3, 4, 5). Panel A controls for a linear polynomial in distance, Panel B for a quadratic polynomial in latitude and longitude. Cols. 1, 6: regressions control 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 heteroskedastic robust (Cols. 2, 3) in parentheses. Rows 3-7 of each panel: standard errors robust to time-series and spatial correlation calculated with the formula of [Conley \(1999\)](#). In these estimates, spatial correlation is assumed to decay linearly until a cutoff. We report results from 5 different cutoffs: 5 km, 10 km, 25 km, 50 km and 100 km. *p<0.1, **p<0.05, ***p<0.01.

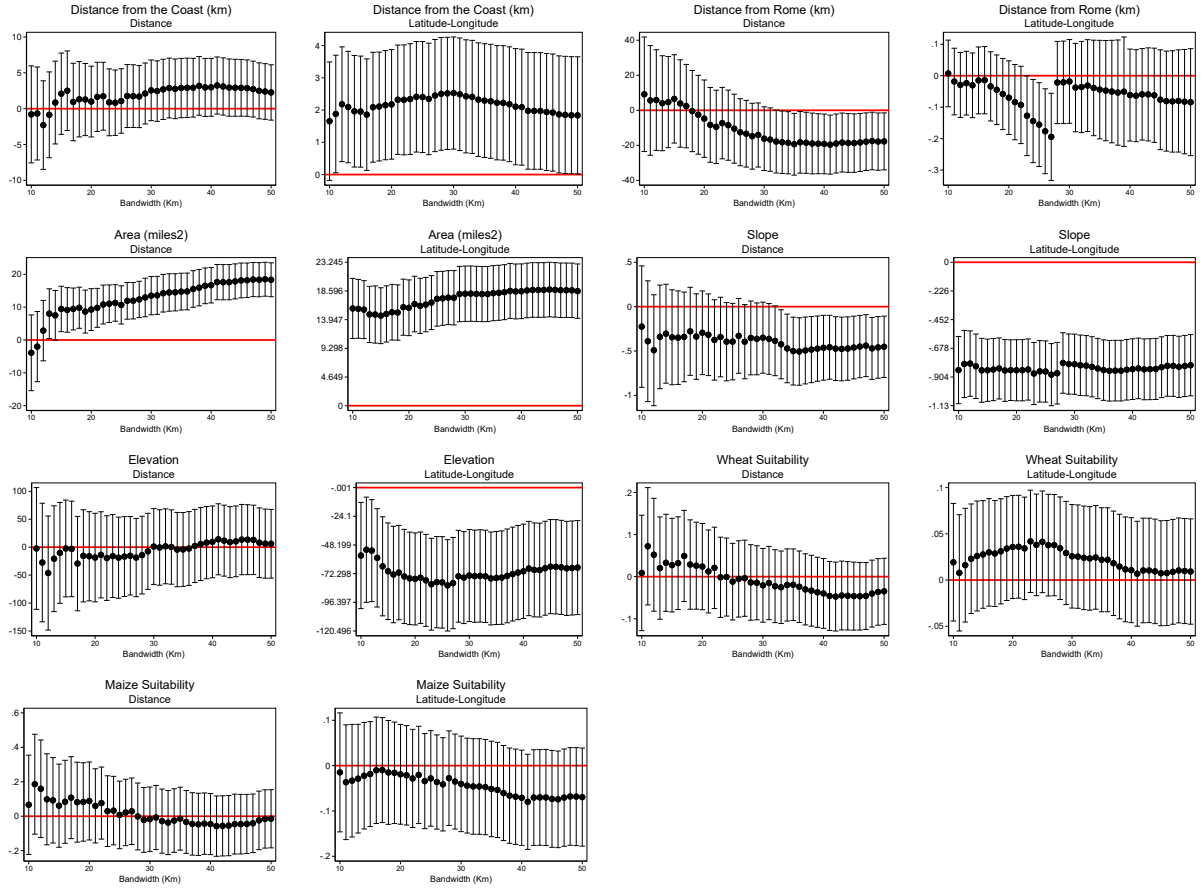
D Additional Results

D.1 Southern Italy

Figure D.1: Balance – Southern Italy

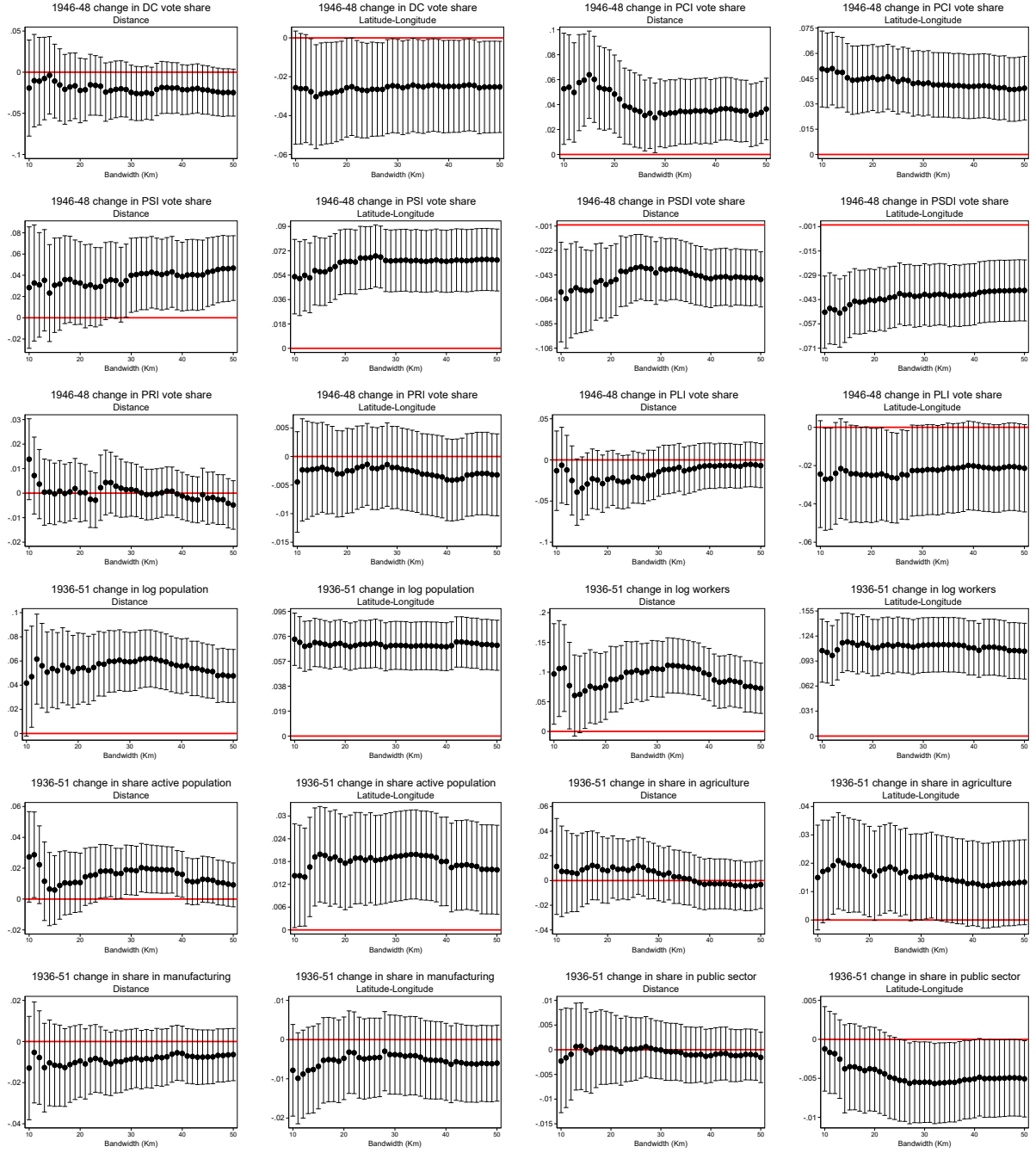






Notes: The graphs report the coefficients of separate regressions of the RD specification in Equation (2) along with 95% confidence intervals. We report separate estimates for each bandwidth between 10 and 50 km in 1 km intervals. The subtitle *Distance* indicates the RD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RD 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.

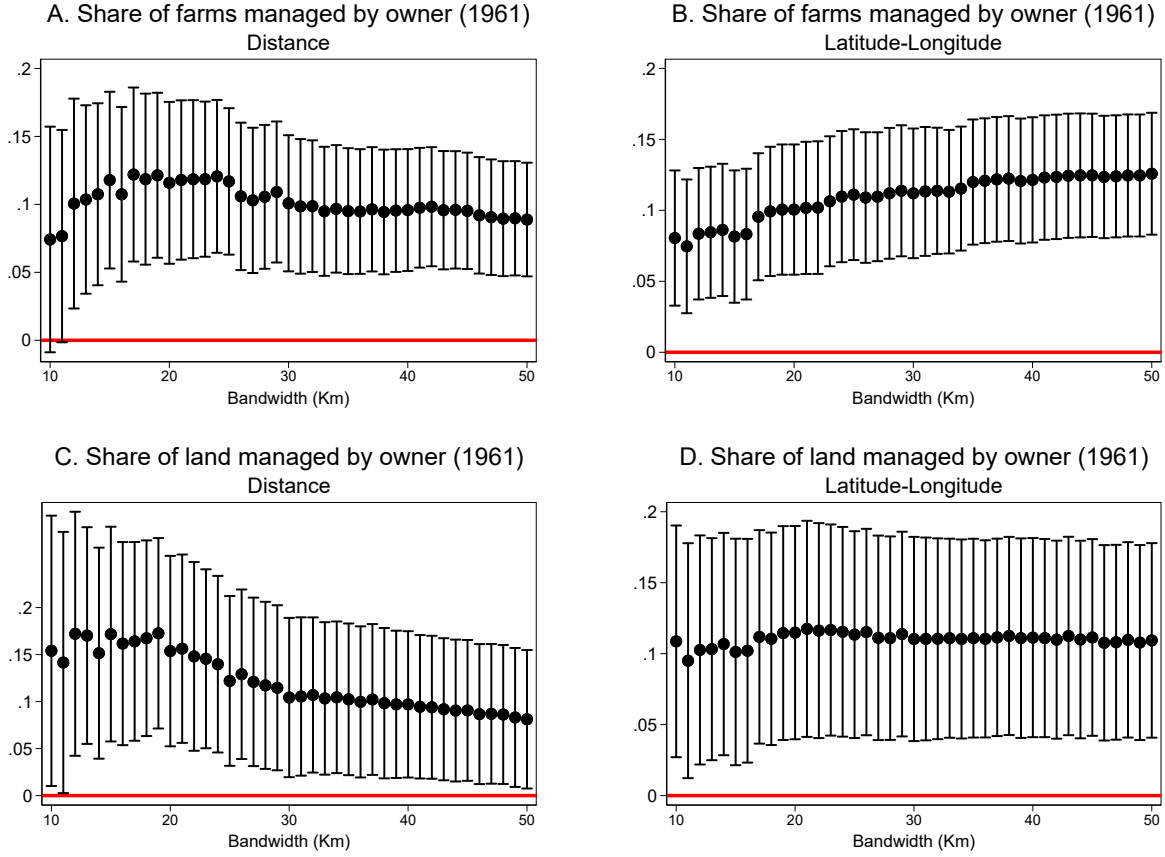
Figure D.2: Pre-trends – Southern Italy



Notes: The graphs report the coefficients of separate regressions of the RD 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 RD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RD 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.3: The impact of the reform on farm management.



Notes: The graphs report β of RD 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 heteroskedasticity robust standard errors and plot 95% confidence intervals as bars around the coefficients.

D.3 Effect of the Reform on PCI, Other Parties and Turnout

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

Preliminary Graphical Evidence. Appendix Figure D.4 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 about 3.5 p.p. Appendix Figure D.5 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.6-Panels A-B plot the effect of the reform on PCI vote share over the entire period (β_t in Equation (1)) in the two specifications. The 1946 coefficient confirms the absence of differential trends pre-reform (see also Figure 2). The treatment coefficient in the 1953 election suggests that in treated towns PCI vote share decreased by between 2.3 and 2.4 p.p. during the first election after the land reform, from a control mean of 28%: a decrease of between 8 and 9 percent. This effect is slightly 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.6-Panels C-D, we find that the results are generally stable across bandwidths, with the exception of very small bandwidths in the distance specification. These results confirm that the land redistribution was successful in stemming the rise of communism in the countryside.⁵⁰

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.7 reports treatment coefficients from a panel RD regression that includes the 1919, 1921, and 1924 elections (the last ones before the Fascist dictatorship), as well as the 1946 and the 1948 ones. We look at two left-wing parties: the Italian Socialist Party (PSI) and the Italian Communist Party (PCI). PSI was the largest left-wing party until 1947: it won relative majorities in the elections of 1919 and 1921 and had one of its leaders, Giacomo Matteotti, killed by fascist hit men in 1924.⁵¹ PCI was relatively small before the war: founded in 1921, it collected 4.6% of votes that year and 3.7% in 1924. We report pre-fascism elections results for the two specifications at 20 km (distance) and 35 km (coordinates), for both parties (Appendix Figure D.7). In pre-fascism elections, 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

⁵⁰In 1948 the PCI and PSI ran on a joint list (*Fronte Democratico Popolare*), so the 1948 baseline reflects their combined vote. Using the combined PCI+PSI vote share as the outcome throughout, treated towns show a differential decline of about 2 to 4 percentage points after the reform, mirroring the DC gains and consistent with the results for PCI alone.

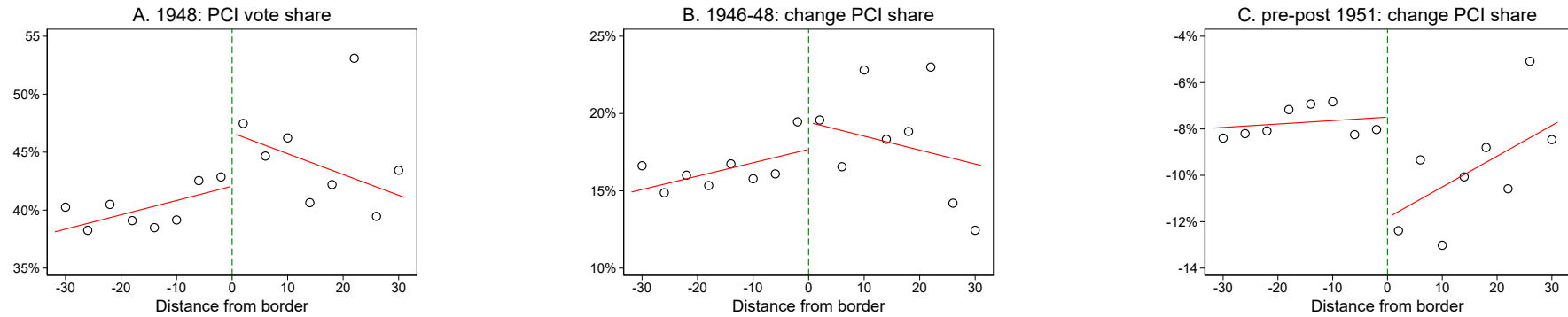
⁵¹PSI lost ground to PCI after 1947, when it split into 2 parties. One of these parties ran together with PCI in 1948. After that year, PSI never received more than 15% of votes. When we look at the effect of the reform on PSI vote shares in 1946-92 we find no significant effect (see Appendix D.8).

specification; the coefficient for 1946 is significantly negative only in the distance specification. Looking at coefficients for 1921 and 1924, PCI vote shares seem to grow faster in treatment towns, even though this differential trend disappears between 1946 and 1948. Overall, this evidence suggests that 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.8 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 exceptions are two effects: a small negative effect on the vote share of MSI (a small neo-fascist party), which is not stable across specifications and bandwidths, and – only in the coordinate specification – a positive effect on PSDI, a small social-democratic party allied with DC in many DC-led governments, including the one that enacted the reform.⁵² Taken together, these estimates imply that between 63% (distance) and 90% (coordinates) of the combined gains of DC and PSDI came at the expense of the Communist Party, with the remainder plausibly coming from MSI and other minor parties. Additionally, in Appendix Figure D.9, we verify that the reform did not affect turnout. The only significant coefficient is in 1946, a pre-reform year, in the distance specification: treated towns show slightly higher turnout before the reform. This difference is small (0.7 p.p., against a control-town baseline of $\sim 95\%$) and completely disappears in the coordinate specification.

⁵²While significant in the multidimensional RDD when pooling all post-reform years ($p = 0.06$), this effect is not stable across time and mostly driven by the 1968 elections alone.

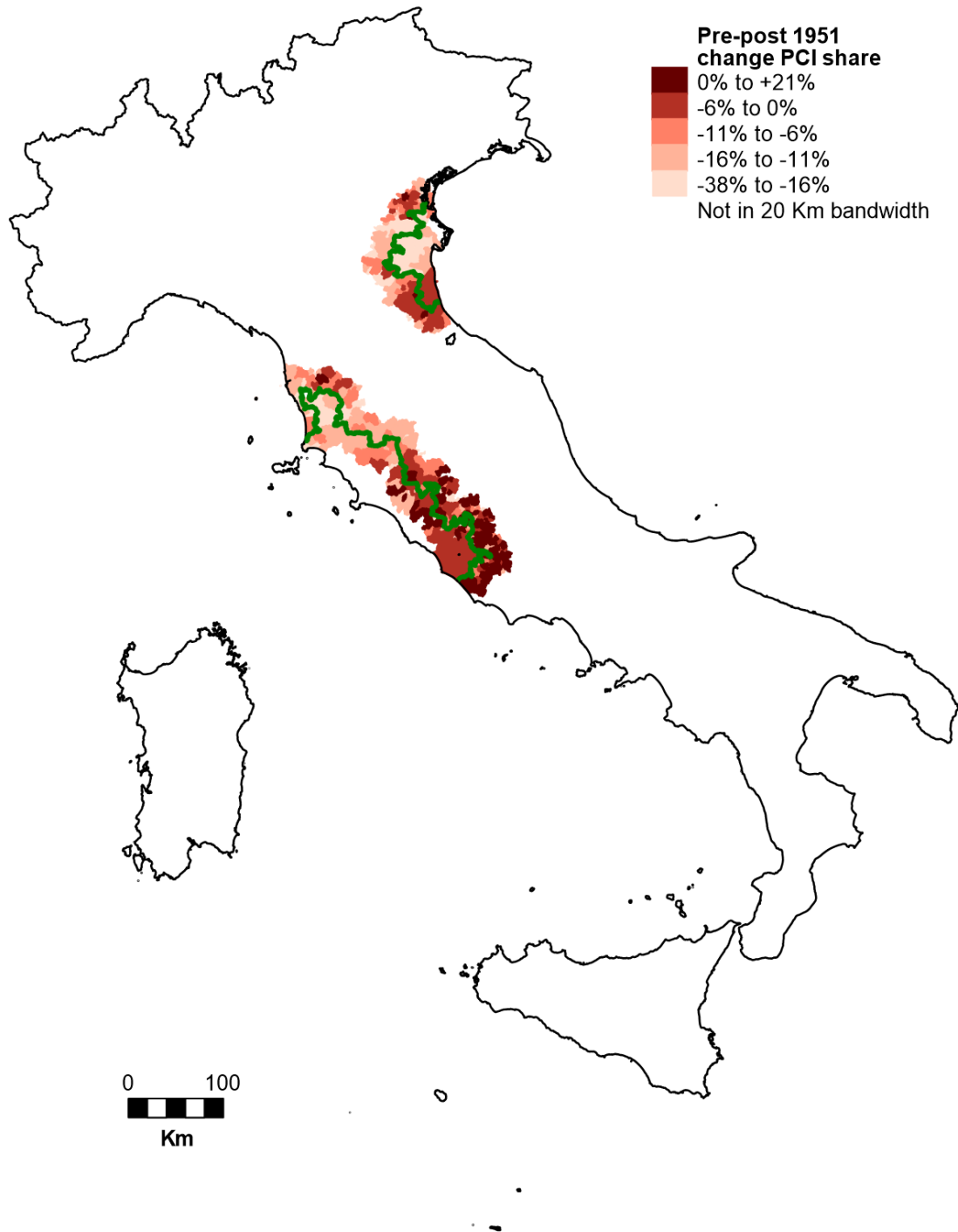
Figure D.4: Balance, pre-trends and effect of reform on PCI vote share: graphical evidence



Notes: The Figure presents graphical evidence on the panel RD. 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.5: Electoral effect of the reform: visualization of impact on a map

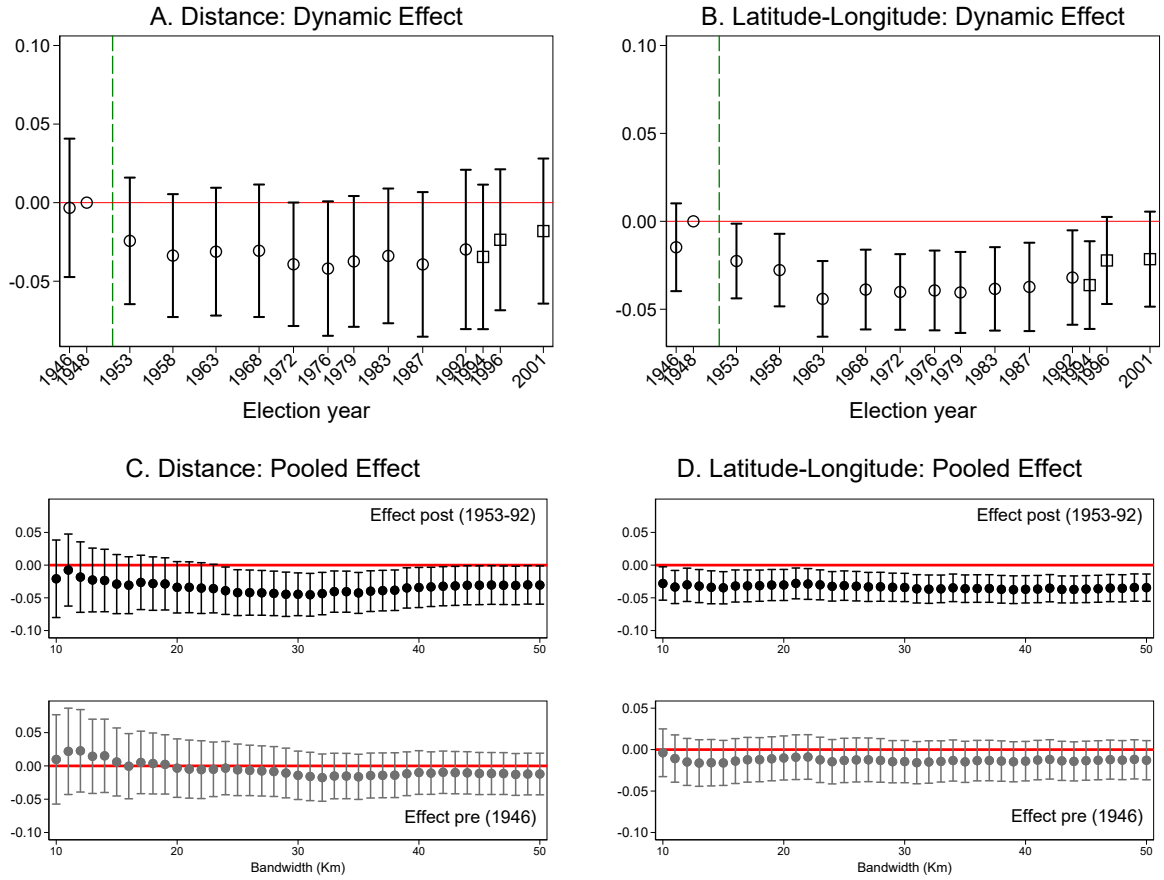
Change PCI share



Bandwidth: 20Km.

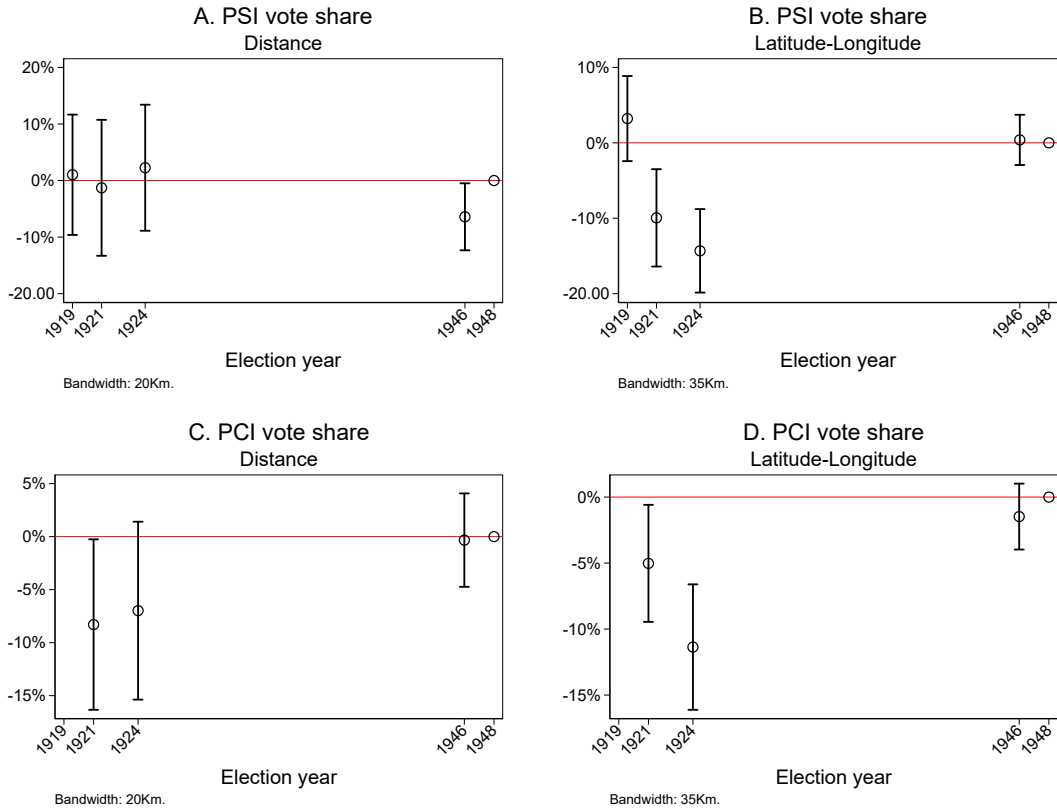
Notes: Change in PCI vote share between pre- (1946-48) and post-reform elections (1953-92) in the towns within 20 km from the reform borders of Delta Padano and Maremma.

Figure D.6: The electoral impact of the land reform – Communist Party (PCI)



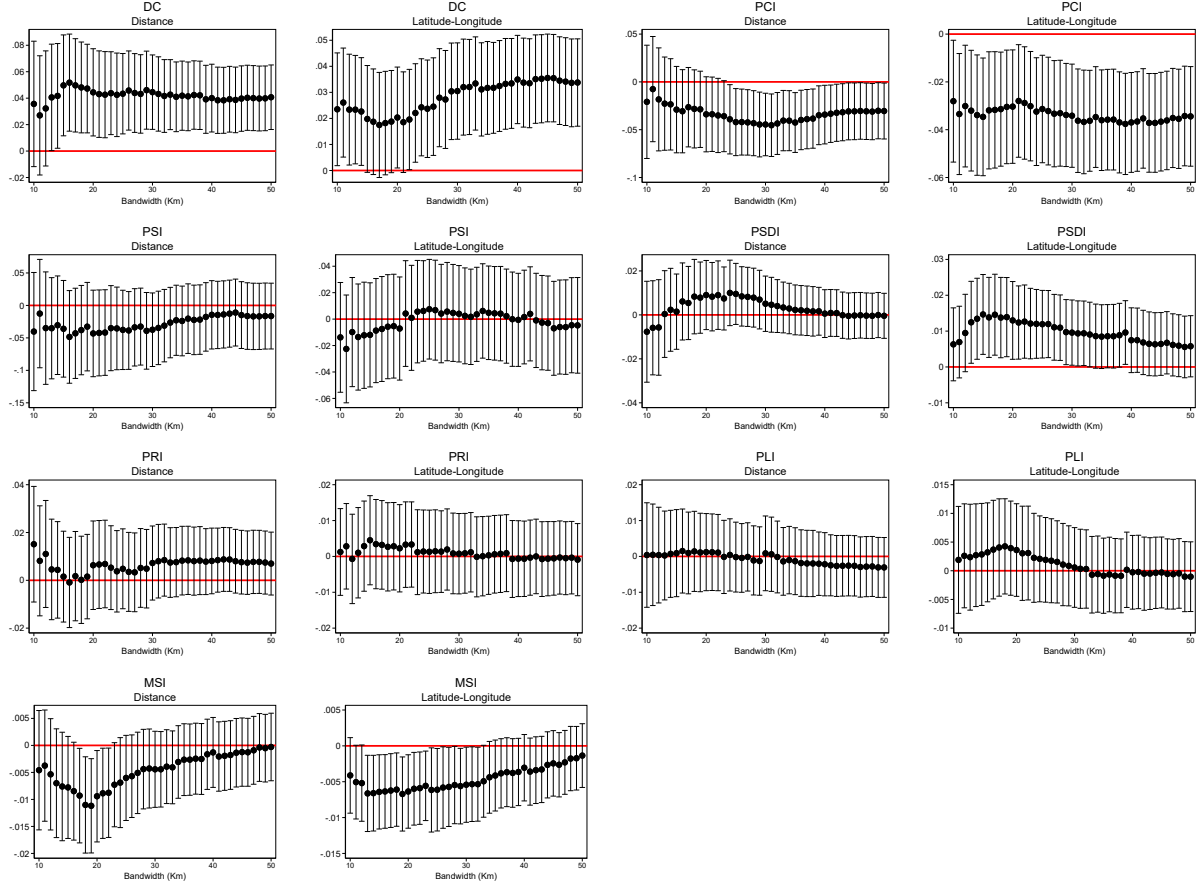
Notes: Dependent variable is Communist Party (PCI) vote share. Panels A and B display coefficients β_t for each year from the panel RD Equation (1). The omitted category is the β of 1948 and the sample consists of all towns within 20 (Panel A) and 35 (Panel B) km from the reform borders of Delta Padano 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.7: 1919-1948 Pre-Trends PCI and PSI



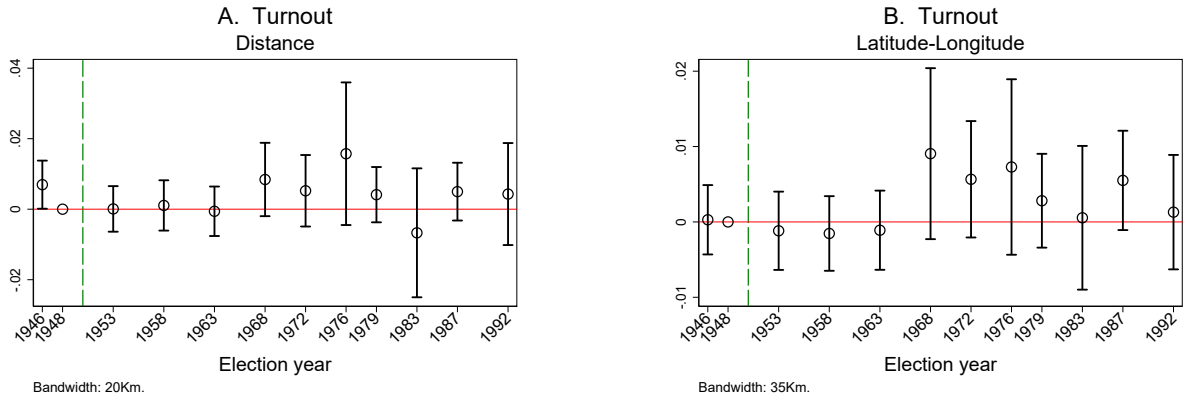
Notes: Dependent variable is Socialist Party (PSI) vote share (Panel A and B) and Communist Party (PCI) vote share (Panel C and D). All panels display coefficients β_t for each year before the reform from the panel RD Equation (1). The omitted category is the β of 1948. The sample consists of all towns 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.8: 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 RD regressions controlling for a linear polynomial in distance. The subtitle *Latitude-Longitude* indicates the RD 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.

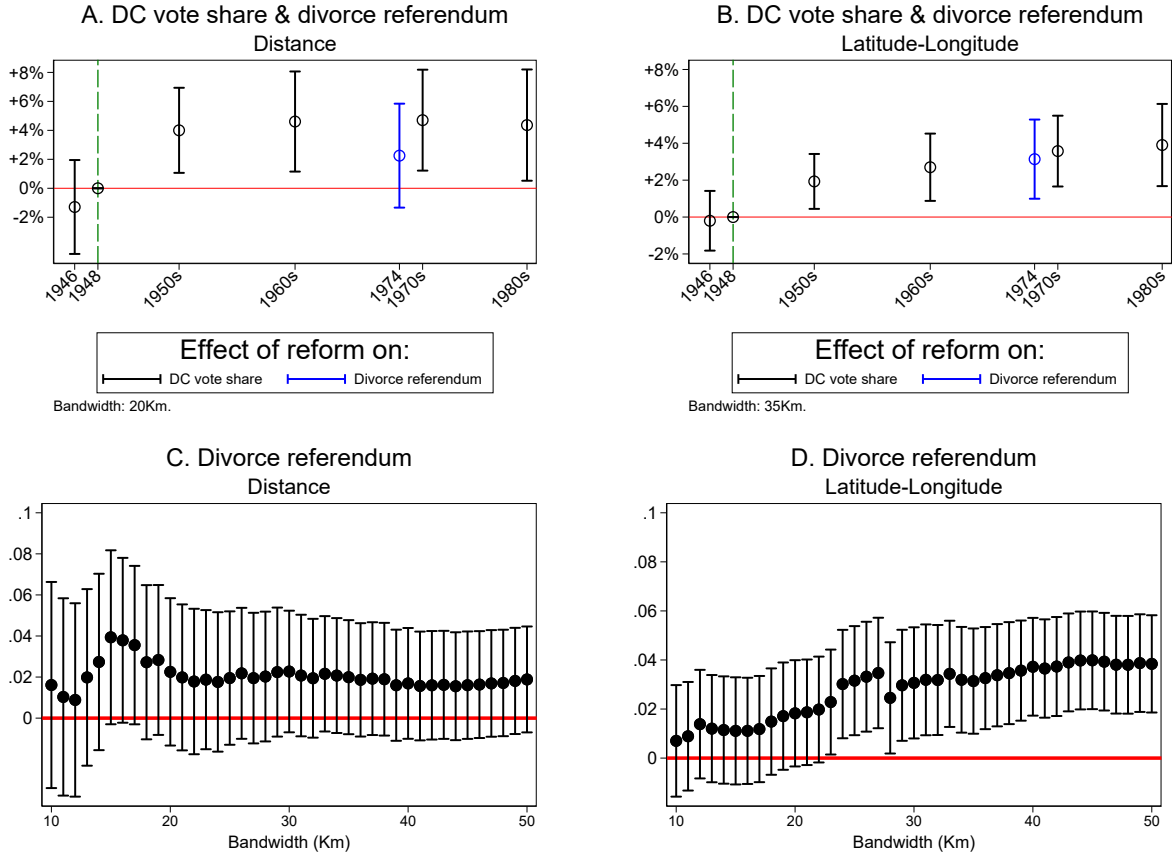
Figure D.9: Turnout: Panel Coefficients



Notes: The Figure displays coefficients β_t from the panel RD Equation (1), which controls for year $\times$ reform area and town fixed effects. The omitted category is the β of 1948. Dependent variable is votes cast divided by number of eligible voters. Panel A controls for a linear polynomial in distance. Panel B controls for a quadratic polynomial in latitude and longitude. Units of observation are town-years. The sample consists of all towns within 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.4 Divorce Referendum

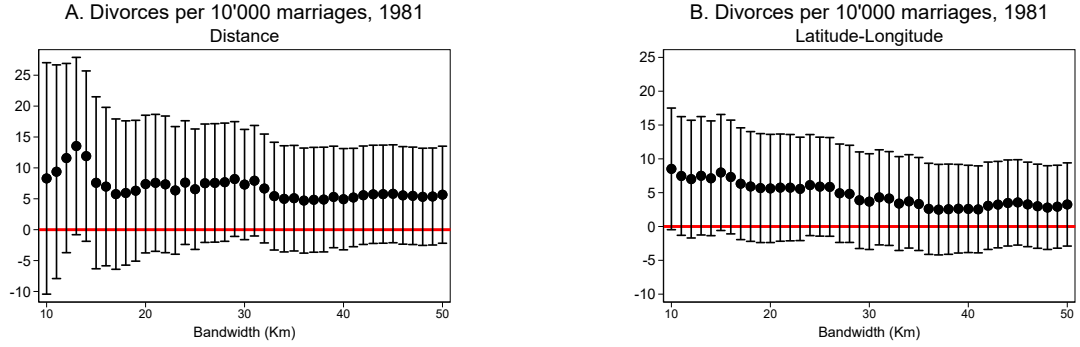
Figure D.10: 1974 Referendum on the Repeal of Divorce



Notes: Panel A and B display coefficients β_t from the panel RD Equation (1), which controls for year $\times$ reform area and town fixed effects. We estimate β for separate decades, for year 1946, as well as for the 1974 divorce referendum. The omitted category is the β of the election of 1948. We include the 1992 election in the 1980s decade. The dependent variable is Christian Democrat (DC) vote share in every year except 1974; the source is [Corbetta and Piretti \(2009\)](#). In these years, we plot the β in black. In 1974 dependent variable is share of “yes” votes in the divorce referendum; the source is [Ministry of Interior \(1977\)](#). In this year, we plot the coefficient in blue. The sample consists of all towns within 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.

D.5 Divorces in 1981

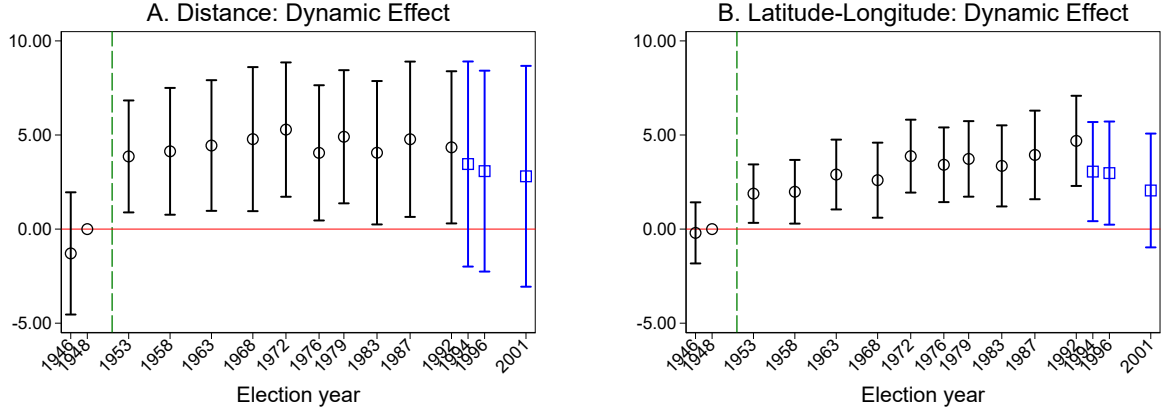
Figure D.11: Divorces per 10'000 marriages 1981



Notes: The graphs report coefficients β_i of 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 divorced individuals divided by the number of married individuals in the municipality (both measured in 1981), multiplied by 10'000. 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 The impact of the reform on DC votes after 1992

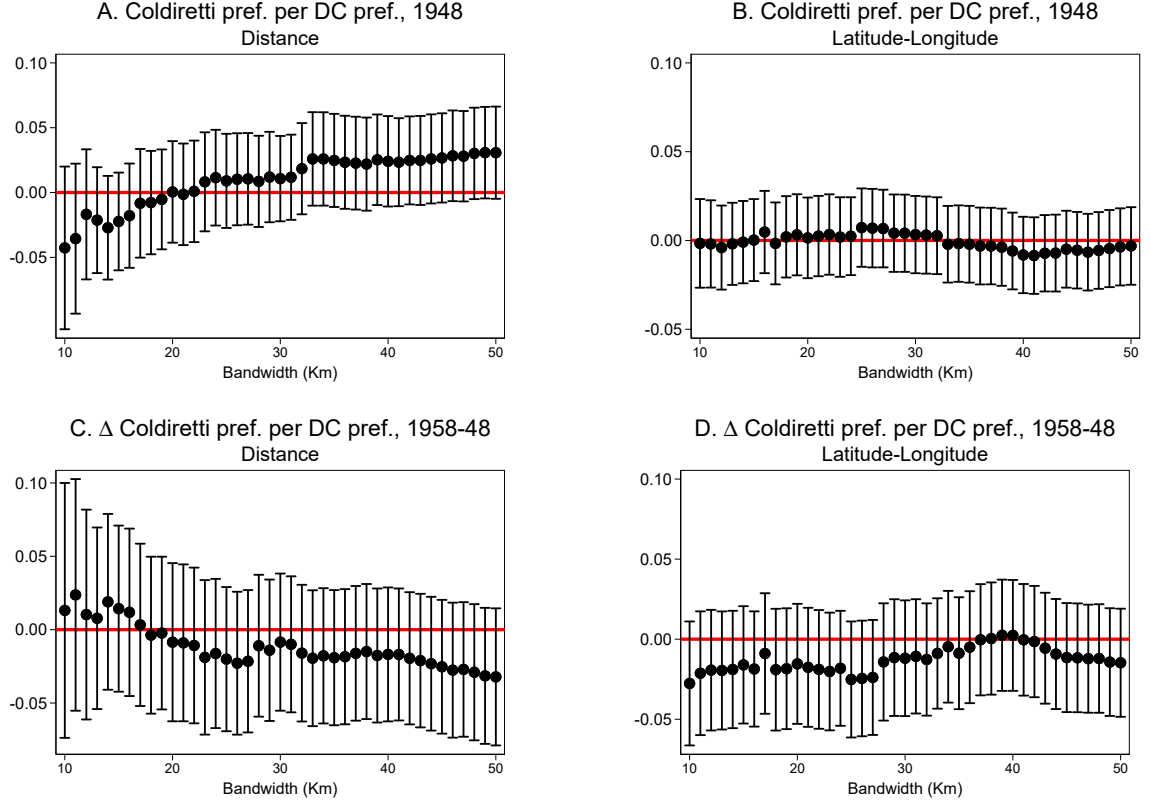
Figure D.12: The impact of the reform on DC votes after 1992



Notes: Dependent variable is Christian Democrat (DC) vote share. Panels A and B display coefficients β_t from the panel RD Equation (1), $t = \{1946, \dots, 2001\}$. The omitted category is the β 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). All regressions control for year $\times$ reform area and town fixed effects. The regression in Panel A controls for a linear polynomial in distance, the regression in Panels B controls 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.

D.7 The impact of the reform on electoral preferences for Coldiretti

Figure D.13: The impact of the reform on electoral preferences for Coldiretti



Notes: Dependent variable is the share of electoral preferences gained by DC candidates connected to *Coldiretti* (out of the total electoral preferences gained by DC candidates). In Panel A and B the outcome is the share of electoral preferences gained by DC candidates connected to *Coldiretti* in 1948. Panel C and D plot the change in the same variable between 1948 and 1958. All panels display coefficients β_t from the cross-section RD Equation (2). 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 reform area fixed effects. Regressions in Panel A and C control for a linear polynomial in distance, regression in Panels B and D control for a quadratic polynomial in latitude and longitude. Electoral preference data are from [Ministero dell'Interno \(1948, 1958\)](#); see Appendix B for the identification of *Coldiretti* candidates and its sources. Units of observation are towns. We estimate robust standard errors and plot 95% confidence intervals as bars around the coefficients.

D.8 Gratitude

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

	DC vote share	
	(1)	(2)
Treatment x 1950s	0.030** [0.012]	0.034*** [0.010]
Treatment x 1960s	0.031** [0.013]	0.037*** [0.011]
Treatment x 1970s	0.051*** [0.013]	0.057*** [0.012]
Treatment x 1980s	0.074*** [0.015]	0.083*** [0.013]
Old population in 1951 $\times$ Treatment $\times$ 1950s	0.002 [0.015]	0.001 [0.013]
Old population in 1951 $\times$ Treatment $\times$ 1960s	-0.003 [0.018]	-0.005 [0.015]
Old population in 1951 $\times$ Treatment $\times$ 1970s	-0.015 [0.019]	-0.020 [0.017]
Old population in 1951 $\times$ Treatment $\times$ 1980s	-0.038* [0.021]	-0.045** [0.018]
Old population in 1951 $\times$ 1950s	0.014* [0.008]	0.008 [0.006]
Old population in 1951 $\times$ 1960s	0.013 [0.009]	0.006 [0.007]
Old population in 1951 $\times$ 1970s	0.032*** [0.010]	0.024*** [0.008]
Old population in 1951 $\times$ 1980s	0.046*** [0.010]	0.039*** [0.009]
Mean Y Control	0.35	0.37
Number of Towns	417	652
Bandwidth	20 Km	35 Km
Specification	DiD	DiD
Observations	4972	7756

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.9 Cooperatives

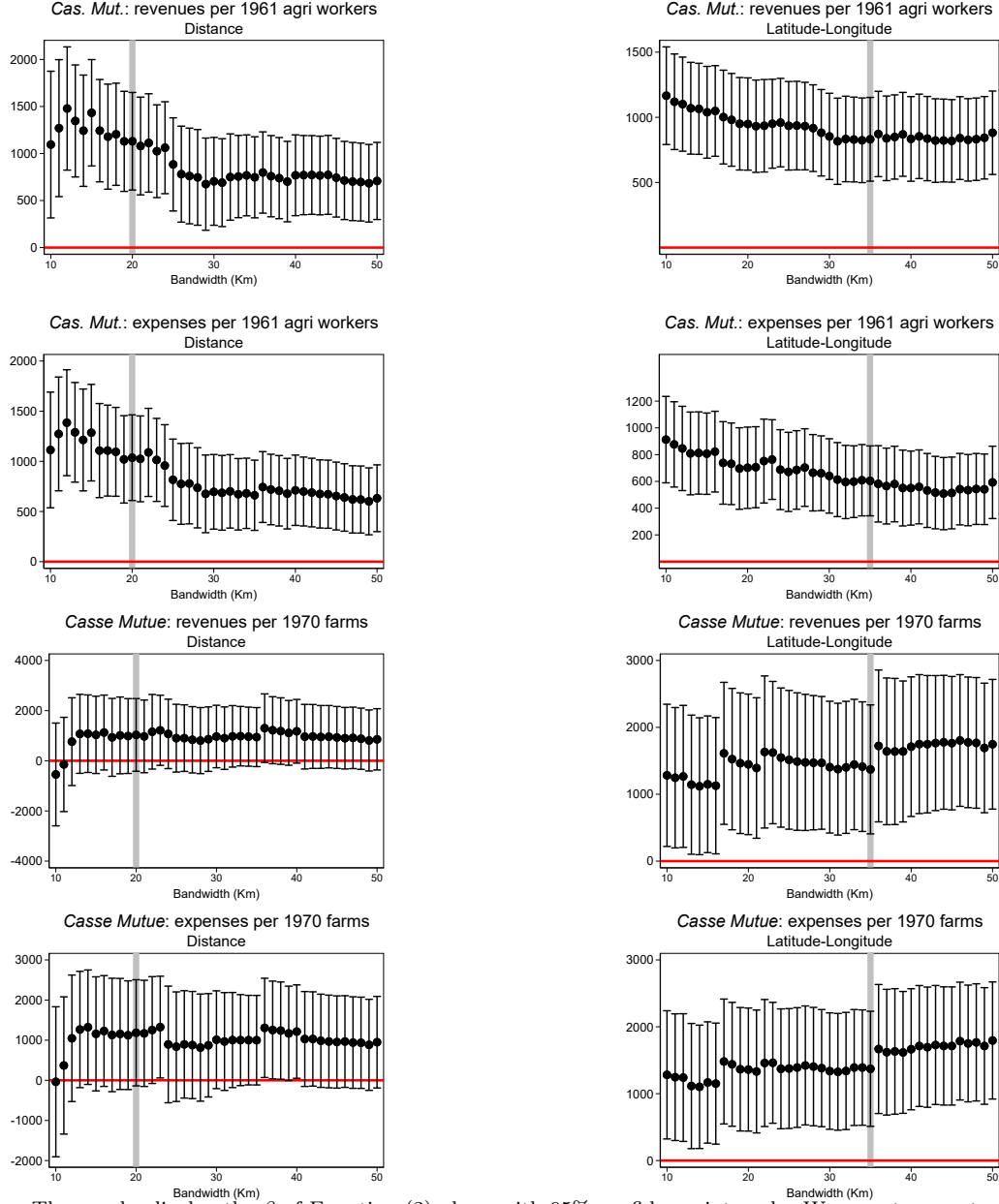
Table D.2: Interaction with the presence of cooperatives.

	DC vote share	
	(1)	(2)
Treatment x 1950s	0.016* [0.009]	0.019** [0.009]
Treatment x 1960s	0.013 [0.012]	0.017 [0.012]
Treatment x 1970s	0.020 [0.014]	0.021 [0.013]
Treatment x 1980s	0.027* [0.015]	0.031** [0.015]
At least 1 cooperative (dummy) × Treatment × 1950s	0.023* [0.012]	0.023** [0.011]
At least 1 cooperative (dummy) × Treatment × 1960s	0.025* [0.015]	0.026* [0.014]
At least 1 cooperative (dummy) × Treatment × 1970s	0.033** [0.016]	0.035** [0.016]
At least 1 cooperative (dummy) × Treatment × 1980s	0.037** [0.018]	0.038** [0.017]
Mean Y Control	0.35	0.37
Number of Towns	417	652
Bandwidth	20 Km	35 Km
Specification	DiD	DiD
Observations	4972	7756

Notes: The Table reports coefficients from a difference-in-difference specification with heterogeneity in the presence of a beneficiaries' cooperative. We measure the presence of cooperatives with a dummy=1 if there was at least one cooperative in a reform municipality. The regressions control for year × 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.10 Alternative normalizations of *Casse Mutue* budgets

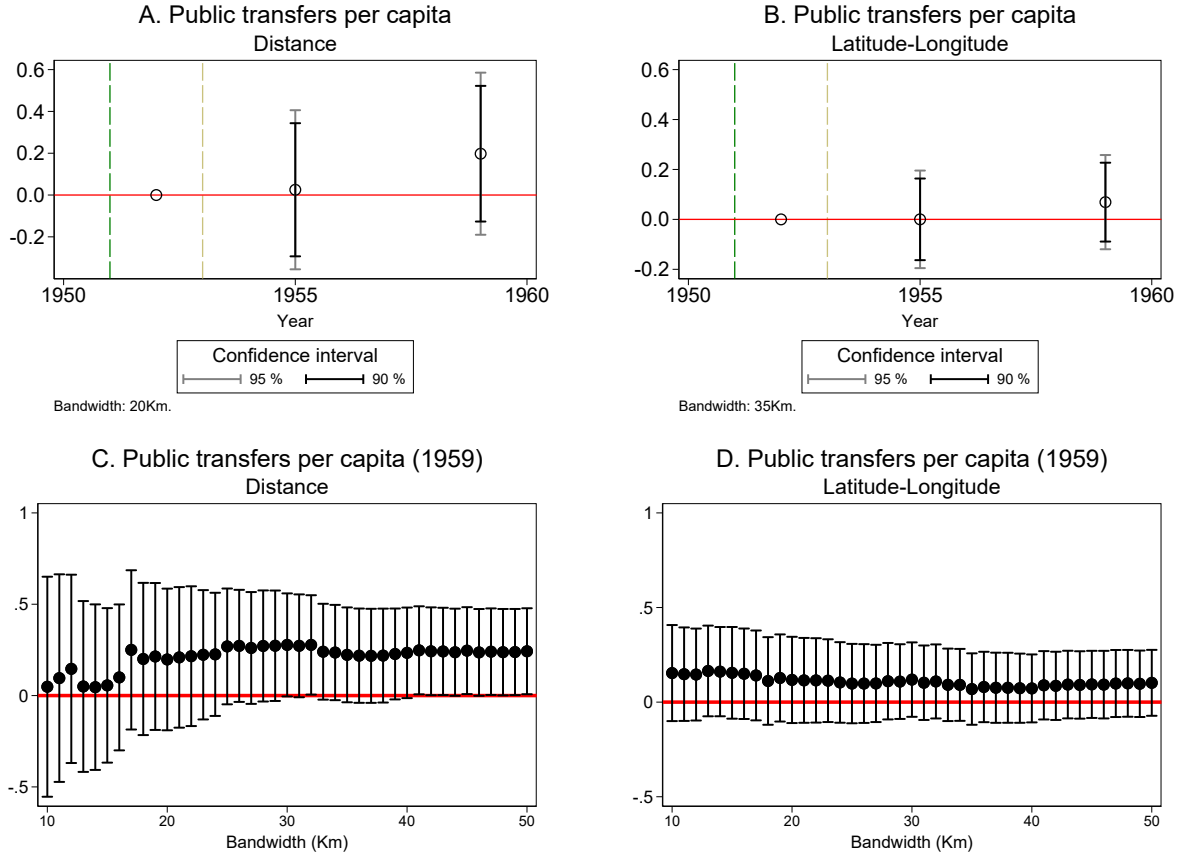
Figure D.14: The impact of the reform on *Casse Mutue* budgets: alternative normalizations



Notes: The graphs display the β of 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. Shaded areas indicate 20 and 35 km: the optimal bandwidths in the two specifications. Dependent variables are *Casse Mutue* revenues (Panels A–B, and E–F) and expenses (Panels C–D, and G–H) scaled by 1961 agricultural workers (Panels A–D) and number of farms in 1970 (Panels E–H). We trim *Casse Mutue* revenues and expenses per agricultural workers at 10'000 to avoid that outliers driving the results. Units of observation are towns. Standard errors heteroskedasticity robust.

D.11 The impact of the reform on pork-barrel spending

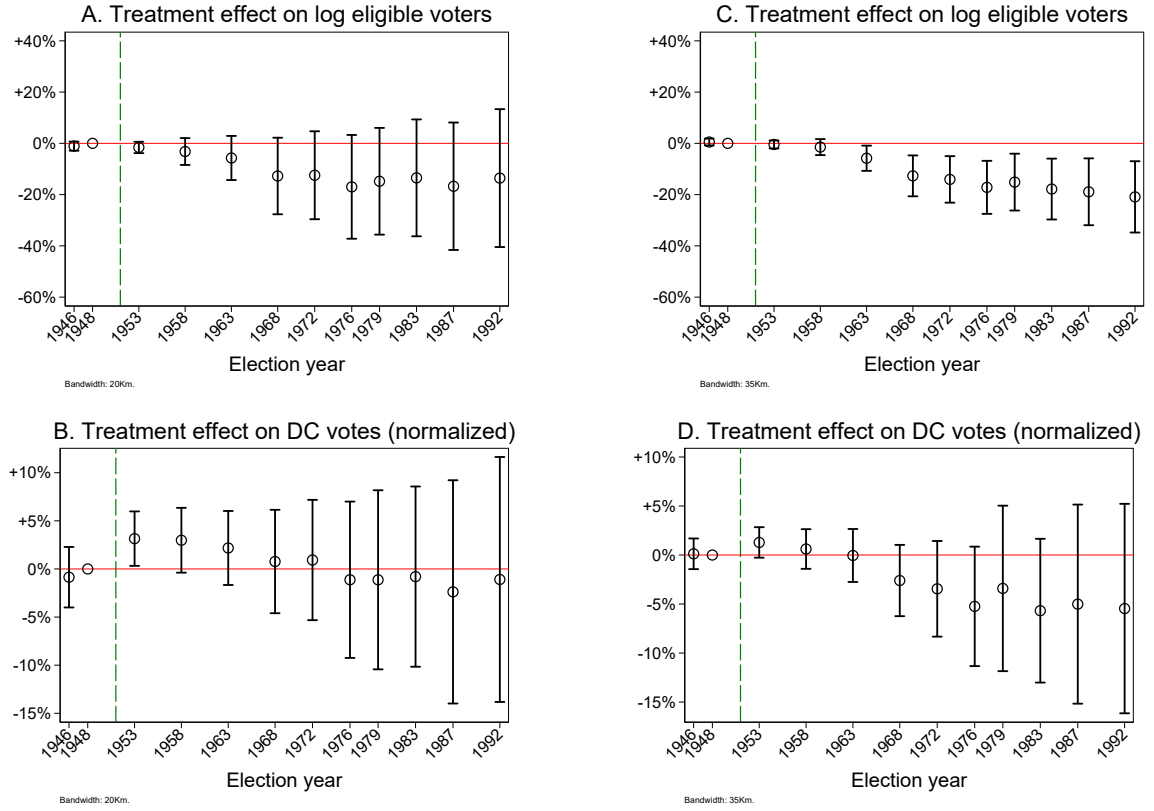
Figure D.15: The impact of the reform on pork spending.



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 β_t for each year from the panel RD Equation (1). The omitted category is the β 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 1951 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.12 Migration

Figure D.16: Impact of reform on the number of eligible voters and DC votes



Notes: All panels display coefficients β_t from the panel RD Equation (1), which controls for year $\times$ reform area and town fixed effects. The omitted category is the β 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.

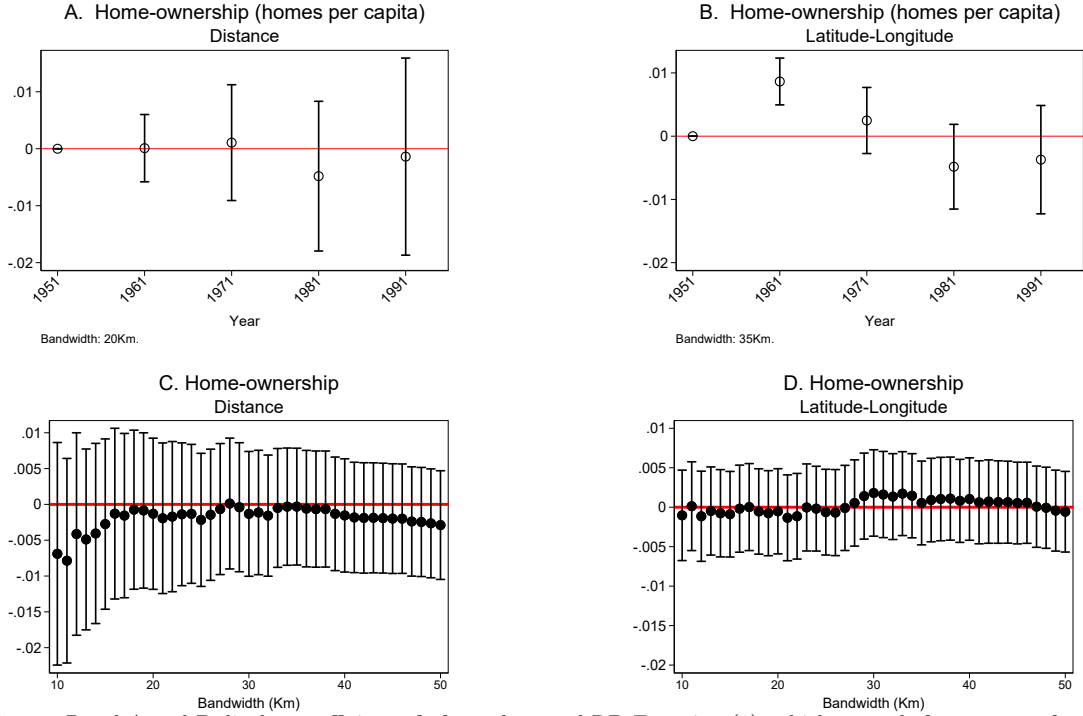
Table D.3: Impact of reform in the north on population structure. Bandwidth: 20 and 35 Km.

	Distance					Latitude-Longitude				
	Share population aged				Share	Share population aged				Share
	0-20	21-45	46-65	>65	males	0-20	21-45	46-65	>65	males
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x 1961	-0.003	-0.003	0.002	0.004	-0.000	-0.006**	-0.001	0.003	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 x 1971	-0.002	-0.003	-0.004	0.009	-0.001	-0.013**	-0.000	0.006	0.007*	-0.002
	[0.008]	[0.007]	[0.007]	[0.007]	[0.002]	[0.005]	[0.004]	[0.004]	[0.004]	[0.002]
Treatment x 1981	-0.004	-0.005	-0.004	0.012	-0.003	-0.017***	-0.004	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 x 1991	-0.006	-0.004	-0.002	0.012	-0.004	-0.021***	-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	0.29	0.34	0.23	0.14	0.50	0.29	0.34	0.23	0.14	0.50
Number of Towns	417	417	417	417	417	652	652	652	652	652
Observations	2085	2085	2085	2085	2085	3260	3260	3260	3260	3260

Notes: The Table reports coefficients β_t from the panel RD 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 β 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.

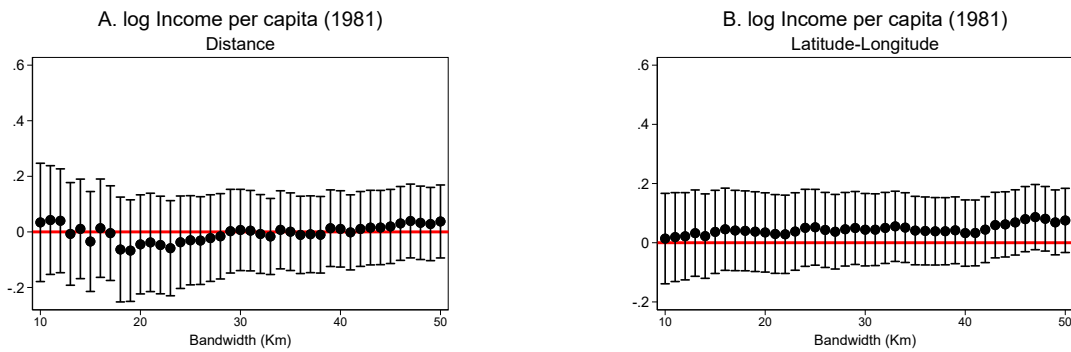
D.13 Economic Growth and Development Patterns

Figure D.17: The impact of the reform on home ownership.



Notes: Panel A and B display coefficients β_t from the panel RD Equation (1), which controls for year $\times$ reform area and town fixed effects. We estimate β for separate census years. The omitted category is the β of the elections of 1951. The dependent variable is the number of 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.18: The impact of the reform on income.



Notes: The graphs report coefficients β_t of the RD 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.4: Impact of reform in the north on active population and sectoral shares. Bandwidth: 20 and 35 Km.

	Distance						Latitude-Longitude					
	Active pop. (1)	Active men (2)	Active women (3)	Agriculture (4)	Manufacturing (5)	Services (6)	Active pop. (7)	Active men (8)	Active women (9)	Agriculture (10)	Manufacturing (11)	Services (12)
Treatment x 1936	0.009 [0.017]			0.025 [0.017]	-0.005 [0.011]	-0.000 [0.005]	-0.020* [0.011]			0.007 [0.014]	-0.010 [0.009]	0.001 [0.003]
Treatment x 1961	-0.022 [0.018]	-0.002 [0.013]	-0.041 [0.031]	-0.041** [0.021]	0.006 [0.010]	-0.004 [0.005]	-0.047*** [0.010]	-0.022*** [0.008]	-0.071*** [0.017]	-0.018 [0.012]	-0.017*** [0.007]	-0.002 [0.004]
Treatment x 1971	-0.013 [0.018]	0.005 [0.018]	-0.031 [0.031]	-0.036 [0.029]	-0.001 [0.018]	0.021** [0.010]	-0.040*** [0.011]	-0.021* [0.011]	-0.057*** [0.019]	-0.030* [0.016]	-0.020* [0.011]	0.009 [0.007]
Treatment x 1981	0.002 [0.021]	0.004 [0.018]	0.004 [0.034]	-0.020 [0.032]	-0.006 [0.021]	0.022 [0.014]	-0.040*** [0.012]	-0.018* [0.011]	-0.056*** [0.020]	-0.016 [0.018]	-0.019 [0.014]	-0.005 [0.009]
Treatment x 1991	0.002 [0.021]	-0.008 [0.015]	0.014 [0.036]	0.009 [0.036]	-0.028 [0.022]	0.038*** [0.012]	-0.042*** [0.013]	-0.023* [0.013]	-0.057*** [0.021]	-0.010 [0.020]	-0.006 [0.015]	-0.007 [0.008]
Mean Y Control Group	0.56	0.85	0.31	0.42	0.20	0.17	0.56	0.85	0.32	0.40	0.23	0.17
Number of Towns	417	417	417	417	417	417	652	652	652	652	652	652
Observations	2502	2085	2085	2502	2502	2502	3912	3260	3260	3912	3912	3912

Notes: The Table reports coefficients β_t from the panel RD Equation (1), which controls for year $\times$ 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 β 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.14 Firms' growth

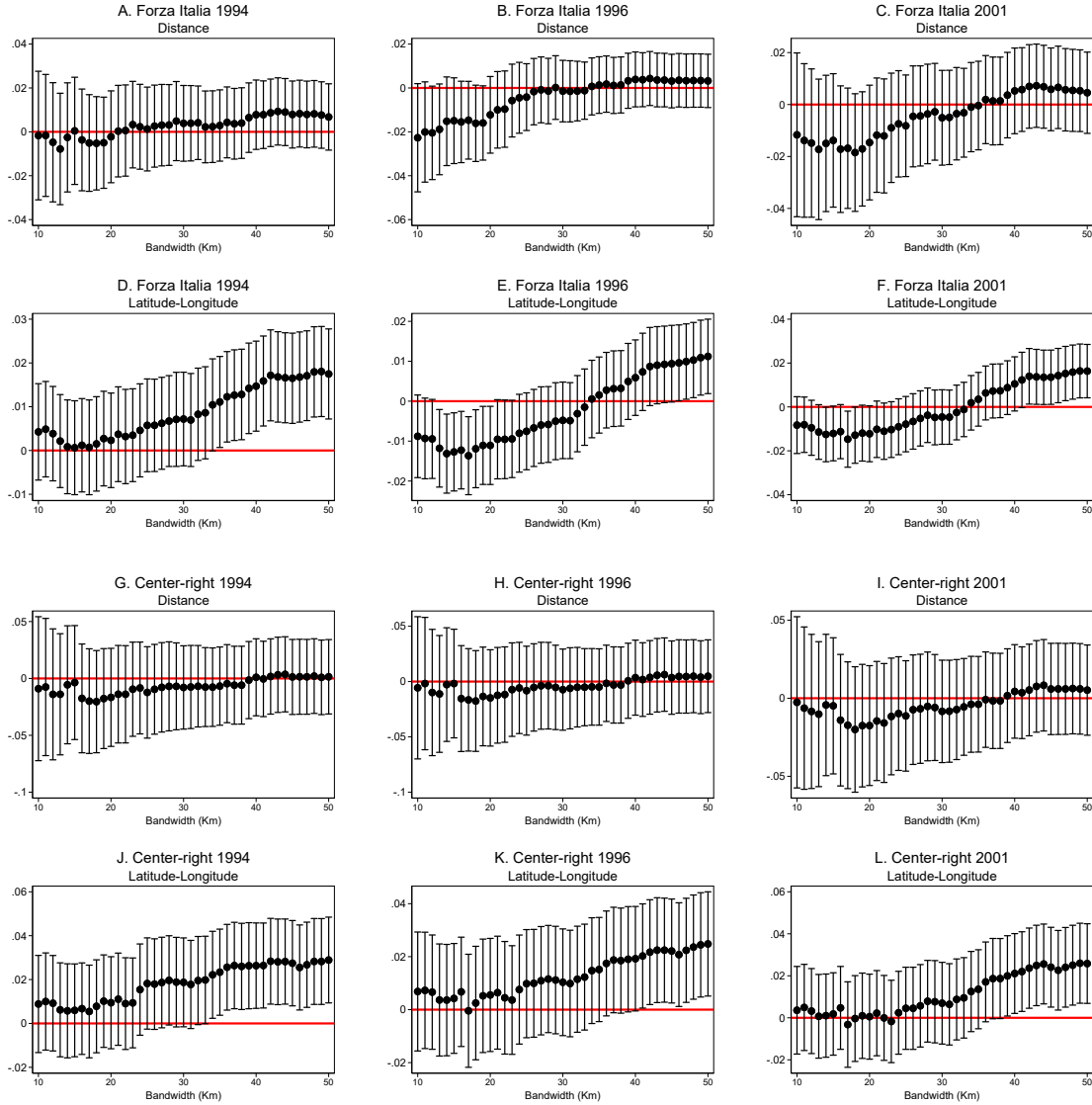
Table D.5: Impact of reform in the north on the number of workers per firm plant.

	Distance						Latitude-Longitude					
	Number of plants per capita			Number of workers per plant			Number of plants per capita			Number of workers per plant		
	(1) < 20km	(2) < 10km	(3) < 50km	(4) < 20km	(5) < 10km	(6) < 50km	(7) < 35km	(8) < 10km	(9) < 50km	(10) < 35km	(11) < 10km	(12) < 50km
Treatment x 1961	0.004 [0.007]	0.001 [0.006]	-0.002 [0.003]	0.439 [0.277]	0.866* [0.441]	0.110 [0.205]	-0.002 [0.003]	0.001 [0.003]	-0.002 [0.003]	-0.036 [0.173]	0.282 [0.233]	-0.083 [0.150]
Treatment x 1971	0.006 [0.007]	0.006 [0.007]	-0.003 [0.003]	-0.026 [0.326]	0.058 [0.438]	-0.266 [0.226]	-0.001 [0.003]	0.002 [0.003]	-0.002 [0.003]	-0.587** [0.244]	-0.276 [0.280]	-0.664*** [0.203]
Treatment x 1981	0.008 [0.008]	0.006 [0.007]	-0.001 [0.004]	-0.035 [0.375]	0.177 [0.527]	-0.350 [0.265]	-0.001 [0.004]	0.001 [0.004]	-0.003 [0.004]	-0.422* [0.246]	-0.259 [0.309]	-0.382* [0.217]
Treatment x 1991	0.006 [0.008]	-0.001 [0.007]	-0.002 [0.004]	-0.225 [0.362]	0.091 [0.509]	-0.464* [0.276]	-0.004 [0.004]	-0.003 [0.004]	-0.006 [0.004]	-0.466** [0.229]	-0.419 [0.274]	-0.441** [0.205]
Mean Y Control Group	0.05	0.05	0.05	2.85	2.75	3.14	0.05	0.05	0.05	3.05	2.75	3.14
Number of Towns	417	234	910	417	234	910	652	234	910	652	234	910
Observations	2078	1166	4543	2078	1166	4543	3253	1166	4543	3253	1166	4543

Notes: The Table reports coefficients β_t from the panel RD Equation (1), which controls for year $\times$ 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 β of 1951. Columns 1-6 control for a linear polynomial in distance. Columns 7-12 control for a quadratic polynomial in latitude and longitude. Sources are the decadal economic and population censuses. Units of observation are town-years. The samples consist 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.

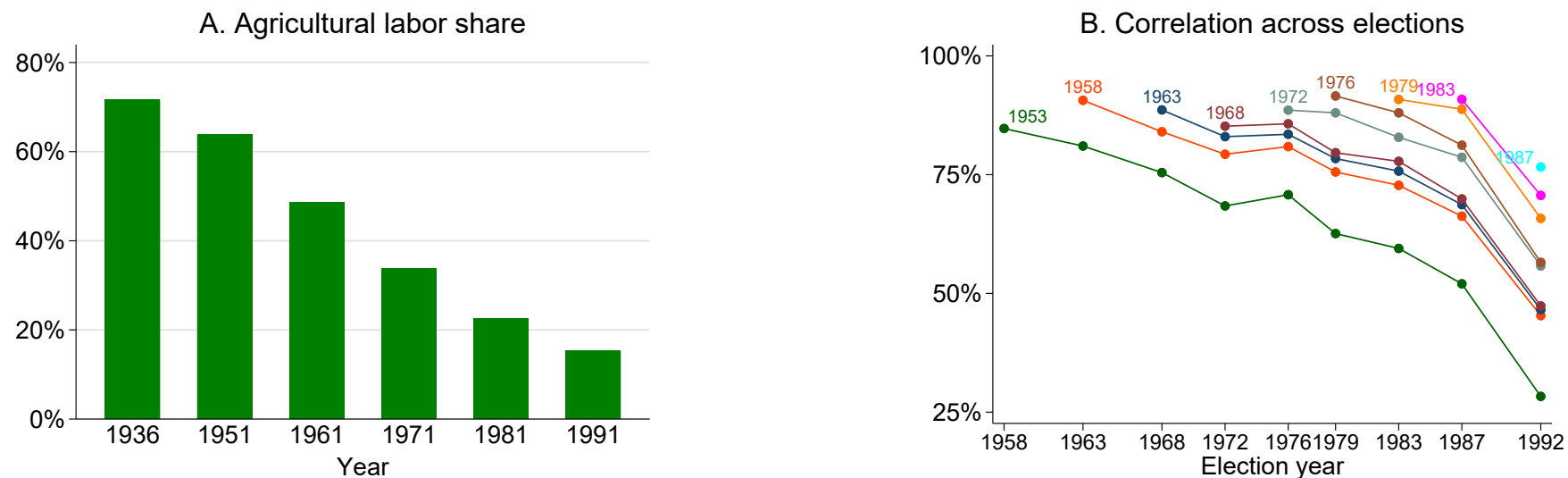
D.15 Economic Conservatism and Changing society

Figure D.19: The impact of the reform on Forza Italia/center-right vote share after 1992.



Notes: The graphs report coefficients β_t of the RD 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.20: 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.