Government Transfers and Votes for State Intervention[*]

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August, 2023

Abstract

Government transfers might have long-lasting consequences on electoral outcomes. We study a regional policy implemented in Italy over the second half of the twentieth century and leverage variation in transfers to show that parties promoting more state intervention in the economy performed better in the targeted areas relative to places that were not subsidized, decades after the end of the policy. This effect does not seem to mirror long-term differences in the economic performance of treated and control areas, which were small, nor other attitudes within the electorate.

JEL codes: H71, H76, I38, P16, R58

Government transfers have historically been a widely used policy tool and have been analyzed in several branches of the economic literature. An established finding in political economy is that transfers tend to generate electoral rewards for the incumbent government that promotes them (Manacorda et al., 2011; Pop-Eleches and Pop-Eleches, 2012; De La O, 2013). This paper moves one step further to investigate whether the electoral effect of transfers persists over time and goes beyond pro-incumbent voting. Do communities that have benefitted of transfers in the past continue to support welfare policies, regardless of which party proposes these policies? More in general, can past exposure to state intervention lead to better electoral results for parties advocating a more active role of the state in the economy, long after the end of the intervention?

To shed light on the role of transfers in shaping voters' support for state intervention in the long term,[1] we study a large place-based policy, the *extraordinary intervention*, conducted in Italy over the second half of the twentieth century. The program was initiated in 1950 by the incumbent Christian Democratic Party and carried out by a state-owned agency called *Cassa per il Mezzogiorno* (CasMez). Between 1950 and 1992, the CasMez conveyed large amounts of financial resources – in the form of firm subsidies and infrastructure spending – towards backward areas of Southern Italy to stimulate economic development. Initially led by a technocratic steering committee, the management of the CasMez shifted over the years in the hands of local politicians. Starting from the mid 1970s, CasMez resources progressively became welfare transfers to local communities (Felice and Lepore, 2017).

We study national elections taking place after the end of the *extraordinary intervention* in 1992 and document that parties with pro-state, welfare-oriented platforms performed better in municipalities previously more exposed to CasMez aid. This result holds on average across elections and is most pronounced more than two decades after the termination of the program in the context of the 2013 general election, which after a very long time elicited voters' views on the role of the state in the economy. This followed the first appearance of a new party (*5-Star Movement*) as a strong contender to mainstream parties and proposing a large redistributive program, more regulation and less competition in the domestic market. To capture voters' support for state intervention, we construct a party-level measure by exploiting the scores developed by the Manifesto Project (Volkens et al., 2019), which denote the position of parties across different categories based on their manifesto. Among the categories included in the Manifesto Project's classification, we focus on those capturing a party's pro-state position ("Market regula-

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[1]To ease exposition, throughout the text we will use the expression "voters' support for state intervention" as a shorthand for "voters' support for parties promoting state intervention in the economy".

tion", "Economic planning", "Controlled economy", "Nationalisation" and "Welfare state expansion"), which we then combine in a composite score. This score is then mapped at the municipality level using party vote shares to build the main outcome. We show that the salience of topics related to state intervention in the political debate, as measured by the cross-party variance in the composite score for each election, attains its largest value in 2013.

The specific locations targeted by regional policies are not randomly picked but tend to differ from other areas in terms of economic dynamism and other local conditions (Neumark and Simpson, 2015). To the extent that such differences are correlated with voting outcomes in the future, a simple unconditional comparison of treated (subsidized) versus untreated municipalities gives rise to selection issues. To isolate exogenous variation in transfers, we exploit the historical border separating the CasMez territorial jurisdiction from the rest of Italy and run a spatial regression discontinuity experiment. The border was set by the policymaker in 1950 and did not change until the end of the policy in 1992. It does not systematically overlap with the administrative borders that delimit Italian regions and has never been considered under the EU cohesion policy, nor for any other place-based program in Italy. Thanks to historical records of parliamentary discussions, we further document that the setting of the border was largely informed by technical reasons related to the execution of infrastructure projects and therefore likely immune to political interests, which we confirm by inspecting discontinuities in electoral outcomes before 1950. We also fail to detect meaningful jumps in baseline geographic, economic and demographic covariates at the cutoff. These considerations suggest that differences in outcomes across this border arguably refer to the past exposure to CasMez intervention and not to other, possibly unobserved treatments.

We first pool together all general elections after the end of the policy in 1992 and document a positive jump in voters' support for state intervention at the CasMez border on average across elections. The estimated effect is however not very large at 10 percent of a standard deviation. We then narrow our focus to the 2013 vote and estimate a larger discontinuity, equivalent to about 85 percent of a standard deviation. These results are remarkably stable across specifications and estimation methods, and survive a large battery of robustness tests. Importantly, we show that there are little or no discontinuities in voters' views on a diverse range of topics made available by the Manifesto Project, which alleviates concerns that our findings are driven by other attitudes within the electorate. We show in particular that there is no jump in a score, constructed again using the Manifesto categories, aimed at capturing voters' anti-establishment attitudes. Indeed, our results for 2013 might be driven by differential populist stances, which were also on the

rise during those years particularly within the 5-Star Movement's electorate. We perform two more placebo exercises to make sure that our measure of voters' support for state intervention does not mistakenly reflect their anti-establishment attitudes. First, we observe that the 1994 votes for Forza Italia, a right-wing party that ran for the first time at the 1994 general election with a strong anti-establishment narrative but no pro-state orientation, are not robustly associated with CasMez intervention. Second, we show that the votes share of the 5-Star Movement is balanced between treated and control municipalities in the context of the 2014 European Parliament election, which arguably elicited the anti-establishment, eurosceptic attitudes of the Movement's voters rather than their views on the economy.

We then focus on economic outcomes. Our evidence suggests that the policy mildly stimulated employment while it was in place but failed to induce self-sustained economic benefits. We detect no substantial discontinuity in the employment rate, income per capita and education levels at the CasMez border in 2011 (just before the 2013 vote). We document more meaningful effects on the composition of employment, as the policy promoted a transition from agriculture into industry over the second half of the twentieth century. A positive discontinuity in the industry share of employment of about 5 percentage points at the CasMez border is still visible in 2011. We also observe sizable differences in population trends between treated and control areas. We document large declines in population between 1951 and 2011 in municipalities north of the CasMez border, versus zero or even slightly positive population growth just south of the border over the same period. Data available after the policy years show that fertility and mortality rates are overall similar between treated and control municipalities, which suggests that differential migration flows (most likely in the form of lower emigration rates south of the border) have led to the observed patterns in population.

In the last part of the paper we discuss potential mechanisms at the basis of our findings. Key channels that have been highlighted in the political economy literature such as reciprocity (Finan and Schechter, 2012) or poor information of voters (Manacorda et al., 2011) rely on a direct relationship between the government that enacted the policy and voters/recipients. These channels can be ruled out in our set-up, as virtually all of the parties in place during the *extraordinary intervention* disappeared from the Italian political landscape following corruption scandals in the early 1990s. Alternative drivers must then be at the basis of this persistent effect of transfers on voting outcomes. A first potential channel is economic status, which in theoretical models plays a central role in determining support for state intervention (Meltzer and Richard, 1981; Benabou and Ok,

2001).[2] However, we do not observe substantial differences in economic performance between treated and control municipalities in the long term. We thus ask ourselves what might explain that residents of subsidized areas show more support to political platforms promoting more state intervention in the economy, even though previous intervention does not seem to have triggered self-sustained economic gains. A key possibility is that the past effects of the intervention are still reflected in the economic and demographic structure of the targeted municipalities, which as a consequence might differ from other areas in the composition of voters. For example, parties promoting more state intervention might perform better in areas with larger industry base (such as those south of the CasMez border), as the decline in manufacturing that has occurred in Italy (and most advanced economies) over the past decades induced voters in those areas to demand more protection from the state. Another potential driver is selective migration triggered by the policy at the border, with people more in favor of state intervention deciding to settle relatively more in treated areas in response to transfers. Specifically for our case, our results may be explained by lower emigration rates in subsidized municipalities if those who stayed did so in response (or thanks) to CasMez intervention. A last channel we consider is that prolonged exposure to state intervention has directly affected individual preferences towards the role of the state in the economy. The inability to disentangle the precise mechanism is undoubtedly a limitation of this paper, which however rules out important candidate drivers and offers potential explanations that can be tested in future research.

Our work contributes to several strands of the literature. First, it relates to the political economy studies exploring the interplay between government policies and voting behavior (for a review see De La O, 2013). In recent work, Slattery (2022) estimates a positive impact of subsidy giving on votes for the incumbent in the United States. Specifically for Italy, Caprettini et al. (2021) document large and persistent electoral gains for the incumbent party following a land redistribution reform enacted in some areas of Italy in the early 1950s. Colussi et al. (2020) show that the *extraordinary intervention* led to pro-incumbent voting in more subsidized municipalities. While the existing literature largely focuses on pro-incumbent voting, we illustrate that the effect of public transfers on voting outcomes might extend beyond electoral returns for the incumbent government and remain visible in voters' behavior long after the party that proposed the original transfers ceases to exist.[3] Our investigation also contributes to the stream of literature analyzing

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[2] For instance, a below-median earner is expected to be in favor of redistribution because she is going to benefit from it.

[3] A related paper, also stressing how the impact of economic policy on voting can be persistent, is Carillo (2022), which shows how infrastructure investments performed by the Fascist regime boosted votes

the determinants of preferences for redistribution and the welfare state (for a review see Alesina and Giuliano, 2011). As highlighted above, the key determinant of these preferences identified in theoretical models is the economic status. However, more recent studies have emphasized the role of historical and environmental factors. The closest to our paper is probably Alesina and Fuchs-Schündeln (2007), which investigates the role of political regimes as a determinant of preferences for state intervention. The authors document that people that have lived under the Communist regime in East Germany display more favorable attitudes towards the role of the state in providing social services relative to West Germans after reunification.

Our paper also relates to the literature assessing the role of regional programs and government transfers. These policies are widespread all over the world and their effects on economic growth have been widely explored (e.g., Becker et al., 2010; Busso et al., 2013; Kline and Moretti, 2014; Bianchi and Giorcelli, 2021). There is indeed a hot debate, both in the US and Europe, on the need for more regional transfers. Recent socio-economic shocks have been unevenly distributed across territories (Becker et al., 2017; Rodríguez-Pose, 2018), in a context where market-based convergence mechanisms, such as the flow of people to high-income regions and of capital toward poorer areas, work only imperfectly (Austin et al., 2018). Rajan (2019) suggests that regional interventions are indeed a powerful tool to support local communities as relevant elements of a healthy market economy. Our work produces novel evidence about the economic effects of a relevant place-based policy - the Italian *extraordinary intervention* - while it was in place and long after its termination. This policy has been the largest attempt at tackling the North-South gap in the Italian history and one of the most ambitious place-based programs ever conducted in developed economies over the last decades (Felice and Lepore, 2017). Our paper, while not evaluating the aggregate welfare consequences of the policy, is among the first casting some light on its reduced form causal effects on economic outcomes.[4] Much less evidence exists instead about the impact of government transfers on voting, especially in the long run. Our contribution introduces a new perspective on the debate on place-based intervention, by observing that it can have a long-lasting impact on electoral outcomes. Importantly, we show that a program aimed at jumpstarting economic development in backwards areas had (unintended) consequences on voting outcomes that persisted long after its termination.

The paper is organized as follows. Section 1 provides a brief historical overview of

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not only for the Fascist party at the time, but also for neo-fascist movements after decades.

[4]Other recent studies estimating the economic impact of the *extraordinary intervention* with alternative identification strategies (not based on the CasMez border) also find small effects in the long run (Colussi et al., 2020; Buscemi and Romani, 2022).

the *extraordinary intervention* in the South of Italy. Section 2 illustrates how we construct the main outcome. Section 3 discusses identification and Section 4 presents the empirical analysis. Section 5 discusses the potential mechanisms behind our findings. The last section concludes.

1 Historical background

Reducing the stark divide between Southern regions and the rest of the country was a pressing issue for the Italian policymakers in the aftermath of World War II. A regional policy was then introduced in 1950 under the name of *extraordinary intervention*, with the goal of promoting self-sustained development for the lagging South. The government agency in charge of the intervention was the *Cassa per il Mezzogiorno* (CasMez), established in 1950 with an initial ten-year mandate and charged with the management of ample financial endowments. CasMez expenditures have been estimated at slightly less than 1 percent of Italy's GDP, on average each year over the four decades of the *extraordinary intervention* (Felice and Lepore, 2017).[5]

During its first decade of activity, the agency's mandate was that of providing southern territories with basic infrastructures. The CasMez managed the execution of investments in a range of domains including transport, water supply networks and agriculture. A new phase of the *extraordinary intervention* began in the 1960s, when the mandate shifted towards the direct promotion of industrial development.[6] Grants were disbursed to finance firm investments for building new plants, enlarging existing ones or purchasing machinery. Infrastructure intervention remained part of the business, but its primary target gradually shifted from agriculture to the needs of the industrial sector.

CasMez expenditures throughout the 1950s and the 1960s were managed by an independent and centralized technical committee. Starting in the 1970s, however, the autonomy of the agency was progressively hampered as the newly instituted regional governments played a more and more prominent role into the *extraordinary intervention*. Many of the decision-making prerogatives shifted to regional policymakers and local bureaucrats, who gradually replaced CasMez technicians in the planning and evaluation of the interventions.[7] The cost of the program jumped from a total of around 49 billion euros

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[5]In per capita terms, CasMez expenses amounted to roughly 200 real euros (2011 prices) yearly. They compare well with other very generous regional policies, such as the EU Structural Funds Program (1989-present; Becker et al., 2010) and the German *Zonenrandgebiet* (1971-1990; von Ehrlich and Seidel, 2018).

[6]See Law n. 634/1957 and Law n. 555/1959.

[7]See Law n. 717/1965 and Law n. 853/1971. Borgomeo (2018) studies the determinants of the allocation of CasMez funds. By means of a regression discontinuity design exploiting close elections, the author shows that the allocation was responsive to political incentives. A more recent paper (Buscemi and

(2011 prices) disbursed between 1950 and 1970 to almost 120 billion euros from 1971 to 1986 (Felice and Lepore, 2017). The *extraordinary intervention* was gradually phased out and officially terminated in 1992, as the large and complex system of state holdings was being dismantled or privatized.[8]

2   The 2013 Italian general election: a vote on state intervention

We seek a suitable outcome variable that captures voters' support for parties promoting more state intervention, long after the termination of the policy. However, these views are unlikely to be elicited from the electorate to the same extent across election years, as the political debate might revolve around themes that are unrelated with the role of the state in the economy. In this regard, we argue that the 2013 Italian general (parliamentary) election provides an ideal set-up. The appearance of a new political faction, the 5-Star Movement, in the national political arena as a strong contender to mainstream parties forcefully directed the public debate in the run-up to the 2013 vote towards welfare issues and the role of the state in the economy. The positions of other parties, and in turn those of the electorate, on these topics were elicited in a way that was arguably unprecedented in the recent political history of the country. Looking at how different Italian parties, characterized by contrasting views on these issues, fared at the 2013 election arguably provides a suitable measure of voters' support for state intervention.

The platform of the 5-Star Movement was centered upon the so-called *reddito di cittadinanza* (citizen's income), a monetary transfer in favor of low-income, unemployed households. The salience of this policy proposal in the Movement's agenda is clear from the words of the Movement's leader Beppe Grillo, two weeks before the election to be held in late February: "*The first thing we will do, after entering the Parliament, is to introduce a citizen's income to save people*".[9] For the first time in the recent political history of the country, a relevant party put redistribution at the top of its agenda. The Movement put forward many other proposals explicitly aimed at hardening regulation and thwarting market competition. For instance, listed in their manifesto were the introduction of salary caps for managers of listed companies, as well as a proposal "preventing the

Romani, 2022) documents that regional governments politically aligned with the central administration received more CasMez funds.

[8]The CasMez was shut down in 1984 and replaced by another state-owned agency called AgenSud (*Agenzia per la promozione e lo sviluppo del Mezzogiorno*), which remained in place until the end of the program in 1992. Because the prerogatives of the AgenSud were in practice identical to those of the CasMez, to ease exposition we will refer to the agency in charge of the program solely as CasMez.

[9]See https://basicincome.org/news/2013/03/italy-5-star-movement-and-the-confusing-proposal-of-a-citizens-income and https://www.altalex.com/documents/news/2013/02/13/elezioni-grillo-primo-provvedimento-m5s-sara-reddito-di-cittadinanza.

dismantlement of manufacturing firms active predominantly in the domestic market".
This sparked broad and unprecedented public interest into welfare policies and, more in
general, towards the role of the state in the economy.[10]

The votes share of the 5-Star Movement at the 2013 election would be a natural, yet
imperfect measure of voters' support for pro-state party platforms. This is because wel-
fare policies and regulation were not the only electoral promises brought about by the
5-Star Movement, hence they might not have been the sole drivers of the party's perfor-
mance at the polls. Indeed, the Movement's platform included a few other innovative
proposals such as a focus on renewable energy sources, the removal of party funding and
even a referendum on euro membership. More importantly, we are concerned that the
marked anti-establishment connotation of the 5-Star Movement also played a key role
in determining the party's performance at the 2013 election.[11] A second reason why we
do not use the votes share of the 5-Star Movement as our main outcome is that welfare-
related instances were present in the political manifestos of other parties, albeit with less
urgency and clamor than for the 5-Star Movement. This would imply that only looking
at the Movement's votes share would deliver an incomplete picture of the overall support
for state intervention of the Italian voters.

We thus consider alternative, more suitable outcomes. We build an index of vot-
ers' support for state intervention using party-specific scores developed by the Manifesto
Project (Volkens et al., 2019), envisaged to capture how particular economic and social
categories are supported across political platforms. The Manifesto Project is a large-scale
initiative that collects data on the programmatic supply of over 1,000 parties from 1945
until today in more than 50 countries, by covering several topics related to political ide-
ology and party preferences. Specifically, for each party and election year, the score asso-
ciated to a particular category (e.g., support for environmental protection) is computed
using the incidence of sentences related to that category in the party's publicly available
manifesto. We focus on five categories that denote a party's support for state intervention
in the economy: "Market regulation", "Economic planning", "Controlled economy", "Na-
tionalisation" and "Welfare state expansion" (described in Table 1). We then aggregate
these scores to obtain the overall incidence of pro-state sentences in a party's manifesto.
The cross-party variance in this composite score roughly measures the extent to which

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[10]Other factors further corroborate the idea that the views of parties and voters on the role of state in
the economy were strongly elicited in 2013. For instance, the Italian electoral law in 2013 was based on a
proportional system where voters could express their preference only for a list and not an individual. In
addition, the 2013 vote came at the end of a two-year technocratic government that, in the aftermath of the
sovereign debt crisis, promoted rigid austerity measures.

[11]In this regard, we will provide evidence that past exposure to CasMez aid does not seem to relate to
the populist attitudes of voters, while it is robustly associated with their support for state intervention.

parties differentiate from each other on their pro-state positions - a proxy for the salience of state intervention topics in the political debate. Figure 1 plots the cross-party variance of the composite score across election years and shows that it peaks exactly at the 2013 vote. This evidence, albeit suggestive, confirms the idea that views on state intervention were likely elicited more strongly from the electorate in 2013 than in other election years after the end of the *extraordinary intervention*.[12] We then obtain our main outcome variable by standardizing the composite score between 0 and 1 to ensure comparability over time and combining it with local party shares in each election year. In formulae:

$$stateint_m^t = \sum_j share_{j,m}^t \cdot manifesto_j^t \tag{1}$$

Where $stateint_m^t$ measures support for state intervention in municipality m and election year t, $share_{j,m}^t$ is the votes share of party j in municipality m and election year t and $manifesto_j^t$ is party j's (standardized) composite Manifesto score for election year t.[13] We compute this index every year including, importantly, those prior to the beginning of the policy to test the balancing properties of our outcome. While the 2013 vote will be a key focus for the reasons outlined above, we will exploit the full depth of the Manifesto Project's archives and show baseline results and robustness tests for the sample of all elections after the end of the policy.

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[12]We perform the same exercise for many other Manifesto scores, for which we compare the cross-party variance across election years. Virtually none of the scores show a pattern similar to that of state intervention in Figure 1 - the only exceptions are "Nationalism", "Culture" and "Equality" - see Figure A1 in the Online Appendix. Figure A2 in the Online Appendix reproduces the salience pattern for the composite state intervention score of Figure 1 across all national elections since 1946. Even over this much longer time period, the 2013 vote stands out as the one where the cross-party variance in the composite score is highest.

[13]We will only use the variable computed in Equation 1 as our outcome throughout the analysis, since single components could be affected by random errors and statistical noise in their coding (Benoit et al., 2009; Volkens et al., 2013). Online Appendix Table A1 reports the raw (non-standardized) value of the composite score for each party at the 2013 election. Appendix Table A2 shows descriptive statistics for the outcome computed in Equation 1 in the CasMez area in 2013.

Table 1. Manifesto scores

Score	Description
Market regulation	"Support for policies designed to create a fair and open economic market"
Economic planning	"Favourable mentions of long-standing economic planning by the government"
Controlled economy	"Support for direct government control of economy"
Nationalisation	"Favourable mentions of government ownership of industries, either partial or complete; calls for keeping nationalised industries in state hand or nationalising currently private industries"
Welfare state expansion	"Favourable mentions of need to introduce, maintain or expand any public social service or social security scheme"

Description of the Manifesto scores used to compute the index of voters' support for state intervention in Equation 1. More details available in the Manifesto Project Dataset – Codebook (Version 2019b).

Figure 1. Salience of state intervention across election years (1994-2018)
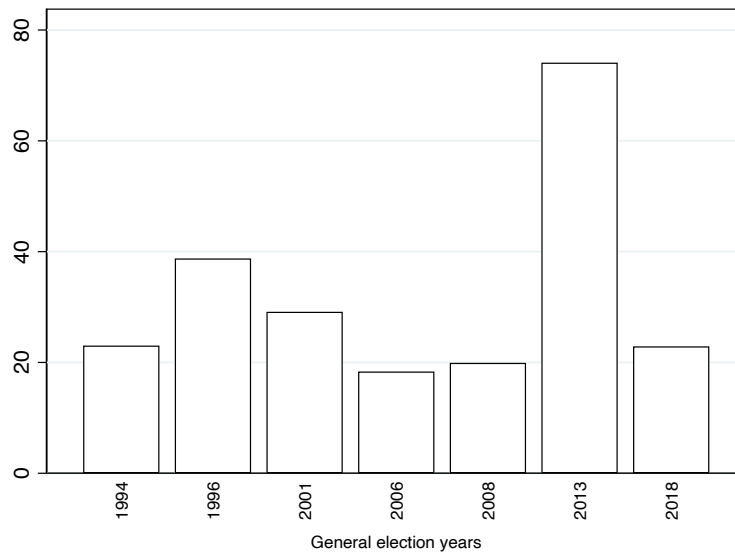
Each bar measures the variance of the composite Manifesto score across parties for each election year. The composite score is the sum of the five Manifesto scores described in Table 1. See text for details.

2.1 Other data sources

We obtain detailed information about the universe of CasMez activities between 1950 and 1992 from the ASET database, which reports the type of intervention (firm transfer or infrastructure project), the year of approval and the total financial resources allocated.[14]

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[14]The ASET (Archives for Economic and Regional Development) Project has been launched in 2013 with the goal of cataloguing all activities performed within the *extraordinary intervention*.

Conveniently, we can geocode these interventions at the municipality level. We thus collapse the data to obtain a dataset reporting CasMez transfers between 1950 and 1992 for around 3,000 municipalities located in ten Italian regions.[15] Data on voting at all general elections between 1946 and 2018 is sourced from the Italian Ministry of Interior.[16] We complement this dataset with a rich set of controls for geographic, demographic and economic characteristics for each municipality, sourced from decennial census data starting in 1951. Further details about the data and sources are provided in the online appendix.

3 Identification

Identifying the causal effect of a place-based policy on voting outcomes is challenging. Places targeted by public transfers tend to differ systematically from other areas. For example, the policymaker might intervene more intensively in poorer regions, or channel larger sums of money towards politically connected municipalities. These differences between locations might be correlated with electoral outcomes and generate spurious results. In turn, this will invalidate any empirical strategy that simply compares with each other municipalities that are differentially exposed to transfers. Controlling for municipality-level characteristics overcomes this challenge only in part, as long as the allocation mechanism remains unknown and unobserved confounders are not ruled out.

To identify the effect of interest, we exploit the definition of the program's territorial jurisdiction as a source of exogenous variation in CasMez transfers. The Italian South is conventionally referred to as the area encompassed by the six southernmost regions of the country and the islands of Sicily and Sardinia. This region is separated from the rest of Italy by the upper borders of Abruzzo, Campania and Molise (Figure 2, Panel (a)). At the time of inauguration of the *extraordinary intervention* and definition of the covered area, however, the policymaker set the northern boundary of the CasMez jurisdiction above those administrative borders to include some neighboring municipalities in Lazio and Marche (the orange line in Figure 2, Panel (a)). This area was defined in 1950 (a time

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[15] Abruzzo, Basilicata, Calabria, Campania, Lazio, Marche, Molise, Apulia, Sardinia and Sicily. All these regions were fully part of the CasMez jurisdiction except for Lazio and Marche, for which only some municipalities were included (more on this in Section 3). A small number of interventions carried out in some islands of Tuscany are excluded from the sample. We leverage the spatial variation in transfers within the CasMez area more explicitly in Online Appendix D.

[16] We do not focus on local (regional or municipal) elections, for which the debate typically revolves around local issues. By contrast, national parliamentary elections take place at the same time throughout the country and ensure comparability across municipalities, as all voters express their views on topics of national interest. Indeed, the Manifesto Project classification is available only for national elections. In addition, the Ministry of Interior reports data on general elections since 1946 (before the beginning of the *extraordinary intervention*); the data for local elections is only available since 1970 (regional elections) or 1989 (municipal elections).

when the program was supposed to last for ten years only) and remained unchanged until the termination of the policy in 1992.[17] Panel (b) of Figure 2 provides a clear depiction of our "first stage". It plots (log) CasMez transfers cumulated between 1950 and 1992 for each Italian municipality, in thousand euros (2011 prices) per 1951 inhabitant, against the geodetic distance to the border over a (symmetric) 100 kilometers (km) window. A sizable jump in transfers of about 10,000 euros per capita can be noticed at the border.[18]

Figure 2. The CasMez border

(a) The border   (b) (Log) CasMez transfers at the border
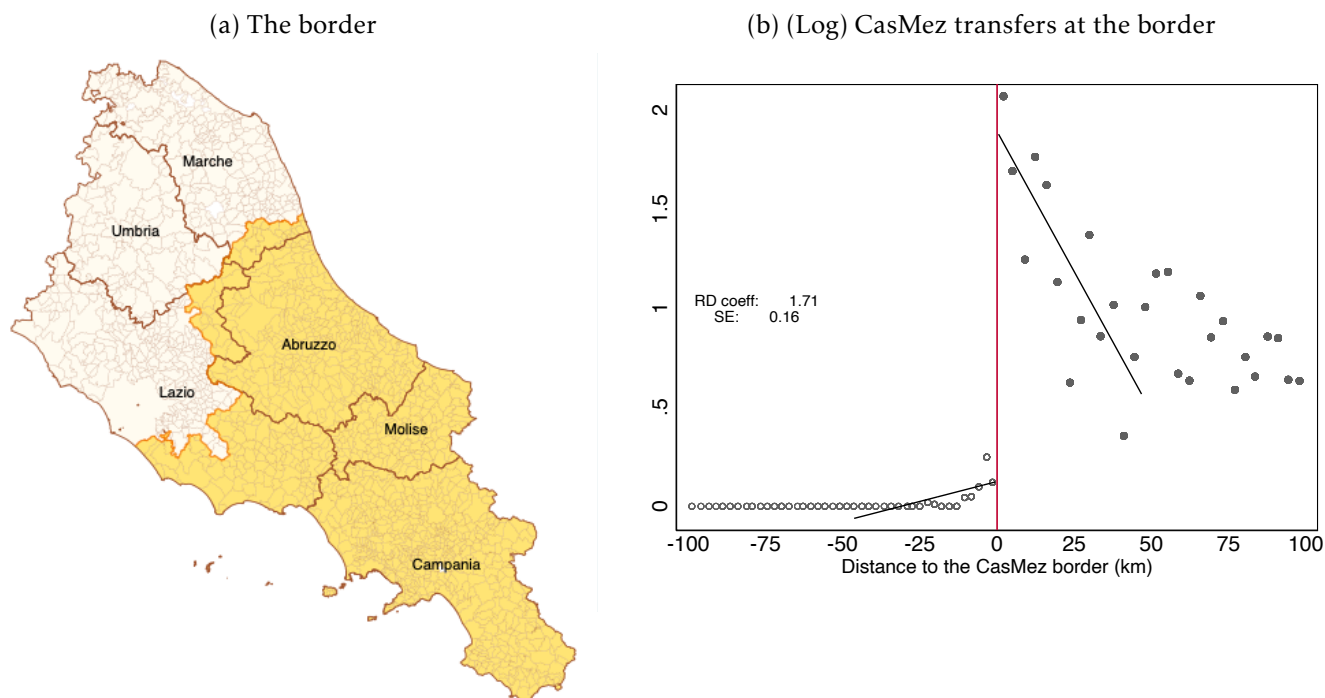
Panel (a) shows the CasMez border in orange. The brown lines denote regional (NUTS-2) boundaries. Panel (b) shows CasMez transfers in (log) thousand euros (2011 prices), cumulated between 1950 and 1992 and scaled by population in 1951. Negative distance denotes municipalities north of the border. The dots are binned means of the outcome computed within disjoint, evenly-spaced bins of the running variable. The optimal number of bins is chosen in a data driven way that mimics the variability of the underlying data (Calonico et al., 2015). The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border. The RD estimate and standard error are obtained from a regression of the outcome on an indicator variable taking value of one for municipalities south of the border, controlling for distance to the border. The polynomial is estimated over a bandwidth of 47 km, obtained applying the algorithm described in Calonico et al. (2014) to the pooled sample of general elections between 1946 and 2018 using support for state intervention as outcome variable and controlling for province, border segment and election year effects (see Section 4 for details).

This jump in government transfers gives rise to a spatial sharp regression discontinuity (RD) design where the CasMez border $\mathbb{B}$ constitutes a two-dimensional discon-

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[17]Online Appendix Figure B1 shows the full jurisdiction of the program.

[18]The small uptick in transfers just north of the cutoff is due to some neighborhoods in the municipality of Rome, which was not fully part of the CasMez jurisdiction. Also, the high value of transfers just south of the border is largely driven by two municipalities (Pomezia and Aprilia) that received very generous subsidies and had a relatively small population. Our results are unchanged when Rome, Pomezia and Aprilia are excluded from the estimation sample.

tinuity in latitude-longitude space that separates the treated area $\mathbb{A}^t$ from the control area $\mathbb{A}^c$. Let the spatial location of the centroid of municipality m be denoted by the latitude-longitude pair $\ell_m = (l_{x,m}, l_{y,m})$. Treatment status is a deterministic function of a municipality's location, which acts as running variable: $T_m = \mathbb{1}[\ell_m \in \mathbb{A}^t]$. Differently from standard RD designs, the running variable in geographic RD is two-dimensional. We collapse it to a one-dimensional metric $\delta_m \equiv d(\ell_m, \mathbb{B})$, computed as the (geodetic) distance between the centroid of municipality m and the closest point on the treatment boundary (Imbens and Zajonc, 2011). Negative distance is assigned to municipalities north of the border, such that $T_m = \mathbb{1}[\ell_m \in \mathbb{A}^t] = \mathbb{1}[\delta_m \geq 0]$. The main identifying assumption behind this approach is the continuity of potential outcomes at the CasMez border. This assumption requires relevant factors (other than the treatment) not to change discontinuously at the border, so that municipalities just north of it make for an appropriate counterfactual for those subsidized (Imbens and Lemieux, 2008).[19]

To provide a first test of the continuity assumption, we look for imbalances at the cutoff by plotting relevant observable characteristics in its vicinity. Panel (a) of Figure 3 shows little discontinuities in labor market and demographic outcomes at the onset of the policy in 1951. The industry share of employment was similar north and south of the cutoff, suggesting that the choice of the border was not informed by the industrial potential of the subsidized areas. Areas south of the border had slightly lower employment and urban population and larger age dependency ratio, but the binned means are fairly continuous at the cutoff. Treated and control municipalities were also similar in terms of education levels and population density. Panel (b) plots the geographic characteristics of municipalities around the border and electoral outcomes for the 1948 election, which took place shortly before the the beginning of the *extraordinary intervention*. Both mean elevation and slope rise as one moves from north to south of the border, but no substantial discontinuity occurs at the cutoff itself. The binned means for voting outcomes also seem overall smooth around the cutoff, although we observe a downtick in the votes share for the incumbent Christian Democrats in 1948 just south of the border (mirrored by an uptick for Communists and Socialists). We address this issue explicitly in Appendix C, where we show that results are unchanged when excluding the (very few) municipalities with unusually large share of votes for the opposition. Most importantly, we find no imbalance in the index of support for state intervention for the 1948 election.[20]

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[19]Selective sorting, according to which units manipulate the running variable to be just above or below the cutoff, is not a concern in our design. Indeed, a McCrary (2008) test reveals no discontinuity in the density of the running variable at the cutoff (Appendix Figure B2). The policy might have induced sorting of individuals at the border, which is discussed below as one of the possible mechanisms driving the results.

[20]We show RD plots for additional variables in Online Appendix Figure B3.

Figure 3. CasMez border - balancing

(a) Labor markets and demography, 1951

(b) Geography and electoral outcomes

Negative distance denotes municipalities north of the border. The dots are binned means of the outcome computed within disjoint, evenly-spaced bins of the running variable. The optimal number of bins is chosen in a data driven way that mimics the variability of the underlying data (Calonico et al., 2015). The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border. The RD estimate and standard error are obtained from a regression of the outcome on an indicator variable taking value of one for municipalities south of the border, controlling for distance to the border. The polynomial is estimated over a bandwidth of 47 km, obtained applying the algorithm described in Calonico et al. (2014) to the pooled sample of general elections between 1946 and 2018 using support for state intervention as outcome variable and controlling for province, border segment and election year effects (see Section 4 for details). Panel (a): "High school education" denotes the share of people aged at least 6 with high school education or more. "Population density" measured as number of inhabitants per km^2. "Age dependency ratio" is the share of those aged below 14 and above 65 as a share of those aged 15-64. "Urban population" denotes the share of resident population living in cities. Panel (b): "Mean elevation" and "Slope" are measured in meters. "Slope" is the difference between the highest and the lowest point in a given municipality. The "Socialist and Communist share" denotes votes for the Popular Democratic Front (which included both the Communist and the Socialist party) in 1948. "Support for state intervention, 1948" is the index of voters' support for state intervention computed using Equation 1 for the 1948 election, accounting for province and border segment effects. See text for details.

A number of institutional features further point to the validity of the continuity hypothesis in this case. First, by inspecting historical records of parliamentary discussions prior to the setting of the border, we document that the choice of the additional municipalities to be included in the CasMez jurisdiction was informed by technical reasons related to the execution of some infrastructure projects.[21] Importantly, at the time the border was set, the *extraordinary intervention* had a well-defined lifespan of only ten

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[21]For example, a part of the regional border between Abruzzo and Marche would have cut in two parts

years and was meant to carry out basic infrastructure works exclusively. Our main focus (transfers) became part of the CasMez range of interventions only in the 1960s and was not even discussed before then. Arguably, prospects of economic development and even short-term political considerations were unlikely to be key concerns when the exact location of the CasMez border was being discussed.[22] Second, the geographic cutoff we exploit does not coincide with other relevant administrative and/or historical borders. The inclusion of municipalities in Southern Lazio and Marche (Figure 2) implies that the CasMez border does not systematically separate regions (NUTS-2) or provinces (NUTS-3). In fact, the border separates administrative units as small as municipalities and there is little reason to expect systematic differences between the many pairs of municipalities located along the border (for a similar argument see von Ehrlich and Seidel, 2018). In addition, no other policy conducted by the Italian government before, during or after the *extraordinary intervention* varies discontinuously through the border, nor do EU regional programs and structural funds. Our cutoff does not coincide with past relevant geographic discontinuities, such as the "Gothic line" and the "Gustav line", exploited in Fontana et al. (2017) as a discontinuity in the duration and intensity of Nazi occupation during World War II, or the historical border that until the country's unification in 1861 separated the Kingdom of the Two Sicilies from the rest of Italy (Alfani and Sardone, 2015; d'Adda and de Blasio, 2017).

That said, some segments of the CasMez border do overlap with NUTS-2 boundaries (those between Lazio, Umbria and Abruzzo) and with the border of the old Kingdom of the Two Sicilies. This implies that municipalities located close to these segments could suffer from a "compound treatment" issue (Keele and Titiunik, 2015) as the observed effect on voting outcomes might be driven, at least in part, by systematic differences between treated and control municipalities that are unrelated to CasMez intervention. In turn, this would make it impossible to separately identify the effect of the policy for these municipalities. For this reason, our baseline estimates will exclude municipalities

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a mountain basin and the river generated from it (Tronto river). Given that the entire area was planned to undergo a reclamation project, all municipalities belonging to that area were included in the program's jurisdiction. A similar rationale led to the extension of the border to annex some municipalities in Lazio (Latina reclamation area - see Cervone-Villa Law draft, 1953).

[22]In contrast, the allocation of funds among municipalities *within* the CasMez area was more likely also informed by political rationales, as the incumbent government often targeted places where support for the opposition parties was higher (Colussi et al., 2020). While there is arguably less scope for political intrusion in the choice of the border itself, in Appendix C we account for the possibility that this choice was driven, at least in some cases, by potentially endogenous political considerations. Specifically, we show that our results still hold when excluding municipalities just south of the CasMez border where electoral support for the Christian Democratic government at the onset of the policy was particularly low compared to that for the Communist and Socialist party - in other words, municipalities that might have been added to the CasMez jurisdiction only to win votes back from opposition parties.

close to segments of the CasMez border that coincide with either regional boundaries or with the old Kingdom border, although results will not vary substantially when these municipalities are included.

4 Estimation and results

We now present the main results, both in graphical form and regression estimates. The baseline specification is a sharp regression discontinuity (RD) design, run either on the pooled sample of all elections after the end of the policy (1994-2018) (Equation 2a) or separately for each election, with a focus on the 2013 vote (Equation 2b):

$$stateint_{m,p,b}^{t} = \alpha + \beta \cdot T_{m,p,b} + \varphi(\delta_{m,p,b}) + \gamma_p + \theta_b + \tau_t + \epsilon_{m,p,b,t} \tag{2a}$$

$$stateint_{m,p,b}^{2013} = \alpha + \beta \cdot T_{m,p,b} + \varphi(\delta_{m,p,b}) + \gamma_p + \theta_b + \epsilon_{m,p,b} \tag{2b}$$

Where $stateint_{m,p,b}^{t}$ captures voters' support for state intervention in election year t as measured in Equation 1 for municipality m in province p and closest to border segment b and $T_{m,p,b}$ is the treatment variable taking value of one if municipality m belongs to the CasMez area and zero otherwise. In either specification, $\varphi(\delta_{m,p,b})$ is a linear polynomial in the geodetic distance between municipality m's centroid and the closest point of the border, γ_p are province (NUTS-3) fixed effects and θ_b are fixed effects associated with three border segments.[23] Equation 2a is run on a pooled sample of elections and also accounts for election year dummies τ_t. A symmetric bandwidth of 47 km north and south of the CasMez border will be used throughout the analysis. This derives from applying the optimal bandwidth selection procedure described in Calonico et al. (2014) to Equation 2a, estimated on the pooled sample of all general elections between 1946 and 2018.[24] As noted above, the baseline analysis excludes municipalities close to segments of the CasMez border overlapping with NUTS-2 boundaries or with the old border of the Kingdom of the Two Sicilies.[25]

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[23]We follow Gelman and Imbens (2019) and choose a low order for the polynomial control function. More flexible specifications are tested in the robustness checks. We obtain border segment effects by splitting the CasMez border in three blocks with an equal number of coordinate pairs.

[24]We choose to derive the optimal bandwidth using the pooled sample of all elections, including those before and during the policy, as we will also report estimates for those elections years. The optimal bandwidth computed over the pooled sample of elections exclusively after the end of the *extraordinary intervention* is equal to 54.5 km. As we will show, results are robust to the choice of the bandwidth.

[25]In practice, we exclude segments of the CasMez border that coincide with either regional delimitations or with the old Kingdom border and obtain a "trimmed" CasMez border. We then compute, for each municipality, the distance to the trimmed border. In the last step, we exclude municipalities whose distance to the trimmed border is larger than their distance to the full CasMez border, that is, those municipalities that

Figure 4 illustrates the behavior of our main outcome – the index capturing voters' support for state intervention – on the pooled sample of elections after the end of the policy (top panel) and specifically for the 2013 vote (bottom panel) - a time when, according to our previous discussion, views on the role of the state in the economy were strongly elicited from the electorate. A small positive discontinuity in the outcome at the cutoff can be noticed already on average across the pooled sample of post-CasMez elections. The jump becomes however more visible when looking at the 2013 vote in isolation.[26]

We quantify these discontinuities in Table 2. We begin by showing regression estimates of the β coefficient for the pooled sample of elections after the end of the *extraordinary intervention* (1994-2018) as estimated by Equation 2a (Panel (a), Column (1)). The results confirm the previous graphical evidence, showing a significant effect of CasMez status on voters' support for state intervention in the years after the end of the policy. The estimated effect is admittedly small at 1.7 points, or 10 percent of a standard deviation in the estimation sample. The RD estimate rises substantially in Column (2), which shows the estimation output for the baseline specification of Equation 2b run specifically for the 2013 election. We estimate a jump of 4.1 points in the index, equivalent to 11 percent of the mean and 85 percent of a standard deviation in the estimation sample.[27] In Panel (b) of Table 2 we implement the non-parametric estimation method proposed in Calonico et al. (2014), where each municipality is weighted using a triangular kernel function giving larger weight to those closer to the border. The estimated coefficient rises overall and remains significant, both in the pooled regression estimates and when focusing on the 2013 vote.[28]

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are closer to segments of the border that overlap with the other "problematic" cutoffs. This rule excludes 198 municipalities, or 35 percent of the total number of municipalities (558) located 47 km north and south of the CasMez border.

[26]This discontinuity might encompass a negative spillover to municipalities north of the border. This implies, as in von Ehrlich and Seidel (2018), that we are unable to estimate the net effect of the policy, but rather a gross effect that includes this spillover.

[27]We also estimate Equation 2b separately for each general election after the end of the policy and plot the resulting estimates for the β coefficient in Appendix Figure B4. The estimated effect is overall positive across years but often non-significant. The second largest positive effect on voters' support for state intervention is estimated for the 1996 vote. Indeed, themes related to state intervention were particularly salient in 1996 according to the cross-party variance in the composite Manifesto score, which attains its second-largest value for the post-CasMez period (after 2013) in 1996 (Figure 1).

[28]The baseline estimates of Table 2 are relative to the sample that excludes municipalities close to segments of the CasMez border coinciding with regional borders or with the border of the Kingdom of the Two Sicilies. Appendix Table B1 shows that results are very similar when focusing on the entire CasMez border.

Figure 4. Support for state intervention at the CasMez border
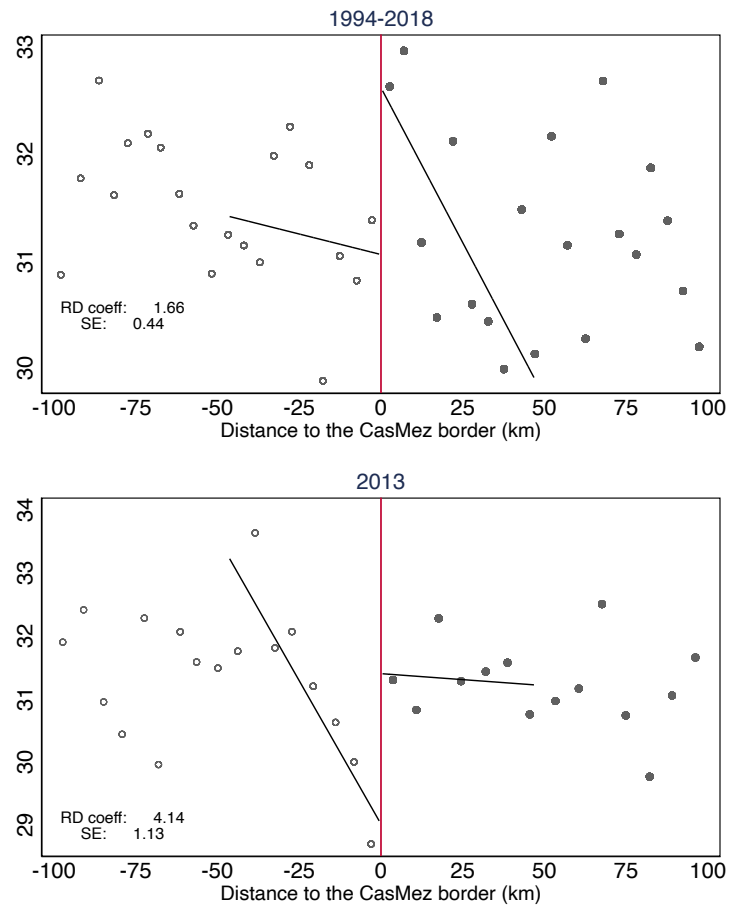
Support for state intervention measured as described in Equation 1. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border on the optimal bandwidth and accounting for province and border segment effects. In the top panel, which pools all election years after the end of the *extraordinary intervention* (1994-2018), we also account for election year effects. See Figure 3 and text for details.

Table 2. Baseline RD estimates

| Outcome variable:	(1)	(2)
Support for state intervention	All elections 1994-2018	2013 election
	Panel (a): Parametric (linear) estimates	
RD estimate	1.66	4.14
	(0.44)	(1.13)
	Panel (b): Non-parametric estimates	
RD estimate	2.21	4.38
	(0.57)	(1.35)
Bandwidth (km)	46.96	46.96
Observations	2470	360
Mean	46.42	38.31
Standard deviation	16.72	4.89

RD estimate associated with a dummy variable equal to one for municipalities belonging to the CasMez area. The dependent variable is the index of voters' support for state intervention obtained in Equation 1. Column (1) reports the estimation output when pooling all election years in the post-CasMez period. Column (2) focuses on the 2013 general election. Panel (a) presents parametric estimates resulting from estimating Equation 2a for Column (1) and Equation 2b for Column (2), respectively. In both cases we specify a linear RD polynomial and report robust standard errors in parentheses. Panel (b) uses the non-parametric estimation and robust bias-corrected inference method proposed by Calonico et al. (2014). For the non-parametric estimates we present the bias-corrected point estimate along with the robust standard error (the conventional point estimate and standard error are, respectively, 1.81 and 0.39 for the pooled 1994-2018 sample and 4.57 and 0.91 for the 2013 election). All regressions are run on the baseline 47-km bandwidth. Descriptive statistics are always computed within the estimation sample. See text for details.

4.1 Robustness exercises

We now focus on the baseline estimates of Panel (a) in Table 2 and perform a battery of robustness tests. We first show the sensitivity to the estimation bandwidth in Figure 5, where we plot the RD coefficient and confidence intervals as each specification (Equation 2a for the pooled sample of post-CasMez elections, Equation 2b for the 2013 vote) is estimated on varying symmetric bandwidths from 10 to 100 km around the CasMez border. Overall, the estimates do not seem particularly sensitive to the choice of the bandwidth. The estimated coefficient is larger but more imprecisely estimated for smaller windows around the border, and stabilizes as the bandwidth reaches about 50 km. Table 3 shows additional checks. In Columns (1) and (2), we test our results when controlling for a quadratic or a cubic (rather than linear) function of the distance to the border. The estimated coefficient tends to rise when more flexible control functions are used. In Columns (3) and (4) we drop municipalities within 5 and 15 km of the border, respectively. This "donut hole" exercise ensures that our findings are not entirely driven by spillovers be-

tween nearby municipalities at the boundary. When excluding municipalities very close to the border (Column (3)) the effect remains roughly similar to the baseline estimate in both magnitude and significance. As more municipalities are excluded, the estimated discontinuity shrinks and becomes imprecisely estimated, although we would caution that comparability between the treated and control group decreases as the hole gets larger. In Columns (5) and (6) we shift the border south and north of the original one and re-estimate our baseline specification. The estimated effect on voters' support for state intervention is small relative to the baseline estimates and not significant, suggesting that no discontinuities occur at these placebo cutoffs. Last, in Column (7), standard errors are corrected to allow for spatial correlation using Conley (1999)'s procedure, with no meaningful difference relative to the baseline estimates.

Figure 5. RD estimate, robustness to bandwidth choice
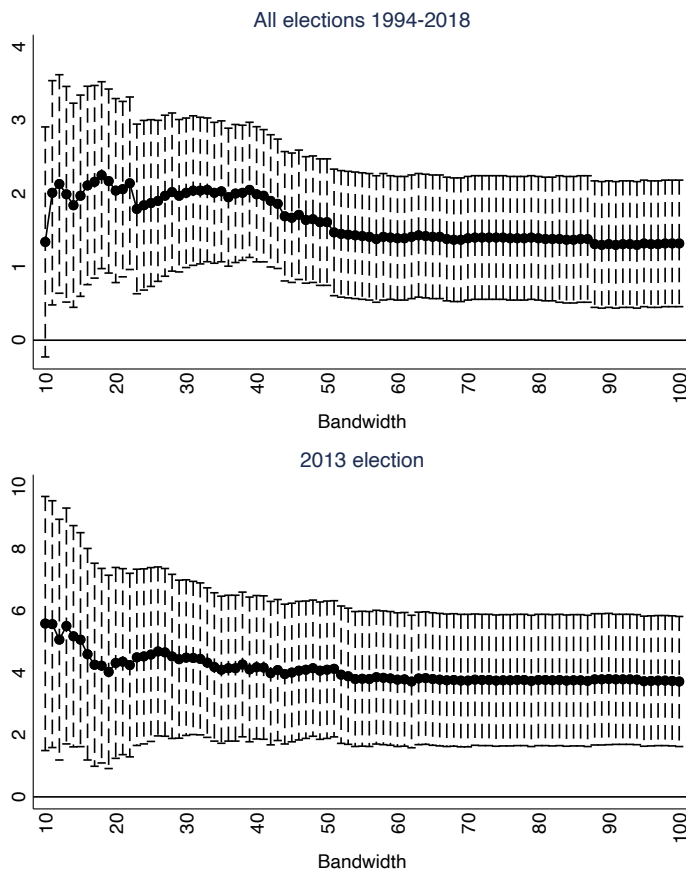
The top panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2a run for the pooled sample of post-CasMez elections, at varying symmetric bandwidths around the CasMez border (in each consecutive regression, the bandwidth is increased by 1 km). The bottom panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2b run for the 2013 election. See text for details.

Table 3. Support for state intervention - Robustness tests

| | RD control function | | "Donut hole" | | Placebo cutoffs | | Spatial SEs |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel (a): All elections 1994-2018						
RD estimate	1.52	2.21	1.81	0.11	0.83	-0.52	1.66
	(0.44)	(0.55)	(0.67)	(1.60)	(0.52)	(0.62)	(0.70)
Observations	2470	2470	2009	1289	4046	1795	2470
Mean	46.42	46.42	46.32	46.12	45.85	47.27	46.42
Standard deviation	16.72	16.72	16.83	17.12	17.73	15.73	16.72
	Panel (b): 2013 election						
RD estimate	4.51	4.13	4.13	3.32	0.00	1.82	4.14
	(1.13)	(1.34)	(1.09)	(2.04)	(1.18)	(1.49)	(1.57)
Observations	360	360	294	190	600	257	360
Mean	38.31	38.31	38.67	38.81	37.00	39.59	38.31
Standard deviation	4.89	4.89	4.56	4.43	5.15	4.06	4.89
Bandwidth (km)	46.96	46.96	46.96	46.96	78.90	43.84	46.96
Polynomial order	2	3	1	1	1	1	1
Donut hole (km)	0	0	5	15	0	0	0

Replication of Panel (a) in Table 2, robustness tests. All results are related to the index for support for state intervention for the pooled sample of elections between 1994 and 2018 (Panel (a)) and the 2013 election only (Panel (b)). Columns (1)-(2) use a more flexible specification for the RD polynomial. Columns (3)-(4) perform donut-hole RD regressions excluding municipalities in a neighborhood of the cutoff. Columns (5)-(6) use placebo cutoffs located 47 km south and north of the CasMez border, respectively. In Columns (5)-(6) we use the optimal bandwidth specific for the placebo border, derived using the same algorithm described in Section 4 for the pooled sample of post-CasMez elections. Column (7) allows for spatially clustered standard errors using Conley (1999) procedure and picks a 8-km radius that maximizes the standard errors, as suggested in Colella et al. (2019). In Panel (a), the specification in Column (7) also allows for arbitrary correlation across years. Descriptive statistics are always computed within the estimation sample. See Table 2 and text for details.

As argued in Section 2, our focus on the 2013 vote is due to the large emphasis that state intervention in the economy had in the political debate at that time, which in turn elicited voters' views on this topic more strongly than in other general elections. That said, our estimates might still incidentally pick up other attitudes among the electorate that are unrelated with their support for state intervention. Importantly, the index we compute partly reflects the votes share of the 5-Star Movement, which at the 2013 election was featuring the highest degree of support for state intervention among the running parties. Indeed, we also detect a large (around 4 percentage points) and significant jump in the Movement's electoral performance at the CasMez border (Table 4, Column (1)). We therefore exploit the Manifesto archives to construct indices of voters' positions along other dimensions. Among these are indicators that capture views on the European Union, free markets, concerns over government efficiency and political corruption or national-

ist attitudes. We describe all the indicators we collect from the Manifesto database in Appendix Table B2. These scores are again mapped at the municipality level using local party shares as described in Equation 1. Figure 6 shows the corresponding RD estimates for each indicator, both for the pooled sample of post-CasMez elections (top panel) and specifically for 2013 (bottom panel). For nearly all indicators, the estimated jump at the border is small and not statistically significant.[29] In particular, we show that electoral support for parties proposing a more transparent and efficient government - a rough measure of voters' anti-establishment attitudes - does not jump at the CasMez border. This is an important test as the strong populist rhetoric was another distinctive feature of the 5-Star Movement's propaganda, hence our results might be contaminated by populist attitudes also associated with voting for the Movement. To complement this evidence we perform two additional placebo checks, showed in Table 4. First, we look at the experience of Forza Italia, a right-wing party which first ran for election in 1994. This historical comparison is particularly suited to our purposes. On the one hand, the strong populist rhetoric of Forza Italia as a new player in the political arena in 1994 (Jones and Pasquino, 2015) compares well with that of the 5-Star Movement in 2013.[30] On the other, Forza Italia was not advocating more state intervention in the economy as was the Movement in 2013. Testing whether the support for Forza Italia in 1994 varies discontinuously at the CasMez border thus serves as a convenient placebo check. Column (2) in Table 4 documents no meaningful discontinuity. In a second test, we examine the 5-Star Movement's performance at the 2014 European Parliament election, which took place not long after the 2013 vote but arguably elicited voters' anti-establishment attitudes rather than their views on state intervention. In that occasion, the Movement was primarily endorsing a radical renovation of European institutions rather than calling for more public intervention in the economy. In fact, the Movement was part of a political coalition (Europe of Freedom and Direct Democracy) that included parties from the opposite side of the political spectrum.[31] The members of this coalition were likely at poles apart in their views on the role of the state in the economy, but were united by marked eurosceptic, anti-establishment positions. The votes share of the 5-Star Movement at the 2014 European election thus provides, in our view, a rather clean measure of anti-establishment voting. If, as we claim, our results are not reflecting differential anti-establishment attitudes, then we should observe little or no jump in votes for the Movement at the CasMez

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[29]Appendix Tables B3 and B4 report the RD estimates used in Figure 6. Figure B5 shows the RD plots for 2013. The largest and most significant discontinuity we estimate is in the "Environmental protection" index - another relevant theme in the 5-Star Movement's manifesto.

[30]Durante et al. (2019) suggest that supporters of these parties have similar human and social capital.

[31]These included right-wing parties such as the UK Independence Party and the Sweden Democrats.

border in 2014. Indeed, this is confirmed in Column (3) of Table 4. Taken together, these results provide reassuring pieces of evidence that our findings largely reflect the impact of transfers on voters' support for state intervention, rather than on populist attitudes or other positions within the electorate.

Figure 6. Other views in the electorate
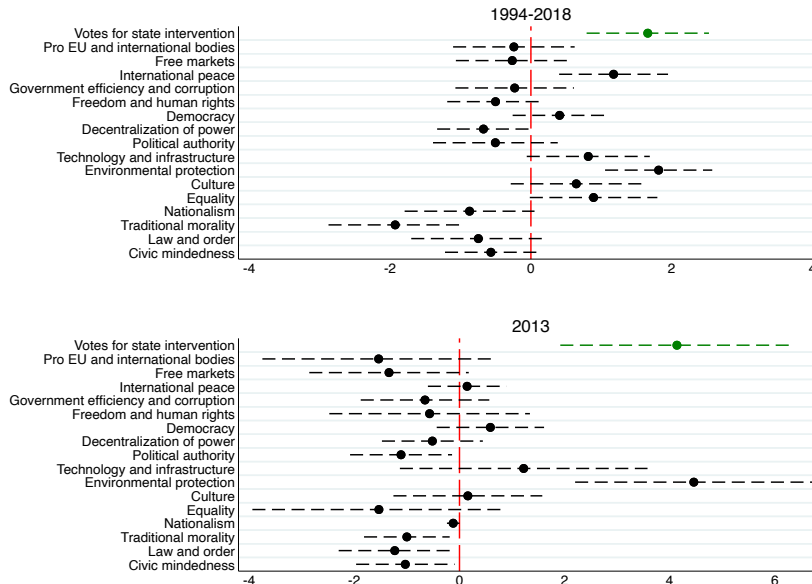
Each index is computed by weighing the Manifesto Project score with party vote shares at each national election using Equation 1. The top panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2a run for the pooled sample of post-CasMez elections. The bottom panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2b run for the 2013 election. Appendix Table B2 describes each score. See Table 2 and text for details.

Table 4. Populism

	(1) 5-Star Movement votes share (%), 2013	(2) Forza Italia votes share (%), 1994	(3) 5-Star Movement votes share (%), 2014
RD estimate	4.38	2.17	1.37
	(1.26)	(1.37)	(0.87)
Bandwidth (km)	46.96	46.96	46.96
Observations	360	359	360
Mean	27.77	19.08	14.42
Standard deviation	5.76	5.67	3.91

Estimation output of Equation 2b using the optimal bandwidth. Column (1) uses the 5-Star Movement votes share at the 2013 general election. Column (2) uses the electoral share of the Forza Italia party at the 1994 general election. Column (3) uses the 5-Star Movement votes share at the 2014 European Parliament election. Robust standard errors in parentheses. Descriptive statistics are always computed within the estimation sample. See Table 2 and text for details.

In a last exercise, we estimate Equation 2a on two different samples - the pooled sample of elections before (1946-1950) and during (1950-1992) the *extraordinary intervention*. We present results in Table 5. The discontinuity is very close to zero in the years prior to the intervention (Column (1)) and becomes positive during the policy years (Column (2)), although the coefficient is small at less than one tenth of a standard deviation and only weakly significant. Column (3) reports again the coefficient for the post-CasMez years (that of Panel (a), Column (1) in Table 2), which is of similar magnitude but more precisely estimated than that for the policy period.[32] As noted above, our research question is whether the effects of government transfers on electoral outcomes persist over time, and especially after the termination of the policy and regardless of whether the party that promoted the transfers is still running. By contrast, the contemporaneous effect on voters' support for state intervention might be influenced by the short-term economic effects of the program, or by rewarding incentives (more on this below). In addition, we would also caution that voters' views on state intervention might not be clearly elicited at each election in the same way, as noticed in Section 2. Hence our focus on the 2013 vote, which occurred two decades after the end of the intervention and provides an ideal set-up for our analysis. We place instead less emphasis on the result in Column (2) of Table 5 as it refers to a period when the policy was still in place and the Christian Democratic party, which was in charge of the program, was still running for government.

Table 5. Support for state intervention - different estimation periods

	(1) Before CasMez (1946-1950)	(2) During CasMez (1950-1992)	(3) After CasMez (1994-2018)
RD estimate	0.87	0.76	1.66
	(1.75)	(0.44)	(0.44)
Bandwidth (km)	46.96	46.96	46.96
Observations	682	3513	2470
Mean	19.21	54.73	46.42
Standard deviation	15.04	12.40	16.72

Estimation output of Equation 2a using the optimal bandwidth. Robust standard errors in parentheses. Descriptive statistics are always computed within the estimation sample. See Table 2 and text for details.

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[32]Online Appendix Figure B6 reports coefficient estimates separately for each election. The estimated discontinuity during the policy years is almost always null or positive, with the important exception of 1983 when we observe a negative jump. Reassuringly, state intervention did not seem to be a particularly salient topic in that year (see Figure A2), suggesting that the discontinuity in 1983 might not reflect different support for state intervention but other factors.

4.2 Economic outcomes

We now examine whether the *extraordinary intervention* had any effect on the economies of the subsidized areas while it was in place and after its termination. Figure 7 shows the impact on labor markets by plotting the employment rate and employment shares over the last phase of the policy (1981 and 1991) and two decades after its termination (2011). The employment rate slightly increased in treated areas while the policy was in place - we notice a positive, although small discontinuity in 1981 and 1991 (quantified at around 2.5 percentage points, see Appendix Table B5), but this difference between treated and control areas fully disappears by 2011. The effect on employment shares is instead more easily discernible as a workforce shift out of agriculture into industry. This result is in line with those found by Kline and Moretti (2014) for the case of the Tennessee Valley Authority – a regional policy from which the *extraordinary intervention* explicitly took inspiration. By the end of the policy in 1991, the share of employment in the primary and secondary sector in the treated areas was 14 percentage points smaller and 13 points larger than in control areas, respectively. In 2011, these differences are more muted but still visible at 5-6 percentage points smaller agriculture share and larger industry share. In Figure 8 we extend our focus to other economic and demographic outcomes, such as income per capita, inequality (Gini coefficient), the share of high school educated and the share of public employees. All these variables, potentially affected by the policy, are measured in 2011 and could potentially be correlated with electoral outcomes in 2013. In line with the results on employment, we again fail to detect sizable discontinuities between municipalities north and south of the border in 2011. In particular there is no meaningful difference in the share of public employees, suggesting that the intervention did not fuel the development of a more prominent public sector.[33]

Last, we investigate whether the policy induced differential population trends at the border by plotting population growth relative to 1951 over time in Figure 9. First, we notice that the entire area under analysis has experienced a decline in population during the policy years. Second, we observe a large positive jump at the CasMez border from the 1980s onwards, suggesting that municipalities just south of the border have experienced less severe depopulation relative to those north of it.[34] This effect is persistent through time, consistent with the findings in Schumann (2014) for the case of a population shock in post-war Germany. The observed patterns in population growth can be

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[33]The last row of Table B5 shows the corresponding estimates. These findings are by and large confirmed by the several robustness checks, presented in Appendix Tables B6 and B7, that test their stability under various specifications.

[34]Appendix Table B8 shows the corresponding coefficient estimates.

driven by differences in fertility and mortality rates, or by differential migration patterns at the cutoff. Municipality-level data, available only after 1991, suggest that neither fertility nor mortality rates were substantially larger or smaller in treated municipalities in the years following the end of the policy (Table B8).[35] We are unfortunately unable to assess whether the policy led to different fertility and mortality rates or, more interestingly, triggered migrations flows across the border while it was in place. That said, the evidence on population growth shows substantial differences between treated and control areas (of 50-60 percent compound growth over six decades). Taken together, and assuming that fertility and mortality rates were similar north and south of the border also before 1991, these results suggest that migration outcomes likely differed between treated and control areas during the policy years – most likely in terms of lower emigration rates south of the CasMez border.

Figure 7. Impact on labor markets
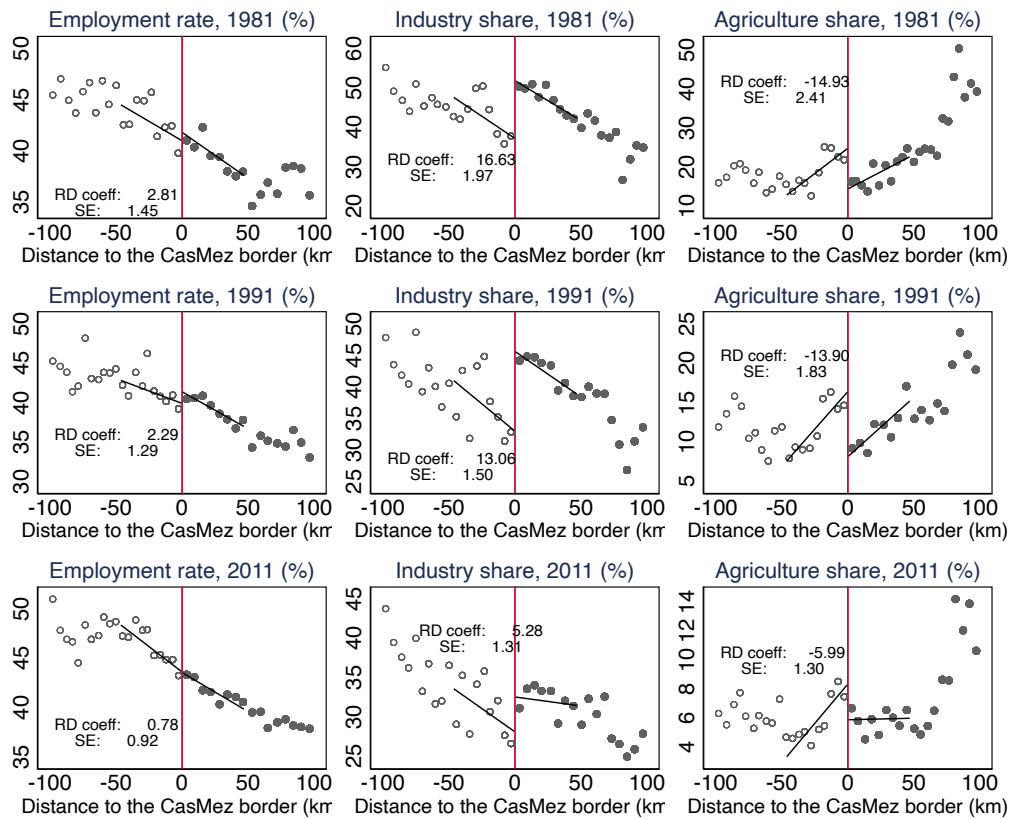
The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border. See Figure 3 and text for details.

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[35]Table B8 also documents that population density was larger south of the border in the immediate aftermath of the policy, consistent with the findings on population growth.

Figure 8. Economic outcomes, 2011

"Income per capita" is measured as taxable income per taxpayer in 2011 (thousand euros). "High school education" denotes the share of people aged at least 6 with high school education or more. "Public employees" is the number of public workers per 1000 people in 2011. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border. See Figure 3 and text for details.

Figure 9. Population growth relative to 1951
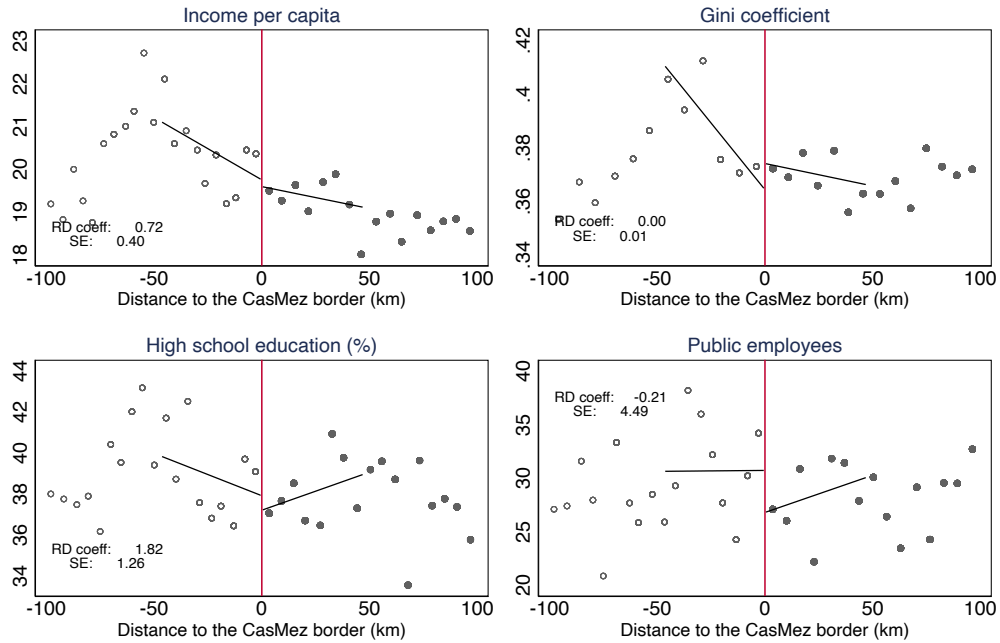

The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border. See Figure 3 and text for details.
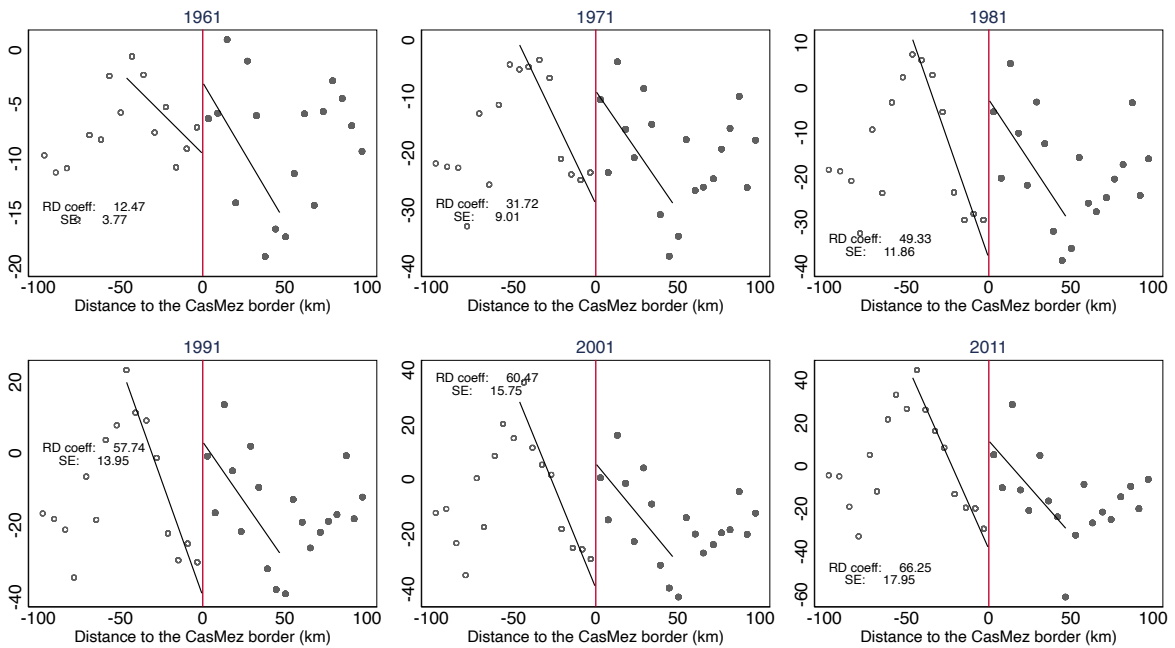
4.3 The Industrial Zones

The above analysis comes with some limitations associated with the use of a spatial RD design. The identified effects are local to the CasMez frontier, which inevitably lowers their external validity. Moreover, the design compares municipalities within the CasMez area to other municipalities outside of it, which did not happen to receive any transfer. Another policy-relevant question is whether a marginal increase in transfers influences voting outcomes in the long term. In the attempt to overcome these limitations, in Appendix D we place ourselves within the CasMez jurisdiction and exploit variation in transfers across municipalities. Such variation is provided by the so-called Industrial Zones, groups of municipalities targeted as suitable hosts for industrial clusters where the CasMez could concede (by law) more generous transfers to firms. We exploit the criteria for the establishment of a Zone in a propensity score matching design that compares municipalities that were part of a Zone to observationally similar municipalities that were not included in these areas and received much less transfers. Our results, presented in Appendix D, are qualitatively in line with the RD estimates.

5 Discussion

Our analysis highlights a link between public transfers and voting outcomes long after transfers have elapsed. We now discuss the possible mechanisms at the basis of this result. Standard channels proposed in the political economy literature cannot explain our findings as they rely on a direct relationship between the politicians in charge of the policy and the electorate.[36] Importantly, the Christian Democratic Party, which promoted the *extraordinary intervention*, disappeared from the political scene in 1994. In fact, a brand-new political landscape emerged in Italy following corruption scandals in the early 1990s. Our findings are then likely to reflect a shift in voters' support for more state intervention in the economy, regardless of the party advancing these proposals.

The first mechanism potentially underlying our results could have been that the policy had permanently affected economic performance in the targeted region. The longstanding literature that has studied the determinants of preferences for state intervention has identified economic conditions as a key factor, hence a better understanding of our results does require some insight into the economic effects of the program. An increase in public transfers should mechanically result into higher wealth and consumption possi-

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[36]Two main channels are reciprocity (Finan and Schechter, 2012), according to which voters reward politicians to which they feel indebted; and rational but poorly informed voters who use policy to infer politicians' views (Manacorda et al., 2011).

bilities in the targeted region. The implications on voters' demand for state intervention in the long-term depend on whether such wealth gains are persistent. To the extent that the economic benefits to subsidized areas relative to other areas are self-sustained, then our findings would most certainly reflect them. For example, economic theory suggests that residents would have less incentives to support welfare policies if local economic conditions are relatively good. As previously noted, however, we fail to find convincing evidence that economic performance differed between treated and control areas in the long run. More precisely, we observe a (small) positive discontinuity in the employment rate of treated regions until 1991, but this effect dissipates in the long run (2011). The positive (albeit small) effect on employment while the policy was in place, combined with possible rises in income and consumption during those years (which we cannot measure), may partly explain why we fail to detect large effects on support for state intervention during the policy years (Table 5). If economic performance was better in treated areas than in control areas, there might have been less incentives for voters to demand a more active role of the state in the economy. However, when focusing on the longer run (2011), we observe an almost negligible effect of the policy on economic outcomes that could potentially have affected support for state intervention during those years.

This result raises an important question. Why would people vote for more state intervention even in the absence of long-term economic benefits? A first possibility is that the impact of the policy on past economic outcomes has persistent effects on voting. For example, the transient economic stimulus in subsidized areas while the policy was in place might have induced higher accumulation of human capital, which could have in turn affected voting. Education outcomes, which should reflect human capital accumulation, do not differ substantially north and south of the border in 2011, suggesting that this channel is unlikely to be at play. A related explanation, which is unfortunately hard to test, is that voters were still mindful of the (short-lived) stimulus that followed the provision of subsidies in their area. The memory of the economic gains resulting from government aid in the past might in turn have made individuals more favorable to pro-state parties, despite the policy's ineffectiveness in the long run.

Another possibility is that the past effects of the intervention are still reflected in the economic and demographic structure of the subsidized areas, thus leading to a different composition of the electorate. For example, we estimate a tangible effect on employment shares, with treated municipalities showing a disproportionately larger industry share and lower agriculture share than control ones. These differences are quite large during the policy years and remain visible in 2011, although to a lesser extent. The well-documented decline in manufacturing witnessed by many developed economies over the

last decade (Gagliardi et al., 2022) might explain, at least in part, why voting outcomes in areas with larger industry base are relatively more favorable to parties proposing state intervention and welfare policies. The composition of voters around the CasMez border might also have changed as a consequence of differential migration trends induced by the *extraordinary intervention*, for instance in the form of selective migration of people in treated areas in response to transfers.[37] While we are unable to assess directly whether the policy led to migration around the border while it was in place due to lack of data, our evidence shows that population growth was substantially larger in treated municipalities. Within a region subject to a gradual decrease in population over the past decades, areas just south of the CasMez border experienced little population changes in contrast with massive declines north of the border. Assuming small differences in fertility and mortality rates, this evidence seems to suggest larger net migration in treated areas most likely in the form of lower emigration relative to control areas. In turn, these patterns might partly explain our results if, for example, people more favorable to state intervention remained south of the CasMez border (or moved there) rather than north of it. This interesting mechanism is unfortunately hard to test with the data at hand.

A further possible mechanism is that individual attitudes towards the role of the state in the economy have responded to past state intervention, which has in turn translated into voting for pro-state parties.[38] Simply put, prolonged state presence in a community can tilt local preferences in favor of a more proactive role of the state in the economy. In particular, theoretical models have stressed that government intervention can alter perceptions about the role of effort as a driver of individual success, which might then be at the basis of this preference shift.[39] We unfortunately lack sufficient data to perform a credible investigation of this channel. We show in Appendix E suggestive evidence that arises when matching our dataset with individual-level survey data from the World Values Survey. We observe that survey respondents living south of the CasMez border tend to support state intervention more and agree more with the statement that luck and connections, relative to effort, determine wealth. These results should however be interpreted with caution because of the small sample size, and warrant further investigation.

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[37]It should be noticed that these migration patterns are a potential mechanism and do not undermine the causal interpretation of our results (von Ehrlich and Seidel, 2018).

[38]This potential driver of preferences for state intervention has so far been explored almost exclusively in the theoretical literature. As noted above, some evidence in this regard has been provided by Alesina and Fuchs-Schündeln (2007) in the context of Germany, where the decades-long experience of the Communist regime in the East has led people to support state intervention more than in West Germany.

[39]If a society believes that effort has only a little role in determining wealth, it will further reinvigorate public intervention in the economy, possibly making these beliefs self-sustained (Alesina and Angeletos, 2005; Benabou, 2008).

6 Conclusion

This paper illustrates that government transfers can have a persistent impact on voting outcomes. We focus on a large place-based program run in Italy from 1950 to 1992 and show that parties proposing a more prominent role of the state in the economy perform better in subsidized communities than elsewhere, even long after the end of the program. This result is particularly marked for the 2013 general election – more than two decades after the termination of the policy and when views on state intervention were strongly elicited within the electorate. Our findings survive several robustness checks and do not appear to reflect differences in other voter preferences, including anti-establishment attitudes. Because the parties that promoted the policy had long disappeared from the political scene, standard explanations offered in political economy cannot rationalize our findings. We also illustrate that the program led to no tangible difference in economic performance between treated and control areas in the long-term, thus ruling out a key driver of support for state intervention. While identifying the precise mechanism underlying our results is challenging, we propose a number of candidate channels related to past effects of the policy, changes in voters' composition in targeted areas and possible shifts in individual attitudes towards the role of the state. Our contribution stresses an important, possibly unintended consequence of place-based policies; future work employing more granular data sources could shed further light on the underlying channels.

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A   Appendix A

Appendix Figure A1. Salience of other topics across election years (1994-2018)

(a)   (b)
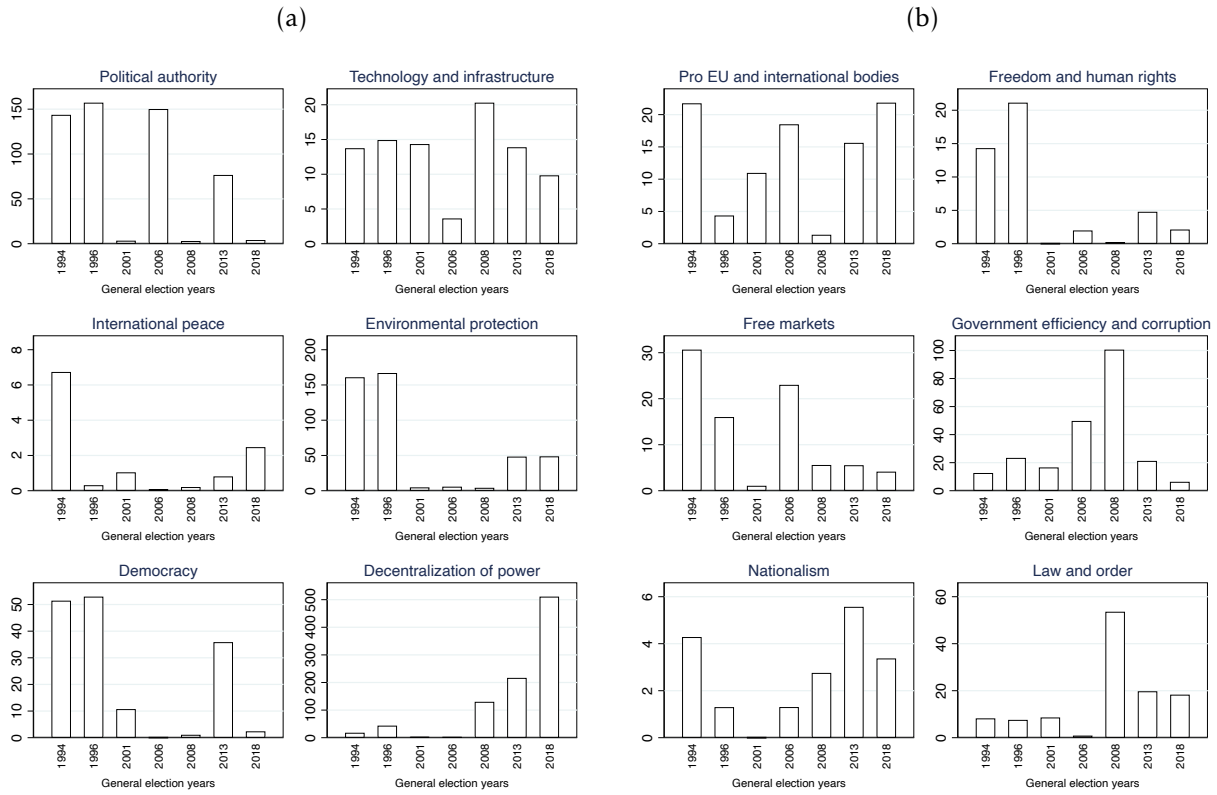
(c)
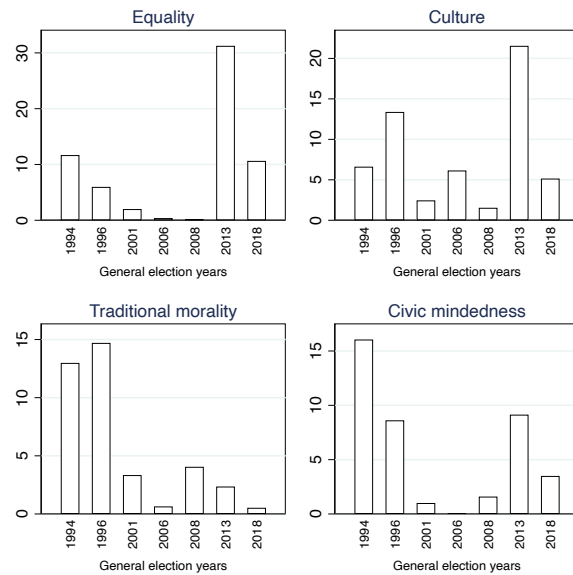
Each bar measures the variance of the Manifesto scores across parties for each election year since 1994. Each score is described in Table B2. See text for details.

Appendix Figure A2. Salience of state intervention across election years (1946-2018)
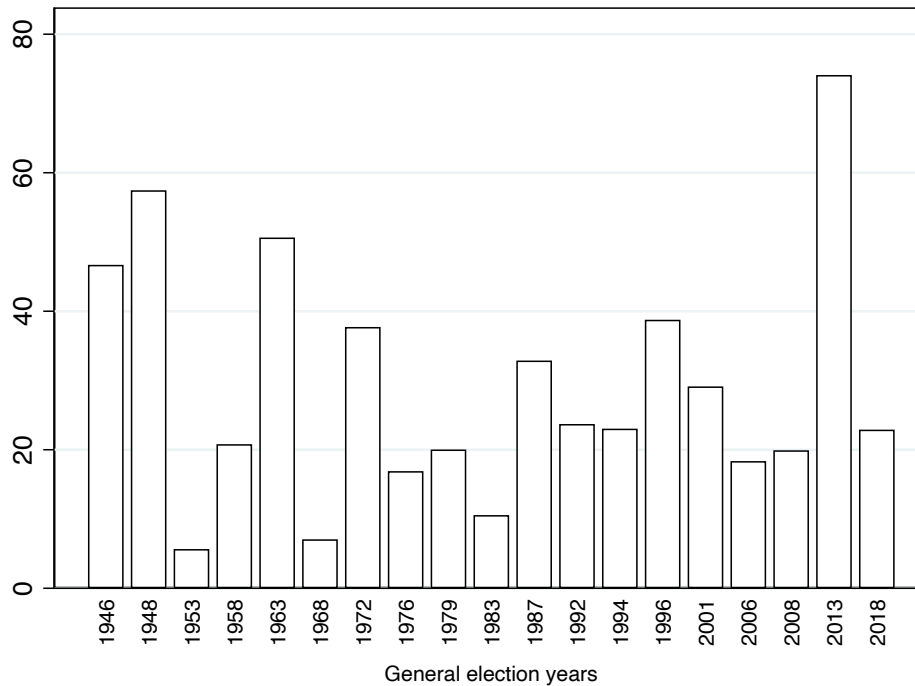
X-axis: General election years

Each bar measures the variance of the composite Manifesto score across parties for each election year. The composite score is the sum of the five Manifesto scores described in Table 1. See text for details.

Appendix Table A1. Party-specific composite Manifesto score (2013 election)

Party	Value of the score
Autonomy Progress Federalism Aosta Valley	9.09
Brothers of Italy	9.55
Civic Choice	11.01
Civil Revolution	8.45
Democratic Centre	10.20
Democratic Party	3.43
Five Star Movement	38.06
Labour and Freedom List	9.47
Left Ecology Freedom	8.04
Northern League	13.33
People of Freedom	13.33
South Tyrolean People's Party	9.22
Union of the Center	3.10

Party-specific composite Manifesto score for the 2013 Italian general election. The score is computed using the incidence of sentences related to the five categories described in Table 1 in the party's publicly available manifesto. See text for details.

Appendix Table A2. Support for state intervention in 2013 - descriptive statistics

	Support for state intervention, 2013
Mean	34.5
Median	34.5
Standard deviation	7.0
Min	10.1
Max	56.4
Number of municipalities	2731

Descriptive statistics in the CasMez area. The index is computed by combining the party-specific composite Manifesto score with party vote shares at the 2013 general election (see Equation 1).

B Appendix B

Appendix Figure B1. CasMez jurisdiction

The darker yellow area shows the CasMez jurisdiction. Brown lines denote regional borders.

Appendix Figure B2. McCrary (2008) test
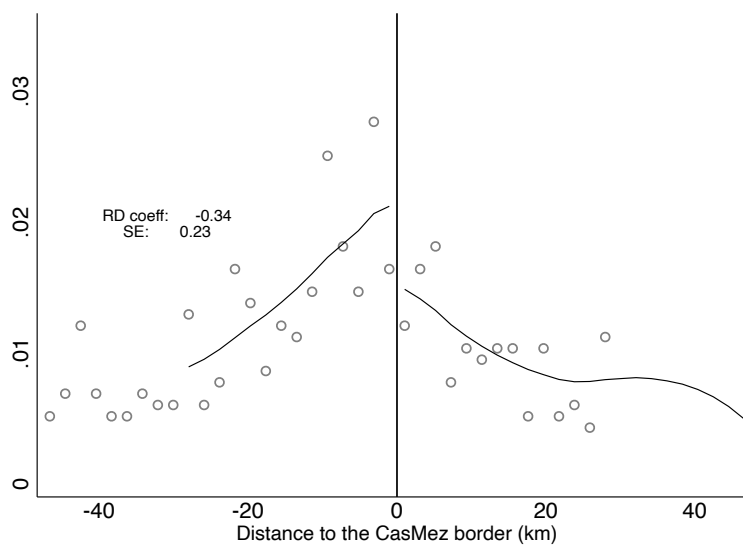
Output of a McCrary (2008) test of continuity in the density of the running variable.

Appendix Figure B3. CasMez border - balancing (continued)

(a) Labor markets and demography, 1951

(b) Electoral outcomes, 1946
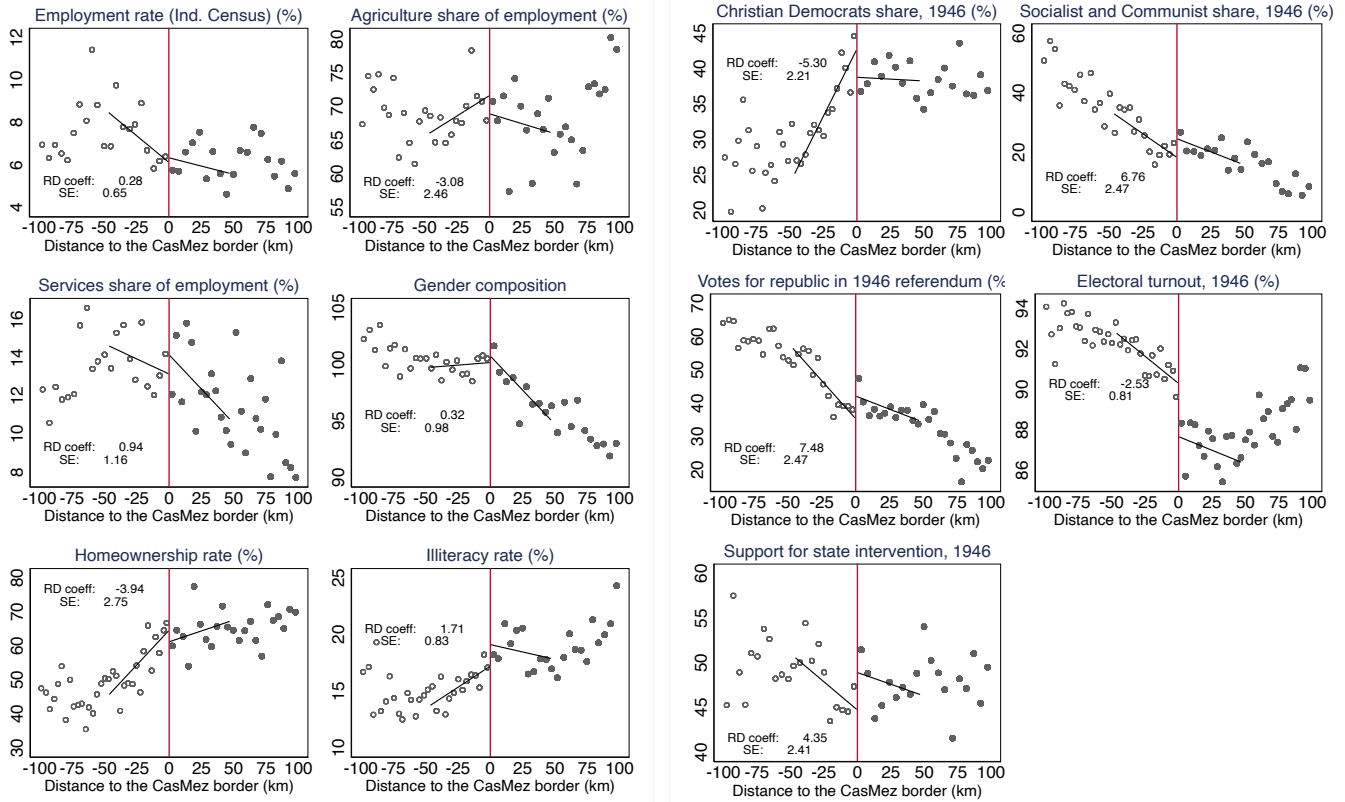
Panel (a): "Employment rate (Ind. Census) shows the total number of employees from Industrial Census as a fraction of the municipality population in 1951. "Gender composition" is the ratio between male and female population (multiplied by 100). "Homeownership rate" is the share of owner-occupied dwellings to total occupied dwellings. "Illiteracy rate" is the share of illiterate residents aged 6 and over to the resident population aged 6 and over. Panel (b): The "Socialist and Communist share" includes cumulated votes for the Communist and Socialist party in 1946 (for comparability with the 1948 election). "Support for state intervention, 1946" is the index of support for state intervention computed using Equation 1 for the 1946 election, accounting for province and border segment effects. See Figure 3 and text for details.

Appendix Figure B4. RD estimate across election years
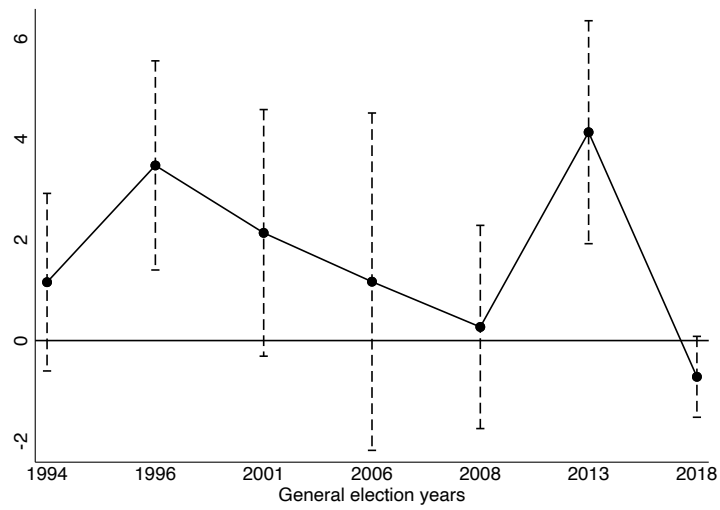
Regression estimates of the β coefficient and 95% robust confidence intervals resulting from the estimation of Equation 2b separately across election years post-CasMez, using the optimal bandwidth. See text for details.

Appendix Figure B5. Other views in the electorate, 2013
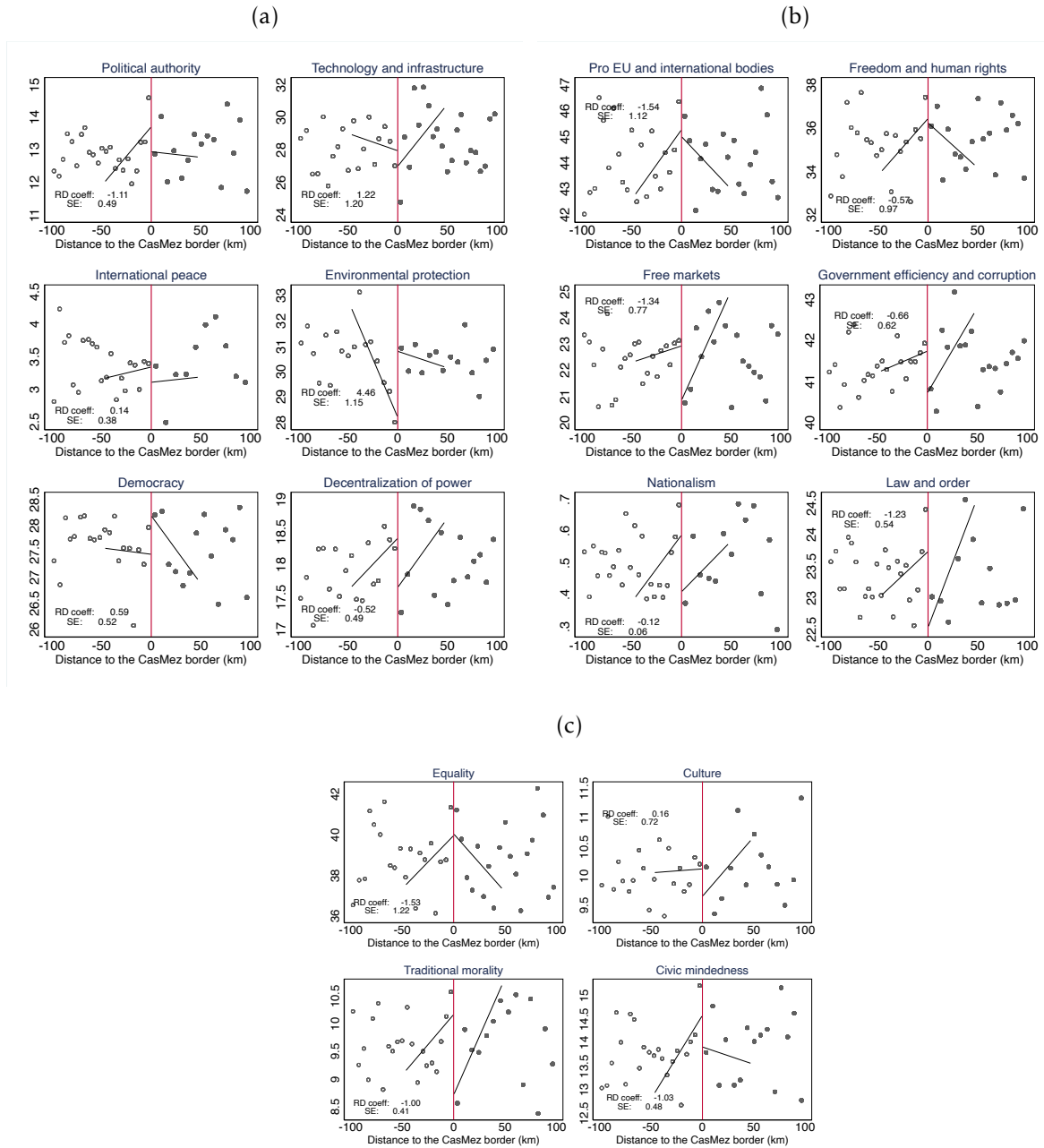
(a)   (b)

(c)

Each index is computed by weighing the Manifesto Project score with party vote shares at the 2013 election using Equation 1. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border and accounting for province and border segment effects. Appendix Table B2 describes each of the above scores. See Figure 3 and text for details.

Appendix Figure B6. RD estimate across election years
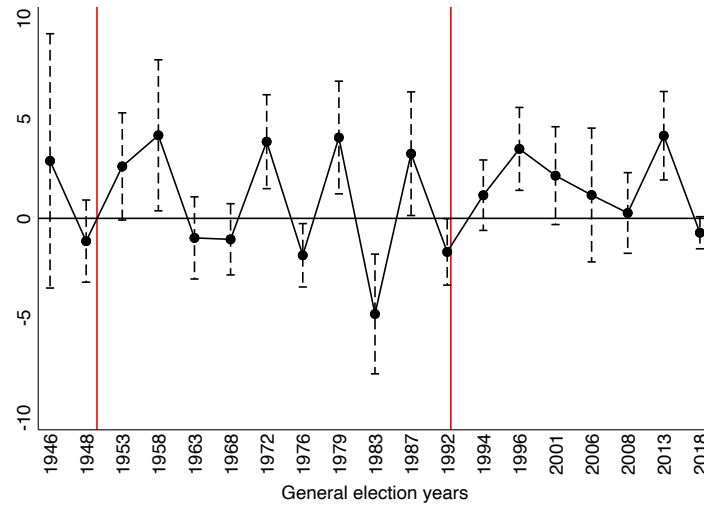
Regression estimates of the β coefficient and 95% robust confidence intervals resulting from the estimation of Equation 2b separately across election years, using the optimal bandwidth. Red lines denote the beginning (1950) and the end (1992) of the *extraordinary intervention*. See text for details.

Appendix Table B1. RD estimates - full sample

Outcome variable:	(1)	(2)
Support for state intervention	All elections 1994-2018	2013 election
Panel (a): Parametric (linear) estimates		
RD estimate	0.97	3.32
	(0.41)	(0.99)
Panel (b): Non-parametric estimates		
RD estimate	1.51	3.80
	(0.45)	(1.13)
Bandwidth (km)	56.15	56.15
Observations	4405	649
Mean	46.54	37.73
Standard deviation	16.61	5.17

Replication of Table 2, including also municipalities close to segments of the CasMez border coinciding with regional borders or with the border of the Kingdom of the Two Sicilies. For the non-parametric estimates we present the bias-corrected point estimate along with the robust standard error (the conventional point estimate and standard error are, respectively, 1.19 and 0.32 for the pooled 1994-2018 sample and 3.99 and 0.74 for the 2013 election). The optimal bandwidth has been re-derived for the entire CasMez border using the same algorithm described in Section 4. See text for details.

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Appendix Table B2. Manifesto scores

Score	Description
Pro EU and international bodies	"Need for international co-operation" + "Favourable mentions of European Community/Union in general"
Freedom and human rights	"Favorable mentions of importance of personal freedom and civil rights"
Free markets	"Favourable mentions of the free market and free market capitalism as an economic model" + "Need for economically healthy government policy making"
Government efficiency and corruption	"Need for efficiency and economy in government and administration" + "Need to eliminate political corruption and associated abuses of political and/or bureaucratic power"
Nationalism	"Favourable mentions of the manifesto country's nation, history, and general appeals"
Law and order	"Favourable mentions of strict law enforcement, and tougher actions against domestic crime"
Political authority	"References to the manifesto party's competence to govern and/or other party's lack of such competence"
Technology and infrastructure	"Importance of modernisation of industry and updated methods of transport and communication"
International peace	"Negative mentions of particular countries with which the manifesto country has a special relationship" + "Negative references to the military or use of military power to solve conflicts" + "Any declaration of belief in peace and peaceful means of solving crises"
Environmental protection	"General policies in favour of protecting the environment, fighting climate change, and other "green" policies"
Democracy	"Favourable mentions of democracy as the "only game in town""
Decentralization of power	"Support for federalism or decentralisation of political and/or economic power"
Equality	"Concept of social justice and the need for fair treatment of all people"
Culture	"Need for state funding of cultural and leisure facilities including arts and sport"
Traditional morality	"Favourable mentions of traditional and/or religious moral values"
Civic mindedness	"Appeals for national solidarity and the need for society to see itself as united"

Description of the Manifesto scores used to compute the indices showed in Figure 6 and Figure B5. More details available in the Manifesto Project Dataset – Codebook (Version 2019b).

Appendix Table B3. Other views in the electorate (2013 election)

	(1)	(2)	(3)	(4)
	Pro EU and international bodies	Free markets	Government efficiency and corruption	Nationalism
RD Estimate	-1.54	-1.34	-0.66	-0.12
	(1.12)	(0.77)	(0.62)	(0.06)
Mean around the border	38.59	25.06	42.59	0.47
Standard deviation	4.66	4.64	2.76	0.42
Observations	360	360	360	360
	Political authority	Technology and infrastructure	International peace	Environmental protection
RD Estimate	-1.11	1.22	0.14	4.46
	(0.49)	(1.20)	(0.38)	(1.15)
Mean around the border	10.79	37.08	4.63	38.11
Standard deviation	2.35	6.09	1.63	4.93
Observations	360	360	360	360
	Democracy	Decentralization of power	Equality	Culture
RD Estimate	0.59	-0.52	-1.53	0.16
	(0.52)	(0.49)	(1.22)	(0.72)
Mean around the border	27.37	18.92	32.40	13.17
Standard deviation	2.49	2.53	5.14	3.03
Observations	360	360	360	360
	Traditional morality	Law and order	Civic mindedness	Freedom and human rights
RD Estimate	-1.00	-1.23	-1.03	-0.57
	(0.41)	(0.54)	(0.48)	(0.97)
Mean around the border	9.73	22.85	11.91	34.04
Standard deviation	2.40	3.27	2.27	4.36
Observations	360	360	360	360

RD estimates of coefficient β in Equation 2b run for the 2013 general election using the optimal bandwidth. Each index is computed by weighing the Manifesto Project scores with party vote shares at the 2013 election. The Manifesto scores are described in Table B2. Robust standard errors in parentheses. See text for details.

Appendix Table B4. Other views in the electorate (all elections 1994-2018)

	(1)	(2)	(3)	(4)
	Pro EU and international bodies	Free markets	Government efficiency and corruption	Nationalism
RD Estimate	-0.24	-0.26	-0.23	-0.87
	(0.44)	(0.41)	(0.43)	(0.47)
Mean around the border	39.93	43.31	59.89	21.65
Standard deviation	14.32	14.62	14.03	18.83
Observations	2470	2470	2470	2470
	Political authority	Technology and infrastructure	International peace	Environmental protection
RD Estimate	-0.50	0.81	1.17	1.81
	(0.45)	(0.45)	(0.39)	(0.39)
Mean around the border	38.20	49.83	19.09	33.76
Standard deviation	23.53	15.49	15.92	23.35
Observations	2470	2470	2470	2470
	Democracy	Decentralization of power	Equality	Culture
RD Estimate	0.41	-0.67	0.89	0.64
	(0.34)	(0.34)	(0.46)	(0.47)
Mean around the border	25.86	22.93	30.43	42.12
Standard deviation	16.01	16.71	13.26	24.10
Observations	2470	2470	2470	2470
	Traditional morality	Law and order	Civic mindedness	Freedom and human rights
RD Estimate	-1.93	-0.75	-0.57	-0.50
	(0.48)	(0.49)	(0.33)	(0.35)
Mean around the border	32.36	41.63	26.16	41.26
Standard deviation	17.27	13.27	18.20	16.88
Observations	2470	2470	2470	2470

RD estimates of coefficient β in Equation 2a estimated on the pooled sample of all general elections in the post-Casmez period using the optimal bandwidth. Each index is computed by weighing the Manifesto Project scores with party vote shares at each election. The Manifesto scores are described in Table B2. Robust standard errors in parentheses. See text for details.

Appendix Table B5. Economic effects - RD estimates

	Employment rate (%)			Industry share (%)		
	1981	1991	2011	1981	1991	2011
RD estimate	2.81	2.29	0.78	16.63	13.06	5.28
	(1.45)	(1.29)	(0.92)	(1.97)	(1.50)	(1.31)
Bandwidth (km)	46.96	46.96	46.96	46.96	46.96	46.96
Observations	357	358	360	357	358	360
Mean	41.19	40.26	43.77	44.25	39.18	31.05
Standard deviation	7.77	7.11	4.88	12.37	12.36	10.58

	Agriculture share (%)			Services share (%)		
	1981	1991	2011	1981	1991	2011
RD estimate	-14.93	-13.90	-5.99	0.13	0.84	0.71
	(2.41)	(1.83)	(1.30)	(2.29)	(1.90)	(1.45)
Bandwidth (km)	46.96	46.96	46.96	46.96	46.96	46.96
Observations	357	358	360	357	358	360
Mean	19.17	12.17	6.15	41.44	48.65	62.79
Standard deviation	11.29	9.21	4.94	13.73	14.31	11.64

	Other outcomes, 2011			
	Income/cap.	Gini	HS educ.	Pub. emp.
RD estimate	0.72	0.00	1.82	-0.21
	(0.40)	(0.01)	(1.26)	(4.49)
Bandwidth (km)	46.96	46.96	46.96	46.96
Observations	360	360	360	360
Mean	19.85	0.38	38.35	29.54
Standard deviation	2.31	0.05	6.43	23.26

Estimation output of Equation 2b using the optimal bandwidth. "Income/cap." is measured as taxable income per taxpayer in 2011 (thousand euros). "Gini" is the Gini coefficient. "HS educ." denotes the share of people aged at least 6 with high school education or more. "Pub. emp." is the number of public workers per 1000 people in 2011. Robust standard errors in parentheses. Descriptive statistics are always computed within the estimation sample. See Table 2 and text for details.

Appendix Table B6. Economic outcomes - Robustness tests

	Bandwidth choice			RD control function		"Donut hole"		Spatial SEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment rate, 1991 (%)							
RD estimate	2.16	2.40	2.24	2.30	2.09	1.53	9.90	2.29
	(1.55)	(1.27)	(1.26)	(1.31)	(1.49)	(1.69)	(3.24)	(1.71)
Observations	237	369	619	358	358	292	188	358
Mean	40.46	40.28	39.42	40.26	40.26	40.31	40.10	40.26
Standard deviation	7.28	7.09	7.29	7.11	7.11	7.04	6.89	7.11
	Employment rate, 2011 (%)							
RD estimate	2.05	0.77	0.38	0.84	1.51	0.23	3.19	0.78
	(1.02)	(0.91)	(0.90)	(0.93)	(1.02)	(1.23)	(2.37)	(1.16)
Observations	238	371	621	360	360	294	190	360
Mean	43.77	43.84	43.08	43.77	43.77	43.84	43.73	43.77
Standard deviation	4.53	4.88	5.73	4.88	4.88	4.99	5.30	4.88
	Industry share, 1991 (%)							
RD estimate	13.83	12.94	10.92	13.09	14.52	13.40	17.11	13.06
	(2.11)	(1.49)	(1.46)	(1.56)	(2.05)	(1.76)	(4.37)	(2.08)
Observations	237	369	619	358	358	292	188	358
Mean	38.80	39.17	38.34	39.18	39.18	39.33	40.25	39.18
Standard deviation	11.63	12.35	11.73	12.36	12.36	12.71	13.28	12.36
	Industry share, 2011 (%)							
RD estimate	5.06	5.37	4.21	5.34	6.62	4.97	2.38	5.28
	(1.75)	(1.29)	(1.24)	(1.34)	(1.67)	(1.35)	(3.35)	(1.79)
Observations	238	371	621	360	360	294	190	360
Mean	30.69	31.07	31.07	31.05	31.05	31.56	31.88	31.05
Standard deviation	10.11	10.64	10.12	10.58	10.58	10.93	11.74	10.58
Bandwidth (km)	25	50	100	46.96	46.96	46.96	46.96	46.96
Polynomial order	1	1	1	2	3	1	1	1
Donut hole (km)	0	0	0	0	0	5	15	0

Continues next page

Appendix Table B7. Economic outcomes - Robustness tests (continued)

| | Bandwidth choice | | | RD control function | | "Donut hole" | | Spatial SEs |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Income per capita, 2011								
RD estimate	1.00	0.73	0.52	0.94	0.61	0.82	1.28	0.72
	(0.52)	(0.39)	(0.38)	(0.41)	(0.47)	(0.52)	(1.73)	(0.45)
Observations	238	371	621	360	360	294	190	360
Mean	19.76	19.84	19.66	19.85	19.85	19.82	19.95	19.85
Standard deviation	2.30	2.32	2.28	2.31	2.31	2.34	2.32	2.31
Gini coefficient, 2011								
RD estimate	0.01	0.00	-0.00	0.01	0.01	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Observations	238	371	621	360	360	294	190	360
Mean	0.37	0.38	0.37	0.38	0.38	0.38	0.38	0.38
Standard deviation	0.03	0.05	0.04	0.05	0.05	0.05	0.06	0.05
High school education, 2011 (%)								
RD estimate	2.63	1.83	1.73	2.11	0.80	2.52	6.36	1.82
	(1.54)	(1.25)	(1.20)	(1.27)	(1.50)	(1.92)	(3.92)	(1.60)
Observations	238	371	621	360	360	294	190	360
Mean	37.85	38.35	38.41	38.35	38.35	38.33	38.64	38.35
Standard deviation	6.05	6.45	6.36	6.43	6.43	6.52	6.92	6.43
Public employees, 2011								
RD estimate	-2.02	-0.38	0.95	-0.44	-3.25	-1.69	15.87	-0.21
	(5.73)	(4.46)	(4.30)	(4.58)	(5.47)	(5.33)	(12.79)	(4.97)
Observations	238	371	621	360	360	294	190	360
Mean	28.80	29.45	29.00	29.54	29.54	29.04	30.27	29.54
Standard deviation	21.71	22.99	21.63	23.26	23.26	22.09	24.27	23.26
Bandwidth (km)	25	50	100	46.96	46.96	46.96	46.96	46.96
Polynomial order	1	1	1	2	3	1	1	1
Donut hole (km)	0	0	0	0	0	5	15	0

Replication of Table B5, robustness tests. Columns (1)-(3) use a 25, 50 and 100 km symmetric bandwidth. Columns (4)-(5) use a more flexible specification for the RD polynomial. Columns (6)-(7) perform donut-hole RD regressions excluding municipalities in a neighborhood of the cutoff. Column (8) allows for spatially clustered standard errors using Conley (1999). Descriptive statistics are always computed within the estimation sample. See Table B5 and text for details.

Appendix Table B8. Population - RD estimates

	Population growth relative to 1951 (%)					
	1961	1971	1981	1991	2001	2011
RD estimate	12.47	31.72	49.33	57.74	60.47	66.25
	(3.77)	(9.01)	(11.86)	(13.95)	(15.75)	(17.95)
Bandwidth (km)	46.96	46.96	46.96	46.96	46.96	46.96
Observations	345	345	345	345	345	345
Mean	-7.54	-18.00	-16.88	-13.80	-11.49	-7.29
Standard deviation	15.20	32.85	43.85	50.45	55.09	61.91

	Fertility rate (%)		Mortality rate (%)		Population density	
	1991	2001	1991	2001	1991	2001
RD estimate	0.01	0.01	-0.04	-0.05	138.34	165.45
	(0.03)	(0.02)	(0.03)	(0.03)	(59.82)	(62.03)
Bandwidth (km)	46.96	46.96	46.96	46.96	46.96	46.96
Observations	358	360	358	360	358	360
Mean	0.16	0.14	0.20	0.20	199.33	213.46
Standard deviation	0.12	0.09	0.13	0.13	290.36	309.75

Estimation output of Equation 2b using the optimal bandwidth. Robust standard errors in parentheses. "Fertility rate" and "Mortality rate" computed as percentages of total population. "Population density" computed as the number of inhabitants per km^2. Descriptive statistics are always computed within the estimation sample. See Table 2 and text for details.

C   Appendix C

In this Appendix we conduct a robustness exercise to ensure that our results are not driven by pre-existing political differences between municipalities north and south of the CasMez border. While we have documented that the choice of the border was likely inspired by technical (exogenous) considerations related to the execution of infrastructure projects, complete information on the decision making process is unfortunately not available. As documented for example in Colussi et al. (2020), the allocation of funds within the CasMez jurisdiction was often higher in places where opposition parties were stronger. What if, at least in a few instances, the choice of the additional municipalities in central Italy to be added to the CasMez jurisdiction was also informed by political convenience? In fact, we show in Panel (b) of Figure 3 that support for the main opposition parties (Communists and Socialists) and the incumbent Christian Democratic Party was overall quite similar north and south of the cutoff in 1948, if not for a small jump driven by municipalities just south of the cutoff. We now consider the possibility that, when the border was set in 1950, the government included certain municipalities only for (endogenous) reasons related to their political orientation. We focus on municipalities just south (10 km) of the border and identify those more likely to have been included because of their strong support for opposition parties relative to the incumbent. Specifically, for each municipality within 10 km south of the border, we compute the difference between the 1948 votes share of the Christian Democrats and that of the Socialist and Communist parties (which run together in 1948). We then flag places where this difference was particularly small - below the 25th percentile.[40] Figure C1 replicates the RD plots of Figure 3 when the flagged municipalities with weakest support for the Christian Democrats are excluded, and shows that vote shares in 1948 (and also in 1946) are almost perfectly balanced at the CasMez border. Table C2 shows that our results are virtually unchanged when excluding these potentially problematic municipalities, as we still estimate a positive effect on voters' support for state intervention long after the end of the policy (the point estimate is almost identical to that for the baseline sample).

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[40]These are municipalities where the lead of Christian Democrats in 1948 was very small, or negative. Table C1 details the distribution of this variable in municipalities 10 km south of the CasMez border.

Appendix Table C1. Christian Democrats' Lead in 1948 south of CasMez border

Mean	24.98
Standard deviation	27.78
Median	30.02
P25	2.88
P75	44.05
Min	-41.73
Max	83.97
Number of municipalities	69

The Table shows descriptive statistics for the difference between the votes share of the Christian Democratic party and the votes share of the Communist and Socialist parties in 1948. The sample includes municipalities up to 10 km south of the CasMez border. See text for details.

Appendix Figure C1. Balancing, pre-CasMez elections
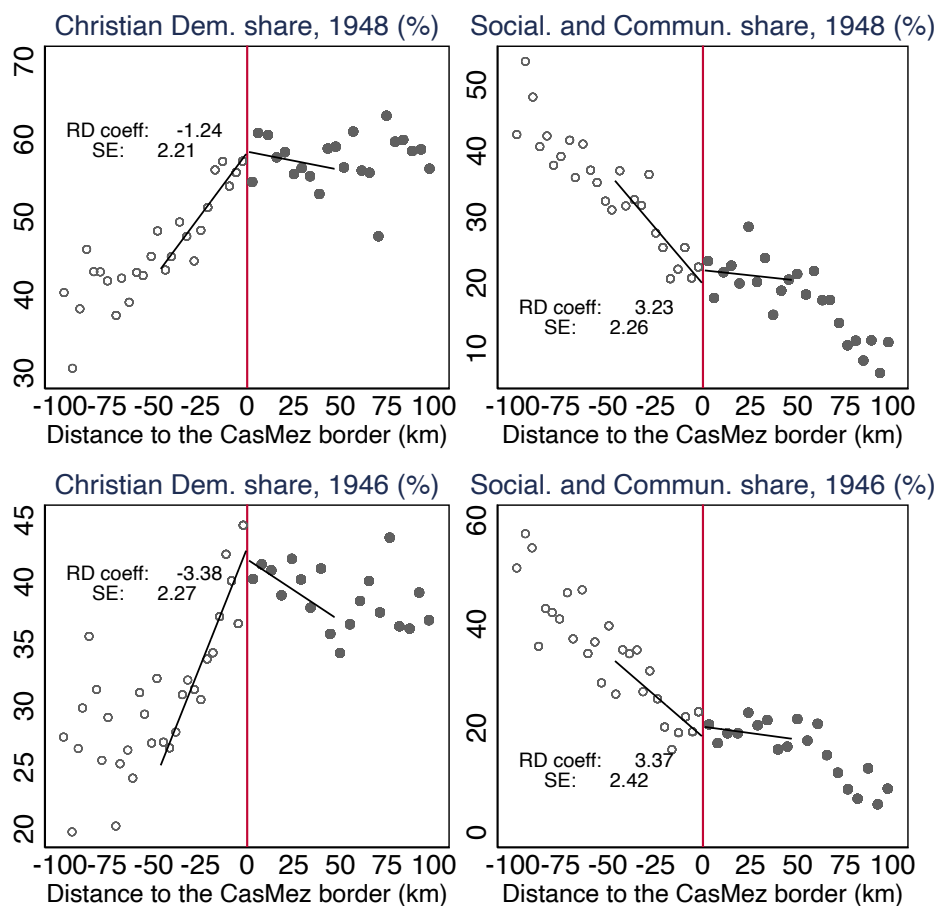
Replication of Figure 3 on sample excluding municipalities south of the border with strong support for opposition parties. See text for details.

Appendix Table C2. RD estimates - Low lead of Christian Democrats in 1948

Outcome variable:	(1)	(2)
Support for state intervention, 2013	Baseline estimates	Excl. low CD-lead municipalities
RD estimate	4.14	4.16
	(1.13)	(1.30)
Bandwidth (km)	46.96	46.96
Observations	360	345
Mean	38.31	38.39
Standard deviation	4.89	4.81

RD estimates of coefficient β in Equation 2b using the optimal bandwidth. Column (1) reports baseline estimates. Column (2) excludes municipalities where the lead of the Christian Democrats relative to the Socialist and Communist parties in 1948 was below the 25th percentile of the distribution up to 10 km south of the border. Robust standard errors in parentheses. See text for details.

D Appendix D

In this Appendix we isolate variation in transfers within the CasMez jurisdiction and relate it to voters' support for state intervention. There is indeed large cross-sectional variation in transfers as is clear from Panel (a) in Figure D1, which shows the cumulative amount of CasMez transfers received by each municipality between 1950 and 1992. To address the endogeneity concerns raised in Section 3 and provide more reliable estimates, we exploit here a source of institutional variation in transfers. As described in Section 1, the main purpose of the *extraordinary intervention* was reoriented from infrastructure investment towards industrial policy with Law n. 634 in 1957, which introduced the Industrial Zones. A Zone was created upon the initiative of a group of municipalities to form a *consortium* and submit a development plan for the area to the CasMez. Importantly, the policymaker disposed that firms located in a Zone could benefit of more generous transfers than other firms in the CasMez region.[41] The ASET historical archives provide a list of the Industrial Zones, together with the 400 included municipalities, which we digitize and plot in Figure D1, Panel (b). A quick glance back at the left panel suggests that transfers were largely concentrated in these areas.

The primary goal of this policy was to encourage industrial concentration in specific areas of the South deemed particularly suitable for industrialization. Legitimate concerns would arise about the validity of an estimation strategy that simply compares municipalities belonging to Industrial Zones to all other municipalities in the sample. Important differences indeed exist between the former and the latter. We inspect them in Table D1, which compares the average CasMez transfer, along with a range of other observable characteristics, between municipalities within and outside of Industrial Zones. On average, cumulative transfers stand at around 8,120 real euros per capita in municipalities belonging to Industrial Zones versus 1,630 in other municipalities in the CasMez jurisdiction. Municipalities belonging to a Zone were also more likely to be a provincial capital and their geographic traits were more prone to industrialization. They were more densely populated and featured a more educated population and a larger industry share of the workforce relative to other municipalities.

We exploit the fact that the inclusion of a municipality in a Zone was subject to the government's examination of a well-defined set of parameters, listed in the 1951 census. An excerpt of the form that a consortium had to fill, for each candidate municipality, when submitting its application to the government is pasted in Figure D2. The form listed a range of demographic, geographic and economic characteristics aimed at assess-

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[41]See the 1965-1970 government coordination plan for public intervention in the South of Italy.

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ing the suitability of the area to future industrial concentration, such as the availability of a large and educated workforce, pre-existing industrial settlements and infrastructure endowment. Conveniently, we observe many of these (and other, likely correlated) characteristics in the 1951 census data, which we use to compute the predicted probability of belonging to a Zone for each municipality in the CasMez area. Specifically, we estimate the following logit regression:

$$e_{m,p} \equiv Pr(IZ_{m,p} = 1 | W_{m,p}, \gamma_p, \epsilon_{m,p}) = \Phi(\alpha + \gamma_p + W'_{m,p} \cdot \beta + \epsilon_{m,p}) \tag{3}$$

Where $IZ_{m,p}$ is a dummy variable taking value of one if municipality m in province p belongs to an Industrial Zone and zero otherwise. The estimation controls for municipality-level geographic characteristics and the following covariates in 1951: population density, number of establishments per person, population age and gender composition, share of people with high school education, labor market participation rate and workforce sectoral composition. Provincial capitals have been dropped from the sample. We also include CasMez infrastructure spending before the establishment of the Industrial Zones to account for pre-existing differences in infrastructure endowments. Last, we control for the municipality's political orientation during the 1960s (when Industrial Zones were being created), proxied by the average votes share for the Christian Democratic party at the 1963 and 1968 election. While obviously not listed among the relevant characteristics for Zone inclusion in the official form, the position of a given municipality across the political spectrum might have influenced such decision. For instance, the incumbent government may have used Zone inclusion to reward local voters in a politically affine municipality, or to erode support for opposition parties in places where these were stronger.

We then match each municipality belonging to a Zone with another municipality lying outside of a Zone but sharing similar values of the estimated propensity score $\hat{e_{m,p}}$.[42] In other words, we construct a matched sample composed of pairs of municipalities that do not differ in terms of relevant characteristics but are subject to differential exposure to the treatment (CasMez transfers) based on whether they belong to a Zone (Abadie and Imbens, 2016). Our matched sample consists of 364 municipalities, half of which belong to a Zone, and is showed in Figure D3. Descriptive statistics are reported in Table D2 and confirm the overall balancing of the sample. A stark gap in CasMez transfers

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[42]We adopt a nearest-neighbor matching without replacement and within a 0.05 caliper, corresponding to roughly one quarter of the standard deviation of the estimated propensity score. The matching procedure excludes municipalities whose propensity score lies outside of the common support (Leuven and Sianesi, 2018).

between municipalities remains, with those included in Industrial Zones receiving on average funds for 7,840 euros per capita versus only 2,290 in control municipalities.

Intuitively, this estimation procedure corresponds to using $IZ_{m,p}$ as an instrument for CasMez transfers. Correct identification thus relies on the conditional independence of potential outcomes and treatment of the Zone status. More precisely, one first requirement is that, conditional on the observed covariates, Zone status is as good as randomly assigned across municipalities.[43] Another requirement is that Zone status affects voters' support for state intervention in 2013 only through the variation it induces to CasMez transfers (exclusion restriction). The existence of well-defined observable criteria for the establishment of a Zone is crucial for the validity of this strategy, which however comes with the big caveat that only selection on observables can be checked and that there might be relevant unobservable differences between the treated and the control group. In this regard, we address the valid concern that the inclusion in a Zone might have been influenced by political incentives by also matching on municipalities' political orientation.

We employ this matched sample to estimate a 2-Stage Least Squares regression specification relating support for state intervention in municipality m in province p in 2013 to the total amount of transfers received from 1950 to 1992 (scaled by population size in 1951), instrumented using Zone status and controlling for province-level fixed effects. The estimation output is showed in Table D3. We estimate that an increase of 1,000 real euros in transfers per capita (one fifth of the mean transfer in the estimation sample) corresponds to a 0.33 points rise in the outcome – about 5 percent of a standard deviation. As said, we have less confidence in these estimates relative to those produced by the RD design, which also accounts for selection on unobservables provided the main identifying assumptions hold. However, the drawbacks of this approach are in part compensated by its greater external validity relative to the RD estimates, which are local to the CasMez border. It should also be noticed that the parameters identified by the two strategies are not directly comparable: in the latter approach, we placed ourselves within the CasMez territory and exploited variation in the intensity of transfers across municipalities. The RD strategy compares instead municipalities within the CasMez area with other municipalities outside of it.

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[43]In other words, two municipalities sharing similar characteristics but with different Zone status can be safely compared as the missed inclusion in a Zone is driven by factors exogenous to electoral support for state intervention in 2013. This ensures that the reduced form effect of $IZ_{m,p}$ on the outcome of interest has a causal interpretation.

Appendix Figure D1. The Industrial Zones

(a) CasMez transfers (1950-1992) (b) Industrial Zones

0-27
27-334
334-1680
1680+
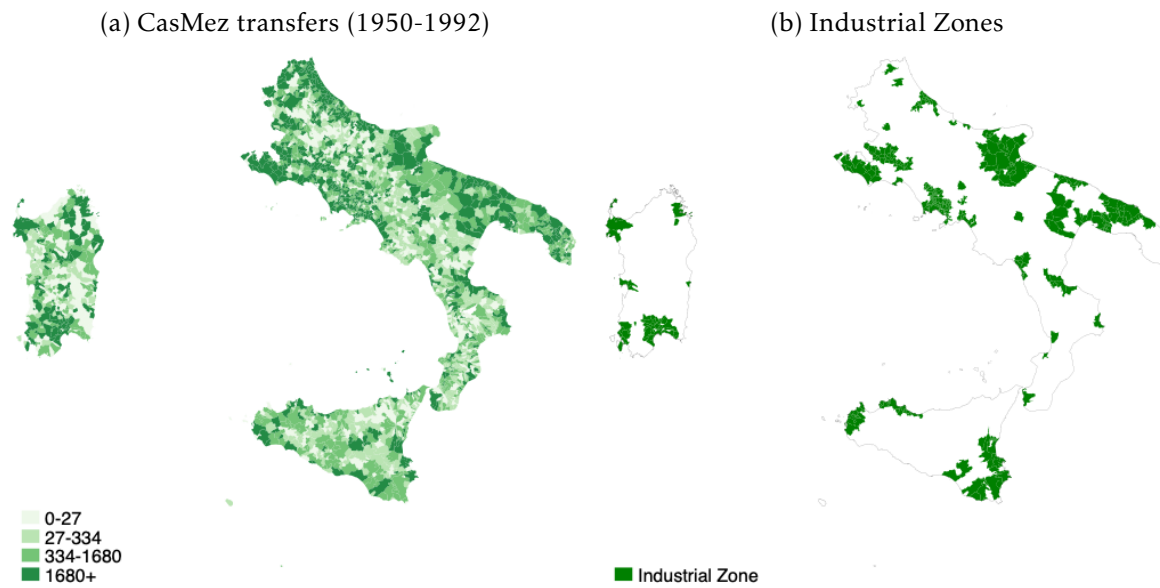
■ Industrial Zone

Panel (a) shows the total amount of CasMez transfers to each municipality between 1950 and 1992 in euros (2011 prices), as a fraction of the population in 1951. Panel (b) shows municipalities belonging to Industrial Zones.

Appendix Figure D2. Industrial Zones - Form

Scheda del Comune di _____

VOCI	Data	Unità di misura	Cifre (*)
I. INDICATORI DEMOGRAFICO-ECONOMICO-SOCIALI			
Popolazione	31-12-1958	N. abitanti	
2. Popolazione attiva (censimento demografico):			
2.1. In complesso	4-11-1951	unità	
2.2. Attivi in agricoltura in % sul complesso	4-11-1951	%	
3. Situazione Industriale (censimento industriale e commerciale):			
3.1. Industrie in totale (estrattive, manifatturiere, delle costruzioni, ecc.):			
3.1.1. Addetti in complesso . .	5-11-1951	numero	
3.1.2. Addetti in esercizi con oltre 50 addetti	5-11-1951	»	
3.1.3. % addetti ad esercizi maggiori (3.1.2.) su addetti in complesso (3.1.1.)	5-11-1951	%	
3.2. Industrie in totale escluso artigianato (1) addetti	5-11-1951	numero	
3.3. Industrie manifatturiere:			
3.3.1. Addetti in complesso . .	5-11-1951	»	
3.3.2. Addetti in esercizi con oltre 50 addetti	5-11-1951	»	
3.3.3. % addetti ad esercizi maggiori (3.3.2.) su addetti in complesso (3.3.1.)	5-11-1951	%	
3.4. Proporzione degli addetti industriali su popolazione attiva:			
3.4.1. Addetti industriali in totale (3.1.1.) per 1.000 attivi (2.1.)	5-11-1951	%	
3.4.2. Addetti industrie escluso artigianato (3.2.) per 1.000 attivi	5-11-1951	%	
3.4.3. Addetti industrie manifatturiere (3.3.1.) per 1.000 attivi	5-11-1951	%	
3.5. Forza motrice utilizzabile:			
3.5.1. In tutte le industrie (3.1.)	5-11-1951	HP	
3.5.2. Media HP per addetto (3.5.1. diviso 3.1.1.)	5-11-1951	»	
3.6. Industrie prevalenti (2):			
3.6.1. . . . . . . addetti	5-11-1951	numero	
3.6.2. . . . . . . addetti	5-11-1951	»	
3.6.3. . . . . . . addetti	5-11-1951	»	
3.6.4. . . . . . . addetti	5-11-1951	»	

(1) Addetti alle industrie in totale (3.1.1.) meno addetti all'artigianato secondo le indicazioni del Censimento Industriale del 1951.

(2) Classi di industrie (secondo classificazione del Censimento) con non meno del 20% sugli addetti alle industrie in totale (3.1.1.).

(*) Da riportare se possibile in quattro colonne distinte per le quattro categorie indicate nella Avvertenza.

Excerpt of the form to be filled by a *consortium* to include a municipality in an Industrial Zone. See the government 1965-1970 government coordination plan for public intervention in the South of Italy.

Appendix Figure D3. Industrial Zones - matched sample

■ Industrial Zone
■ Matched control municipality
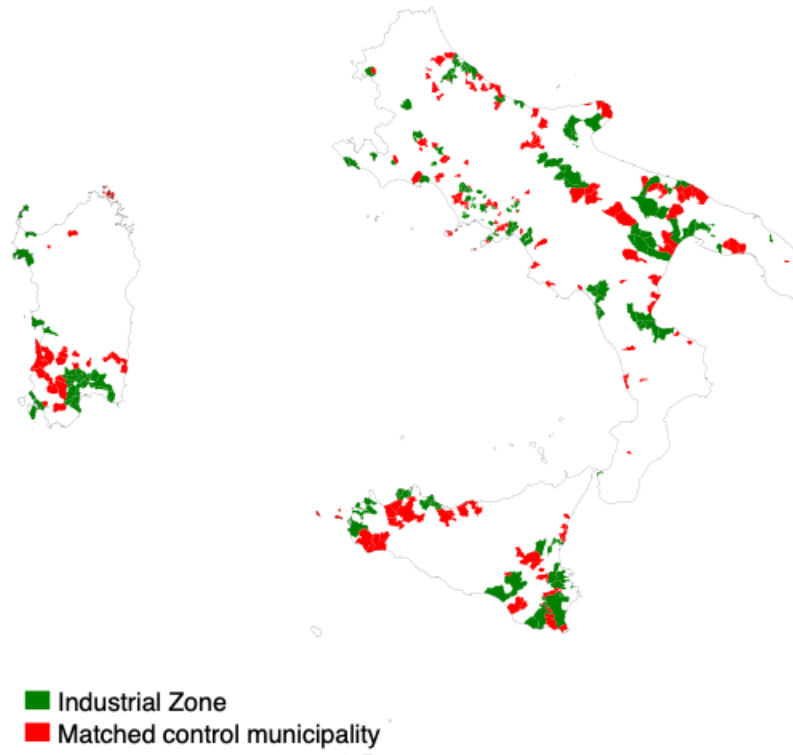
Matched sample resulting from a propensity score matching that relates Zone status to municipality characteristics. See text for details.

Appendix Table D1. Industrial Zones and other CasMez municipalities – descriptive statistics

	Industrial Zone	Other municipalities
CasMez transfers	8.12	1.63
	(12.58)	(4.67)
CasMez infrastructure spending	4.11	3.74
	(7.01)	(5.35)
Provincial capital	0.09	0.01
	(0.28)	(0.09)
Coastal location	0.29	0.16
	(0.46)	(0.36)
Elevation	175.17	468.18
	(163.75)	(318.83)
Population density, 1951	558.55	163.11
	(940.19)	(325.93)
High school education (%), 1951	2.53	1.76
	(1.88)	(0.94)
Agriculture share of employment (%), 1951	53.97	71.39
	(21.50)	(15.25)
Industry share of employment (%), 1951	25.49	16.82
	(12.90)	(11.19)
Number of municipalities	400	2325

Descriptive statistics for the CasMez area (mean and standard deviation in parentheses). "CasMez transfers" and "CasMez infrastructure spending" are in thousand euros (2011 prices), cumulated between 1950 and 1992, scaled by population in 1951 and winsorized at the 1st and 99th percentile. "Provincial capital" and "Coastal location" are dummies equal to one for municipalities that are a provincial capital or are located by the sea, respectively. "Elevation" is measured in meters. "Population density" is measured as number of inhabitants per km^2. "High school education" denotes the share of people aged at least 6 with high school education or more. See text for details.

Appendix Table D2. Matched sample – descriptive statistics

	Industrial Zone	Other municipalities
CasMez transfers	7.84	2.29
	(12.82)	(5.19)
CasMez infrastructure spending	4.11	3.28
	(6.72)	(4.84)
Provincial capital	0.00	0.00
	(0.00)	(0.00)
Coastal location	0.32	0.30
	(0.47)	(0.46)
Elevation	220.48	227.62
	(182.83)	(161.90)
Population density, 1951	299.02	381.12
	(383.95)	(998.35)
High school education (%), 1951	2.00	2.19
	(1.15)	(1.28)
Agriculture share of employment (%), 1951	62.47	60.62
	(15.90)	(18.11)
Industry share of employment (%), 1951	21.87	22.42
	(10.48)	(12.99)
Number of municipalities	182	182

Descriptive statistics for the matched sample based on the predicted probability of belonging to an Industrial Zone (mean and standard deviation in parentheses). "CasMez transfers" and "CasMez infrastructure spending" are in thousand euros (2011 prices), cumulated between 1950 and 1992, scaled by population in 1951 and winsorized at the 1st and 99th percentile. "Provincial capital" and "Coastal location" are dummies equal to one for municipalities that are a provincial capital or are located by the sea, respectively. "Elevation" is measured in meters. "Population density" measured as number of inhabitants per km^2. "High school education" denotes the share of people aged at least 6 with high school education or more. See text for details.

Appendix Table D3. Baseline 2-SLS estimates

	(1)	(2)
	First stage	Support for state intervention, 2013
CasMez transfers	5.39	0.33
(instrumented with Industrial Zone dummy)	(0.96)	(0.09)
Kleibergen-Paap F-Stat		31.60
Observations	364	364
Mean	5.07	38.27
Standard deviation	10.15	6.19

Estimation on a matched sample based on the predicted probability of belonging to an Industrial Zone. CasMez transfers are in thousand euros (2011 prices), cumulated between 1950 and 1992, scaled by population in 1951 and winsorized at the 1st and 99th percentile. The coefficient in Column (2) is estimated using a 2-SLS procedure that instruments CasMez transfers with a dummy equal to one for municipalities belonging to Industrial Zones. Column (1) shows the first stage estimate. The outcome is the index of support for state intervention measured for the 2013 general election using the formula in Equation 1. The regression includes province-level fixed effects. Robust standard errors in parentheses. See text for details.

E   Appendix E

In this Appendix we investigate whether individual preferences for state intervention sourced from survey data show patterns that are in line with our main evidence on voting outcomes at the CasMez border. In particular, theoretical models have posited that past exposure to state presence might decrease the extent to which a society believes that individual effort determines income (Alesina and Giuliano, 2011; Corneo and Gruner, 2002) which might, in turn, reinforce preferences for state intervention (Alesina and Angeletos, 2005; Benabou, 2008). We exploit the 5th Wave (2005-2009) of the World Values Survey (WVS), which contains a small set of questions on individual preferences for state intervention for Italy.[44] We use the following questions, all posed as a self-placement scale from 1 to 10: *i*) 1 (*"Incomes should be made more equal"*) – 10 (*"We need larger income differences as incentives for individual effort"*); *ii*) 1 (*"Private ownership of business and industry should be increased"*) – 10 (*"Government ownership of business and industry should be increased"*); *iii*) 1 (*"The government should take more responsibility to ensure that everyone is provided for"*) – 10 (*"People should take more responsibility to provide for themselves"*); *iv*) 1 (*"Competition is good. It stimulates people to work hard and develop new ideas"*) – 10 (*"Competition is harmful. It brings out the worst in people"*); *v*) 1 (*"In the long run, hard work usually brings a better life"*) and 10 (*"Hard work doesn't generally bring success – it's more a matter of luck and connections"*).[45] In particular, question *v*) pins down the precise mechanism set forth in theoretical models, which is whether individual beliefs on the role of effort versus luck are related to past experience of state intervention. We also aggregate measures *i*) - *iv*) into a composite index computed as the simple mean of the four scores, then standardized between zero and one.

The 5th wave of the WVS features responses from 1,012 individuals scattered around the country. Of these, only 17 live 47 km (the baseline RD bandwidth) south of the Cas-Mez border and these are concentrated in only two municipalities. Even within a larger 100-km bandwidth south of the border, only 48 respondents are available in just four municipalities. To increase sample size and obtain more reliable estimates, we therefore have to widen the estimation bandwidth further and choose a 150-km baseline bandwidth. 248 respondents live within this radius, 138 of which in nine treated municipalities south of the border and the remainder in eight municipalities north of it.

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[44]At the time of writing, Italy features only in the 5th wave of the WVS (2005-2009).

[45]These questions are identified as V116 - V120 in the WVS wave 5 questionnaire (see Inglehart et al., 2018). To ease interpretation, we recode questions *i*) and *iii*) as follows: *i*) 1 (*"We need larger income differences as incentives for individual effort"*) - 10 (*"Incomes should be made more equal"*); *iii*) 1 (*"People should take more responsibility to provide for themselves"*) - 10 (*"The government should take more responsibility to ensure that everyone is provided for"*).

Figure E1 plots the empirical distribution of responses to the five above questions separately for respondents living south (green bars) and north (white bars) of the border. A glance at the histograms suggests that the distribution of responses in treated municipalities is more skewed towards agreement to each statement. In particular, respondents south of the border tend to support more income equality (*i*) and government ownership (*ii*), are relatively more in line with the idea that the state should provide for people (*iii*) and believe that competition is harmful (*iv*). In addition, they also agree more with the statement that luck and connections bring success relative to people in control areas (*v*). Figure E2 reproduces the RD plots showed in the main body of the paper for the (standardized) composite index (top panel) and question *v*) on the role of luck versus effort (bottom panel). The plots confirm the suggestive evidence of Figure E1 of a positive discontinuity at the CasMez border, although the very small sample size and limited variation make these results quite uncertain.

Table E1 shows the estimation output of a simple RD design relating each preference measure for each individual in the WVS data to CasMez status based on the municipality of residence, again focusing on respondents living in a 150-km symmetric bandwidth around the border and controlling for distance to the border and border segment fixed effects. For the categorical outcomes (the five individual indices in Columns (1)-(4) and Column (6)) we specify the model as an ordered logit. We estimate a positive discontinuity at the CasMez border for each of the outcomes. For the composite index (Column (5)), the jump is rather sizable at one standard deviation. The bottom panel shows how coefficient estimates vary when controlling for a set of individual-level covariates available in the WVS database (age, gender, employment status and education). Our evidence points again to a positive jump in each preference index, albeit some estimates lose statistical significance.[46]

This analysis comes with many caveats as the sample size and variation exploited to estimate these coefficients is admittedly small. It might nonetheless offer suggestive evidence that a shift in individual attitudes towards the role of the state in the economy might be among the channels through which past state intervention has affected voting outcomes. Further investigation and more granular data might allow researchers to shed more light on these findings.

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[46]The coefficients in Columns (1)-(4) and Column (6) are expressed in log-odds units and do not have a meaningful interpretation. Table E2 shows the implied predicted probabilities (only for the estimates in Panel a)).

Appendix Figure E1. WVS responses, distribution south and north of CasMez border
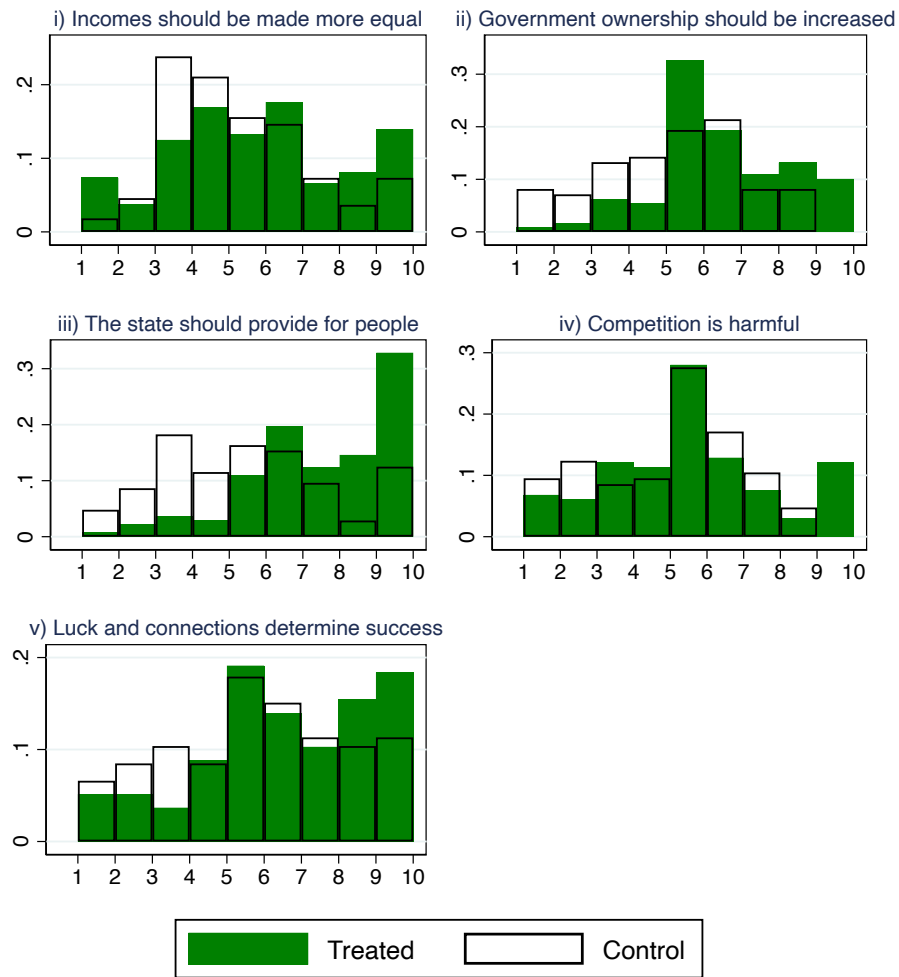
Empirical distribution of responses to five questions in the World Values Survey (2005-2009 wave). Respondents are concentrated in a 150-km symmetric bandwidth around the CasMez border. Green bars denote respondents south of the border. Survey responses are collected on a 1-10 scale based on the degree of agreement with the specific question. See text for details.

Appendix Figure E2. WVS responses, RD plots

World Values Survey 2005-2008
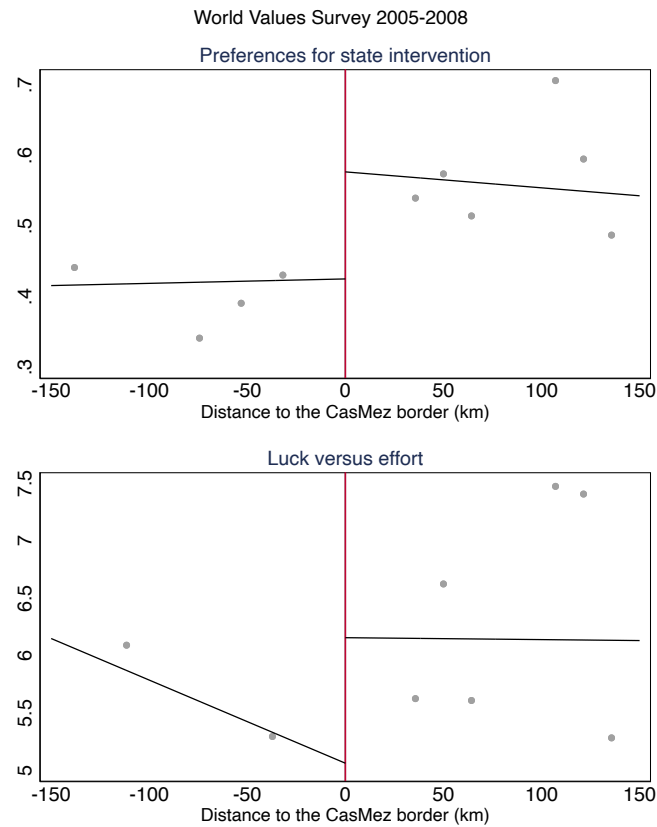
"Preferences for state intervention" is the composite index built as the mean of questions V116 to V119 in the 5th wave of the WVS, standardized between zero and one. Questions V116 and V118 are recoded as described in the text. "Luck versus effort" shows responses to question V120 on a categorical scale from 1 to 10 based on agreement with the statement. The RD estimates and standard errors are equivalent to 0.17 and 0.06 in the top panel, and 1.38 and 0.66 in the bottom panel. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border and estimated on a 150-km bandwidth. See Figure 3 and text for details.

Appendix Table E1. Individual preferences – World Values Survey

WVS question	(1) Incomes should be made more equal	(2) Government ownership should be increased	(3) The state should provide for people	(4) Competition is harmful	(5) Composite index (1)-(4)	(6) Luck rather than effort brings success
Panel (a): No controls						
RD estimate	1.47	1.42	0.89	1.44	0.17	0.98
	(0.51)	(0.60)	(0.59)	(0.61)	(0.06)	(0.50)
Bandwidth (km)	150	150	150	150	150	150
Observations	245	227	241	237	246	242
Mean	5.15	5.46	6.33	4.87	0.50	5.81
Standard deviation	2.35	2.06	2.57	2.26	0.18	2.50
Panel (b): Individual-level controls						
RD estimate	1.53	1.16	0.75	1.33	0.16	0.71
	(0.42)	(0.56)	(0.64)	(0.63)	(0.06)	(0.52)
Bandwidth (km)	150	150	150	150	150	150
Observations	235	218	231	228	236	233
Mean	5.22	5.49	6.35	4.92	0.50	5.81
Standard deviation	2.33	2.02	2.54	2.25	0.17	2.49

Estimation output of a RD design relating individual preferences to CasMez status in a 150-km neighborhood of the CasMez border. Outcomes in Columns (1) - (4) and Column (6) are sourced from the 2005-2009 wave of the World Values Survey and are on a placement scale from 1 (minimum agreement with the statement) to 10 (maximum agreement with the statement). Questions in Columns (1) and (3) have been recoded relative to the original WVS question to ease interpretation. The outcome in Column (5) is a composite index computed as the mean response to questions i) to iv), standardized between 0 and 1. All regressions control linearly for the distance to the CasMez border and for border segment fixed effects. The bottom panel also controls for individual-level covariates (age, gender, employment status and education level). The estimates in Columns (1)-(4) and (6) are obtained through an ordered-logit model. Table E2 shows the implied predicted probabilities. Standard errors clustered by municipality are in parentheses. Descriptive statistics are always computed within the estimation sample. See text for details.

Appendix Table E2. Individual preferences – World Values Survey

WVS question	Incomes should be made more equal		Government ownership should be increased		The state should provide for people		Competition is harmful		Luck rather than effort brings success	
Answer	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated
1	6.0%	3.9%	6.6%	2.2%	4.5%	0.8%	9.7%	7.0%	7.5%	4.5%
2	4.9%	3.3%	6.6%	2.4%	8.7%	1.8%	10.6%	8.4%	8.3%	5.3%
3	20.2%	15.3%	14.3%	6.2%	16.7%	4.6%	11.9%	10.3%	8.1%	5.6%
4	20.2%	18.1%	12.9%	7.1%	10.5%	4.0%	11.2%	10.4%	10.1%	7.7%
5	14.1%	14.5%	28.9%	24.6%	18.3%	10.1%	26.8%	27.7%	19.9%	17.5%
6	14.9%	17.4%	15.5%	22.3%	18.0%	17.6%	13.1%	15.1%	14.0%	14.7%
7	6.0%	7.7%	6.2%	12.2%	8.3%	13.4%	7.6%	9.4%	9.6%	11.5%
8	5.0%	6.9%	5.2%	12.4%	5.5%	12.5%	2.5%	3.2%	10.9%	14.9%
9	2.3%	3.2%	1.8%	4.9%	2.4%	6.8%	1.8%	2.3%	5.1%	7.8%
10	6.3%	9.5%	1.9%	5.6%	7.2%	28.3%	4.7%	6.2%	6.4%	10.5%

Implied predicted probabilities from an ordered logit model relating individual preferences to CasMez status in a 150-km neighborhood of the CasMez border. The corresponding model estimates are showed in Columns (1)-(4) and (6) in Table E1, Panel (a). Outcomes are sourced from the 2005-2009 wave of the World Values Survey and are on a placement scale from 1 (minimum agreement with the statement) to 10 (maximum agreement with the statement). "Control" and "Treated" denote municipalities north and south of the border, respectively. See Table E1 and text for details.