UNIVERSITY COLLEGE LONDON

FACULTY OF SOCIAL AND HISTORICAL SCIENCES

DEPARTMENT OF ECONOMICS

Essays in Labour Economics

Lorenzo Incoronato

Submitted to University College London (UCL)
in fulfillment of the requirements for the degree
of Doctor of Philosophy in Economics

May 2024

Declaration

I, Lorenzo Incoronato, confirm that the work presented in my thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Lorenzo Incoronato
May 22, 2024

Abstract

This thesis consists of three chapters on labour market policies and institutions.

Chapter 1 studies a place-based industrial policy seeking to establish industrial clusters in Italy during the 1960s and 1970s. Leveraging historical and administrative data spanning one century and exploiting the program's assignment criteria for identification, the analysis provides novel evidence of positive and long-lasting effects of place-based industrial policy, with local agglomeration of workers and firms enduring well after its termination. This persistence originates from sustained growth in the services sector, and especially in knowledge-intensive services. The promotion of high-technology manufacturing played a key role in these structural transformations, by boosting demand for business services and developing a skilled local workforce. Accordingly, there are large and persistent effects on local wages and human capital, consistent with agglomeration economies.

Chapter 2 examines the long-run consequences of industrial policy on voting outcomes. It documents that communities that have benefitted of government transfers in the past continue to support state intervention in the economy, decades after transfers have elapsed. This result is not driven by incumbent voting, nor by economic conditions. The paper addresses alternative mechanisms linked to the sectoral composition of employment, migration, and individual attitudes towards the welfare state.

Chapter 3 studies how collective bargaining institutions affect labour market outcomes. It focuses on bargaining decentralization through firm opt-out clauses, where firms can opt out of centralized agreements to negotiate directly with their employees. Leveraging unique matched employer-employee data, the analysis shows that workers experiencing an opt-out suffer wage losses but have higher employment stability. Similarly, opting out firms face lower labour costs and increased survival probabilities.

Impact Statement

This thesis examines two long-debated labour market policies and institutions: industrial policy and collective bargaining. By providing reliable causal evidence and uncovering novel economic mechanisms, my work seeks to improve our understanding on these issues and to inform the academic and policy debate.

Chapters 1 and 2 focus on industrial policy. After decades of reluctance, industrial policy has become prominent in many countries, especially in the form of place-based interventions aiming to create local clusters. This has sparked many questions about the effectiveness of place-based industrial policies (PBIPs). Can they correct market failures and promote local development? Do they lead to inefficiencies? Reliable evidence on the effects of PBIPs remains scarce due to data limitations and identification challenges. Chapter 1 tackles these questions and is the first study highlighting the crucial role of the services sector – typically not the target of industrial policy – in driving the persistent impact of PBIPs through multiplier effects and agglomeration economies. In line with the recent policy discussion, these findings identify the creation of "good jobs" as a key ingredient of successful industrial policy. Additionally, the paper describes how the long-run effects of PBIP depend on the initial conditions in the targeted locations, thus informing the debate on the design and implementation of industrial policy.

These forms of government intervention might have relevant political economy implications, which are the focus of Chapter 2. Moving from the established finding that public transfers reward the incumbent government that promotes them, the paper is the first investigating whether the electoral effects of transfers persist over time and go beyond incumbent voting. The analysis shows that voters' support for state intervention in the economy is significantly higher in response to a program conducted long in the past, and even in the absence of persistent economic effects. These findings contribute to the current debate by highlighting an unintended consequence of government policy, and provide ground for future research on the underlying channels.

Chapter 3 studies collective bargaining institutions, investigating in particular the consequences of bargaining decentralization. Centralized collective bargaining, common in many European economies, is praised for reducing inequality but often criticized for its rigidity. There is indeed intense debate about reform of collective bargaining frameworks to increase flexibility, for example through firm opt-out clauses as advocated by the OECD. To date, however, there is little empirical evidence on how decentralization of this type affects labour markets. The chapter addresses these issues by showing how opt-outs from rigid into more flexible agreements affect workers and firms. In doing so, the study fills an important gap in the literature and provides key insights to policymakers, offering a better understanding of whether a more flexible collective bargaining framework could benefit European labour markets and economies.

Research Paper Declaration Form

- **For a research manuscript prepared for publication but that has not yet been published:**

  1. **What is the current title of the manuscript?**
     Government Transfers and Votes for State Intervention
  2. **Has the manuscript been uploaded to a preprint server 'e.g. medRxiv'?**
     No
  3. **Where is the work intended to be published?**
     American Economic Journal: Economic Policy
  4. **List the manuscript's authors in the intended authorship order:**
     Giuseppe Albanese, Guido de Blasio, Lorenzo Incoronato
  5. **Stage of publication:**
     Forthcoming

- **For multi-authored work, please give a statement of contribution covering all authors:**
  All authors contributed to shaping the research question, conducting the analysis, writing the manuscript and responding to the journal editor and referees.

- **In which chapter(s) of your thesis can this material be found?**
  Chapter 2

e-Signatures confirming that the information above is accurate:

Candidate: Lorenzo Incoronato
Date: May 22, 2024

Supervisor/Senior Author signature: Christian Dustmann
Date: May 22, 2024

Acknowledgements

Personal acknowledgements. I am deeply grateful to my PhD supervisors Christian Dustmann and Uta Schönberg. Christian has been a superb primary advisor. He has believed in my work from the very beginning and encouraged me to push beyond my limits, shaping my way of thinking about economics. Equally important, he has welcome me into an outstanding community of researchers at CReAM and RFBerlin, which I am very proud to belong to and to which I hope to contribute in the future. Uta has been the ideal secondary advisor. She has given me unique exposure to first-class research, teaching me how to ask the right questions and always challenging my ideas with sharp and constructive criticism. If I am excited about this profession and looking forward to the years ahead, it is largely thanks to Christian and Uta.

I thank David Card, Pat Kline and Thomas Lemieux for hosting me at UC Berkeley and UBC. The many conversations I had the privilege of having with them were key to developing my work. I also thank my PhD thesis examiners, Paolo Pinotti and Gabriel Ulyssea, and the broader UCL faculty, especially Kirill Borusyak, Hyejin Ku, Attila Lindner and Imran Rasul, for their precious feedback. I do owe a thanks to Francesco Giavazzi: without his guidance, I most certainly would not be doing what I like today.

I cannot express enough gratitude to my coauthors. I have enormously benefited from working with them, and the constant interactions have made research always fun and engaging. They have become great mentors and friends. I should mention in particular Guido de Blasio, who helped shaping my research agenda on regional policies; and Anna Raute, who sponsored my (many) data trips and always supported me along the way.

The past years would have not been the same without the incredible group of PhD students and postdocs at CReAM and UCL, many of which are now dear friends. Sharing this journey with them has been a true privilege. I also feel lucky to have spent these years in London with amazing people outside of academia, who often provided welcome distractions from work.

Finally, and most importantly, I thank my family for their constant, quiet and unconditional support; and I thank Chiara, who has filled every day with joy.

Funding arrangements. The writing of this thesis has been supported by several scholarships and grants. I acknowledge funding from the Centre for Research and Analysis of Migration, University College London and Rockwool Foundation Berlin. I am also grateful to the Fondazione Zegna for funding part of my PhD through the Ermenegildo Zegna Founder's Scholarship, and to the Fondazione Luigi Einaudi di Torino for sponsoring my research.

Contents

List of Figures

List of Tables

Chapter 1

Place-Based Industrial Policies and Local Agglomeration in the Long Run

Lorenzo Incoronato, Salvatore Lattanzio[1]

1.1 Introduction

In recent decades, advanced economies have witnessed rising spatial inequality as "left-behind" industrial districts struggled to adapt to technical change and globalization. In response to this trend, place-based industrial policies (PBIPs) seeking to bolster local manufacturing and establish industrial clusters have gained traction (Porter, 2000; Kline and Moretti, 2014b).[2] Despite their rising popularity, little is known about the persistent effects of PBIPs on local development. Leveraging a century's worth of data, this paper studies a historical program to assess whether PBIPs benefit the targeted locations in the long run, exploring the sources of persistence, their spillover effects and cost-effectiveness.

There is intense debate on PBIPs among economists and policymakers. While government intervention can correct market failures and foster long-run develop-

[1]Incoronato: UCL; Lattanzio: Bank of Italy.

[2]Many of the industrial policies passed by the United States Congress in 2022 involve the creation of industrial hubs, often in distressed areas, and are *"potentially the most significant place-based policy funding in U.S. history"* (Bartik et al., 2022). Similar shifts towards a place-based approach also feature in the industrial strategies of the European Union (Alessandrini et al., 2019) and the United Kingdom (Fai, 2018).

ment, it can also lead to inefficiencies and misallocation, yielding only temporary benefits (Rodrik, 2019; Heblich et al., 2022). Whether PBIPs favor lasting concentration of economic activity in local communities remains unclear. In addition, these programs might not only impact the targeted industries and locations but produce spillover effects to the rest of the economy. Shedding light on these issues requires examining the impact of PBIP over time and possibly long after its termination. However, reliable evidence is scant as data on historical policies are hard to find and selection problems make causal analysis challenging (Juhász et al., 2023).

This paper takes advantage of a unique historical setting to address these questions. It studies a policy conducted in the 1960s and 1970s to develop industrial clusters in select areas of Southern Italy – the *Industrial Development Areas* (IDAs). Exploiting the criteria ruling the establishment of IDAs for identification, we provide novel causal evidence of positive and long-lasting effects of PBIP, with local agglomeration of workers and firms persisting well after the end of the program.

The IDAs were launched in 1960 as part of a broader regional policy called *Extraordinary Intervention in the Mezzogiorno* (EIM). The EIM was introduced by the government to stimulate economic development in Southern Italy through infrastructure building and investment grants to manufacturing firms. The IDAs were groups of municipalities *within* the EIM jurisdiction identified as suitable hosts for industrial clusters. To direct firms and workers towards IDAs, the government set a higher subsidy rate (hence a lower cost of capital) for firms located in an IDA and financed additional infrastructures. IDA expenses totalled roughly €88 billion, or 0.5 percent of national GDP each year between 1960 and the end of the program in the late 1970s.

The market failure that cluster policies such as the IDAs aim to address are agglomeration economies. As predicted by a simple spatial model, place-based intervention would raise the density of economic agents in the targeted area. In the presence of knowledge spillovers and thick market externalities, higher proximity between agents boosts local productivity. Then, the cluster keeps attracting workers and firms even after subsidies cease and until local prices grow high enough. Because agents do not fully internalize these positive externalities, government subsidies have an efficiency justification (Duranton and Puga, 2004; Moretti, 2011).

A first test of the presence of agglomeration economies (and hence of the success of the intervention) is thus whether the IDA program led to persistently higher economic density, which we compute as the number of workers (and establishments) per square kilometer (km^2). We reconstruct these outcomes for each municipality over one hundred years (1911 to 2011) by manually digitizing historical censuses. The extended time horizon before and after the IDA program allows us to clearly identify its effects and describe how they unfold over time. We complement this dataset with geo-coded records of all the expenses within the policy and rich administrative data for the population of private firms since the 1990s.

Valid identification requires isolating exogenous variation in IDA status, which is challenging for PBIPs due to their selective nature. The criteria set by the government in the late 1950s to establish IDAs offer a unique source of spatial variation. An IDA had to be centered around a large city and included neighboring municipalities. The key requirement was that municipalities bordering the center had to be part of the IDA. This resulted in a "minimum" IDA border traced by municipalities contiguous to the center. Within this cutoff, all municipalities (the center and contiguous ones) were part of the IDA; outside of it, they could be included or not, leading to a 40-percentage-point jump in IDA status at the border.

We exploit this "contiguity rule" in a fuzzy regression discontinuity (RD) design where the running variable is the distance of a municipality from the minimum IDA border and IDA status is the binary treatment. The identifying assumption is that only IDA status changes discontinuously and that areas within and outside of the border are otherwise similar. There are indeed no systematic imbalances in lagged outcomes and other relevant covariates at the RD cutoff before the start of the policy. This is not surprising, as the imposition that municipalities bordering IDA centers be automatically included in the IDA was orthogonal to municipalities' characteristics. To account for unobserved factors, we also rely on a difference-in-discontinuities design that allows for confounding discontinuities at the cutoff as long as they are constant over time – a parallel trends assumption (Grembi et al., 2016).

We estimate a positive effect on employment density emerging while IDAs were in place and continuing to grow afterwards. We measure a discontinuity of about 40 workers per km^2 (50 percent of a standard deviation) at the end of the policy. In 2011 – almost four decades after peak funding in IDAs – the effect is still large at 60 workers per km^2 (60 percent of a standard deviation). We find similar results for firm density. The rise in local employment is, at least in part, driven by higher labor force participation of residents. The novel evidence of *increasing* effects of PBIP after termination stands in contrast with previous findings on industrial cluster policies, which indicate employment effects that are, at best, positive but fading over time (Garin and Rothbaum, 2022). This demands further investigation into the sources of persistence.

Such stark persistence originates from sectors not directly targeted by the policy. By decomposing the baseline effect across sectors, we find that manufacturing – the only subsidized sector – drove most of the growth in employment density during the policy years, but this effect stabilized as subsidies were phased out. In contrast, employment in services started to rise while IDAs were in place and kept growing after their termination. Despite not receiving subsidies, the services sector eventually became the main source of larger agglomeration in IDAs in the long run.

These spillovers to services raise key questions. Why did non-targeted sectors respond to industrial policy? How can the effect on services be so persistent? To answer, we further decompose the response of services. While IDAs were in place, the rise of employment and firm density in services occurred exclusively for non-tradables (e.g., retail, hospitality), in line with local multiplier effects (Moretti, 2010). After the end of the program, however, we document steep growth of knowledge-intensive services (KIS, e.g., information and communication technology, finance, firm services). The creation of new high-skill jobs suggests that PBIP developed a skilled local workforce and stimulated knowledge spillovers, consistent with the presence of agglomeration economies.

These findings are confirmed using an alternative empirical strategy. Exploiting again the contiguity rule described earlier, we compare over time municipalities bordering IDA centers to a new control group: municipalities bordering "placebo centers" in the Center-North of Italy (outside of the EIM jurisdiction). This approach rebuts concerns that our results reflect urban growth, or displacement of economic activity from nearby areas, as the new control group is far away from IDAs and hence unlikely to experience spillovers (Allen and Arkolakis, 2023). In a related exercise, we explicitly estimate the spatial spillovers of the IDA policy by comparing the control group of the baseline design (areas just outside of the minimum IDA border) to its counterpart in the Center-North (areas just outside of the border traced by municipalities contiguous to placebo centers). We find evidence of small displacement effects in manufacturing employment while IDAs were in place, but not in the long run.

These structural transformations towards skilled jobs are primarily a result of the *type* of manufacturing stimulated in the IDAs. We estimate a larger share of manufacturing industries with high technology intensity in treated areas at the end of the policy, which we argue has been crucial for the subsequent development of KIS, in two ways. First, by providing local supply of skilled workers (Hanlon, 2020). Using matched employer-employee data to reconstruct job transitions, we document a growing share of KIS new hires formerly employed in high-technology manufacturing. A second channel is increased demand for high-skill business services such as consulting, human resources and legal activities. Granular industry data confirm that these jobs (and firms) are indeed more widespread in IDAs.

These results suggest that PBIP has successfully promoted long-run development and structural change by creating "good jobs" (Rodrik and Stantcheva, 2021). Accordingly, the effect on local wages is positive and long-lasting. We also estimate a persistently larger share of residents with higher education and skills, consistent with human capital accumulation and knowledge spillovers. Firms in IDAs are more productive and tend to invest more than control firms in the long run, especially in KIS. Last, we find long-run positive effects on local house prices and tax incomes and rule out an alternative source of persistence linked to continued public spending after the policy (von Ehrlich and Seidel, 2018). Taken together, these findings are consistent with agglomeration externalities being subsidized by PBIP and fueling a virtuous cycle in the targeted areas.

Cost-benefit analysis shows that the benefits entailed by the program outweigh the costs. We first calculate a long-term cost per job created of about $30,000, comparable to other regional policies examined in the literature (Criscuolo et al., 2019; Siegloch et al., 2022). We then make a more comprehensive assessment following the approach of Busso et al. (2013a). We compute the net surplus accruing to workers, firms and landlords in the form of wages, profits and housing rents, respectively. In contrast to existing studies, we focus on the surplus generated by the policy only *after* its termination. We find that the present discounted value of the net gains produced between 1991 and 2011 at least compensate for the policy's total costs. These calculations suggest that the IDA program led to a net surplus, assuming that it produced gains also while it was in place or after 2011.

In the last part of the paper, we provide first evidence that the long-run impact of place-based intervention depends on the initial conditions in the targeted locations. We reach this conclusion by comparing the successful experience of IDAs with that of other areas receiving similar subsidies within the EIM program. Namely, we conduct a spatial RD analysis at the border separating the EIM jurisdiction from the rest of Italy following Albanese et al. (2023). For manufacturing employment, we estimate a positive but fading effect qualitatively not dissimilar to that observed for the IDAs. However, employment in services – especially KIS – did not respond to the intervention. There are also no effects on the share of high-technology manufacturing, nor on education and wages.

Comparing these two experiences is instructive. The IDAs were high-potential poles explicitly chosen as future clusters; in contrast, areas around the EIM border had less favorable geography and low density of employment and firms before the policy. Albeit suggestive, these findings illustrate that industrial policy is unlikely to yield long-lasting benefits if implemented in peripheral regions with initial conditions not suitable to future agglomeration.

Related literature and contributions. This paper makes several contributions to the literature. First, it relates to the growing body of research on industrial policies, which despite their broad diffusion remain under-studied in empirical work (Juhász et al., 2023). Recent papers analyzing historical programs have uncovered causal estimates of the effects of industrial policy on local development and structural transformation (Juhász, 2018; Hanlon, 2020; Mitrunen, 2020; Choi and Levchenko, 2021; Giorcelli and Li, 2022; Kantor and Whalley, 2022; Lane, 2022). Our work complements the existing evidence by illustrating how industrial policy shapes the transition towards manufacturing and eventually into advanced services. Specifically, this is the first study providing a detailed account of the dynamic response of the services sector, which is typically not the target of industrial policy.

Second, we contribute to the ongoing debate on place-based policies (Kline and Moretti, 2014b; Neumark and Simpson, 2015; Duranton and Venables, 2018; von Ehrlich and Overman, 2020). In response to skepticism about these programs (Glaeser and Gottlieb, 2008), a growing literature has explored their long-run effects to test for welfare relevant nonlinearities (Kline and Moretti, 2014a).[3] Our focus is on cluster policies, for which most evidence is still short- and medium-run (Falck et al., 2010; Criscuolo et al., 2019; Lu et al., 2019; Cingano et al., 2022; Lapoint and Sakabe, 2022; Siegloch et al., 2022). We complement the scant literature on the long-run effects of cluster policies (Garin and Rothbaum, 2022; Giorcelli and Li, 2022; Heblich et al., 2022) by documenting persistence and offering new insights on the underlying mechanisms. Our work clearly illustrates how the services sector contributes to persistent effects through local multipliers and agglom-

[3]Agglomeration forces might take decades before emerging, which requires tracking the subsidized areas for long enough and ideally well after the termination of the policy (Hanlon and Heblich, 2020).

eration economies. We also identify the policy-driven stimulus to high-technology industries as a key factor. Last, we note that initial conditions matter, and that one of the stated goals of PBIPs – supporting peripheral areas (Bartik, 2020) – might not be fulfilled in places not suited to future agglomeration.

Third, our findings speak to the literature analyzing the manufacturing decline and its impact on labor markets (Moretti, 2012; Autor and Dorn, 2013; Charles et al., 2019; Gagliardi et al., 2023; Helm et al., 2023). If leading to specialization of economic activity and production in a limited set of industries, industrial interventions might undermine long-run development when manufacturing districts must adjust to technological shifts (Barba Navaretti and Markovic, 2021).[4] In contrast, we show that PBIP has expedited structural change in the targeted areas, which transitioned into diversified poles integrating high-skill manufacturing and services.[5] The novel evidence we provide on the ability of PBIP to incentivize high-skill jobs resonates with Rodrik and Stantcheva (2021), who advocate the creation of "good jobs" (and of firms demanding them) as the main target of industrial policy going forward.

Fourth, our results add to the existing evidence on local multipliers (Moretti, 2010; Faggio and Overman, 2014; Becker et al., 2021) and, more broadly, on the spillovers of (place-based) industrial policies to non-targeted sectors and locations (Greenstone et al., 2010; Atalay et al., 2022; Giorcelli and Li, 2022; Lane, 2022; Siegloch et al., 2022). We are the first to break down the spillovers of PBIP across different classes of services, better assessing how these programs shape the structure of the economy. This study also provides the first *dynamic* estimates of the spillover effects of place-based policy to nearby locations, showing displacement of economic activity away from non-targeted areas during the intervention but not

[4] Heblich et al. (2022) study the construction of large plants in China in the 1950s and document a boom-and-bust pattern in host counties, which developed a very specialized production structure with limited technology spillovers. Resonating findings are obtained in Kim et al. (2021) for the South-Korean heavy industry drive.

[5] As showed in Gagliardi et al. (2023) for advanced economies, some manufacturing hubs navigated deindustrialization better than others depending on the share of college-educated workforce, which then led to growth in knowledge intensive services. Our paper highlights the role that government policy can play in this process.

in the long run.

Last, this paper produces novel causal evidence on the EIM – the most ambitious regional program in Italy's history (Felice and Lepore, 2017). Recent studies in political economy (Colussi et al., 2020; Buscemi and Romani, 2022) consistently report a null economic impact of the EIM in the long term. Among these, Albanese et al. (2023) find that EIM transfers led to a transition out of agriculture towards industry, halted the growth of services and did not raise local employment in the long run. We show instead that the intervention has successfully promoted development in a few targeted areas of Southern Italy – the IDAs. Our results also relate to Cerrato (2024), which focuses on the aggregate welfare consequences of the EIM and documents net gains in national industrial production. Our analysis examines more in depth a specific dimension of the EIM – the IDAs – and goes beyond the direct impact on manufacturing, unveiling the effects of the program on other areas of the economy and unpacking the sources of persistence.

The paper is organized as follows. Section 1.2 provides an overview of the policy; Section 1.3 describes the data sources; Section 1.4 outlines the identification strategy; Section 1.5 presents the baseline results; Section 1.6 explores the underlying mechanisms; Section 1.7 conducts cost-benefit analysis; Section 1.8 further discusses our findings. The last Section concludes.

1.2 Historical background

The North-South economic divide has been a recurring theme in Italy's policy debate, particularly so in the aftermath of World War II when this gap was at its peak. An ambitious regional policy called *Extraordinary Intervention in the Mezzogiorno* (EIM) was put in place by the central government in 1950 to jump-start development in an area covering 40 percent of Italy's surface (Law n. 646/1950).[6] The program had an initial lifespan of ten years, which was then prolonged several

[6]GDP per capita in the South was roughly half of that if the Center-North in 1951 (Felice, 2017). See Iuzzolino et al. (2011) and De Philippis et al. (2022) for details on the Italian North-South divide. The term Mezzogiorno ("Midday") is conventionally used to identify the South of Italy.

times until 1992. The government mandated the intervention to a state-owned agency called *Cassa per il Mezzogiorno* (Cassa).

At its onset, the main goal of the EIM was to accelerate structural transformation by enhancing agricultural productivity and promoting a shift to manufacturing. To achieve this, the Cassa financed infrastructure interventions (mostly in transportation and water supply) during its first decade of activity (see Appendix 1.A.1 for details on the functioning of the EIM). A new phase of the EIM began in the late 1950s, when the program was extended both in time and scope and its focus shifted markedly towards industrial policy to support businesses in Southern regions and attract investments.[7]

To pursue its new mandate, the Cassa conceded capital and interest grants to firms located in its jurisdiction. The eligible investments were those for building new plants, enlarging existing ones, purchasing machinery and performing works such as connections to energy and transport services. The following years saw a dramatic increase in EIM expenses, which during the 1970s reached yearly peaks of roughly 2 percent of Italy's GDP and 8 percent of aggregate investment.

The core of this industrial policy (and the focus of our paper) were the *Industrial Development Areas* (IDAs), established during the 1960s. The IDAs were clusters of municipalities within the EIM region identified as suitable for industrial agglomeration, with the goal of "*clearly directing the location choices of economic agents*" and "*establishing positive externalities thanks to the proximity to other industries and workers*" (Cassa's Annual Report, 1958-59).

An IDA was created upon the initiative of a group of local authorities (municipalities and provinces) called a *consortium*. The consortium submitted a development plan for the area to the Cassa, outlining the proposed investments and reporting information about the included municipalities. Each IDA was centered around a provincial capital and extended to more municipalities surrounding the

[7] In the policymaker's words, entrepreneurs located in the South (or willing to locate there) needed to be compensated "*for the natural inferiority of the Mezzogiorno relative to other areas, with its subsequent costs and risks*" (See Cassa's Annual Report, 1957-58 and Laws n. 634/1957 and n. 555/1959).

center, subject to a minimum population threshold (200,000 people as of 1958). Other requirements were related to the geological properties of the area (e.g., low seismicity) and to the presence of basic infrastructure.

Subject to the government's approval of the plan, the Cassa could subsidize the expenses borne by each consortium in its IDA.[8] In addition, the investment grants for individual firms in the EIM area were more generous for firms located in IDAs, which thus faced even lower cost of capital than other EIM firms. This was achieved in two ways. First, the investment subsidy rate was larger for IDA firms. Second, only small- and medium-sized firms in small EIM municipalities could access grants, while there were no size limits for IDA firms.[9]

The IDA program was effectively in place for almost two decades from 1960 until the late 1970s, when investment grants for IDA firms were equalized to those for other firms in the EIM region. EIM transfers continued also through the 1980s, but with no distinction between IDAs and other EIM municipalities. The EIM was terminated by Law n. 488/1992, as the system of state holdings was dismantled or privatized. The Law introduced a new set of firm subsidies that also covered depressed areas in the Center-North (Bronzini and de Blasio, 2006; Cerqua and Pellegrini, 2014; Cingano et al., 2022).

1.3 Data

Identifying the effects of the IDA program over time, disentangling the mechanisms and making cost-benefit assessments requires rich longitudinal data spanning a long time period. This paper draws on several unique data sources.

[8]These included connections to transport and energy services, the construction of plants and houses for workers and their families, and the provision of professional training classes. The original subsidy rate for these expenses was 50 percent, which rose to 85 percent in 1961.

[9]The Cassa was pursuing two separate industrial policy goals. The first ("*industrial concentration*") was to establish large industrial clusters (the IDAs). The second ("*industrial diffusion*") was to favor industrial development in peripheral regions by supporting small firms in municipalities with limited industrial activity.

Interventions from the Cassa. We collect information on the universe of Cassa's interventions from the ASET database, which stores recently digitized records of the agency's activities since its inception in 1950.[10] Records for roughly 110,000 firm subsidies are available and collate information on the grant's amount, year, sector and municipality. The data also include about 75,000 infrastructure projects reporting the financial resources allocated as well as the year, location and type of infrastructure.

Panel (a) in Figure 1.1 shows total EIM expenses (excluding concessional loans) by year, scaled by the total population in the EIM region in 1951. The program only performed infrastructure works during its first decade (the 1950s). A strong industrial push then began in the 1960s with a massive rise in firm investment subsidies.[11] Panel (b) shows that most EIM expenses were concentrated in IDA municipalities, in particular during the peak in the 1960s and 1970s. Especially for IDAs, firm grants went disproportionately to capital intensive industries such as chemical, metallurgy and transport manufacturing.[12]

The ASET archives also provide a list of the IDAs, along with all the included municipalities, which we digitize and plot in Figure 1.2. A total of 14 IDAs comprising 328 municipalities have been established throughout Southern Italy during the 1960s. These are indicated on the map by the yellow regions surrounding the brown IDA centers. On average, IDA municipalities received EIM funding of around €10,000 (cumulated between 1950 and 1992 and measured in 2011 prices) per 1951 resident, twice as much as other EIM municipalities (these differences do not change much if excluding IDA centers). IDA municipalities absorbed more than half of the overall EIM expenses (cumulative €165 billion), despite covering about one tenth of the surface of the entire EIM region and hosting one third of its population.

[10] The ASET (Archives for the Regional Economic Development) project, launched in 2013, was set up to catalogue and preserve the archives and balance sheets of the Cassa.

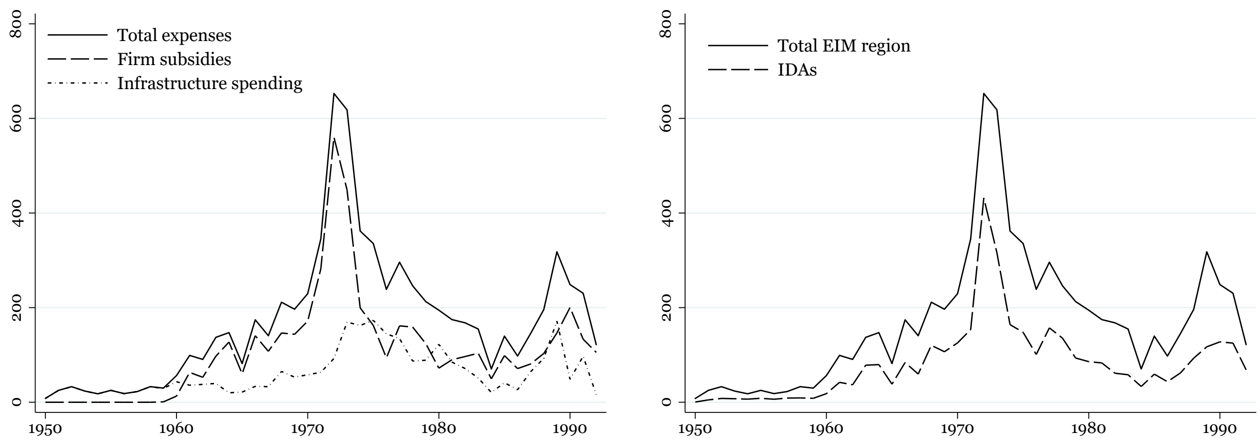
[11] Law n. 853/1971 boosted the Cassa's spending by raising both the agency's financial endowment and the maximum proportion of firm investment that could be financed by a grant.

[12] We describe the ASET data and provide more detail about the Cassa's interventions in Appendix 1.A.1.

Figure 1.1. EIM expenses

(a) Expenses breakdown

(b) Full EIM area and IDAs



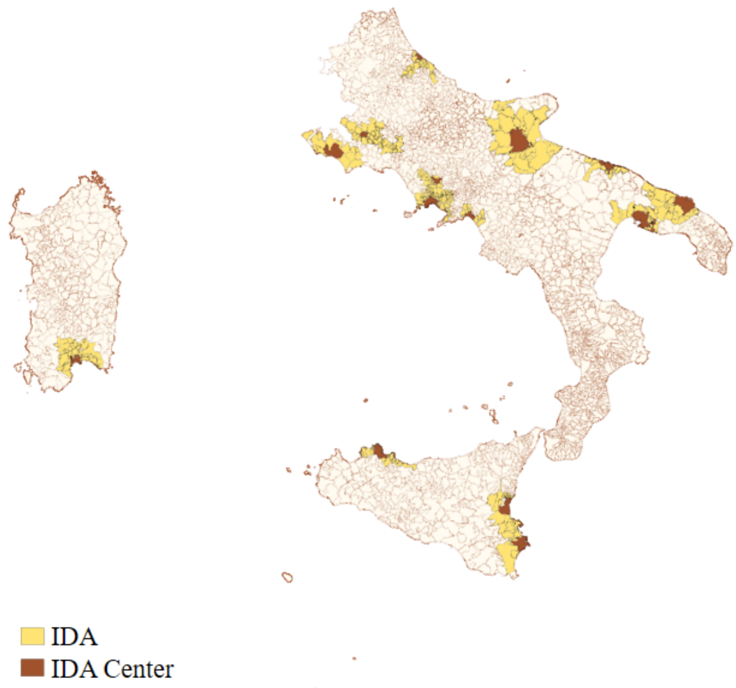
EIM expenses in €(2011 prices) scaled by total population in the EIM region in 1951. Concessional loans to firms are excluded.

Industrial censuses. The main outcome variable of the paper (employment density) is computed using the number of workers per municipality reported in decennial industrial censuses spanning six decades (1951 to 2011, including an intermediate census in 1996), sourced from the Italian statistical institute (Istat). The data allow us to reconstruct employment and establishment counts separately for manufacturing and services. The availability of data well after the end of the policy enables us to tackle key questions on its long-run effects. However, only the 1951 census allows us to evaluate the balancing properties of the outcome prior to the policy, which is essential for identification purposes.[13] We thus reconstruct the evolution of employment (and the number of establishments) across municipalities long before the start of the EIM by manually digitizing the 1911 and 1927 industrial censuses, available in the historical archives of Istat (Appendix 1.A.2).

Social security data. The third main data source of the paper is the administrative archive on the universe of Italian employers in the non-agricultural private sector from social security records (INPS), available at the Bank of Italy. The data

[13]While the EIM was inaugurated in 1950, actual intervention began in the early 1950s and involved infrastructure works only. The Cassa's industrial policy (including the IDA program) started in the 1960s.

Figure 1.2. The Industrial Development Areas



The map shows the EIM jurisdiction. IDA centers are in brown and the other IDA municipalities in yellow. The IDA centers are Latina, Frosinone, Caserta, Napoli, Salerno, Pescara, Foggia, Bari, Taranto, Brindisi, Palermo, Catania, Siracusa, Cagliari.

start in 1990 and include detailed information on firm employment counts, 6-digit sector, location, workforce composition and average wage paid. Importantly, the granular sector-level information will allow us to distinguish manufacturing activities by technological intensity and service activities by knowledge content using the Eurostat/OECD classification. We complement the data with income statements collected by Cerved, matched using firm tax identifiers. The data are available for incorporated limited liability companies and report detailed balance sheet information. Last, we obtain matched employer-employee data by merging the firm dataset with a 7 percent random sample of Italian workers. Importantly, we collapse these micro data at a more aggregate level of analysis (the municipality) as we cannot match the ASET establishment-level subsidy data with the INPS records. We describe this data source more in detail in Appendix 1.A.3.

Other data sources. We use decennial population censuses between 1951 and 2011, reporting municipality-level information on demography and labor markets. We also collect data on geographical traits (mean elevation, mountain surface, seismicity) from Istat. The other sources we use are the OpenCoesione database (funding within Law n. 488/1992 and EU structural funds), the Italian Ministry of the Interior (election data), the Italian Finance Ministry (taxable income), the Osservatorio del Mercato Immobiliare (OMI) at the Italian Tax Office (house prices) and AIDA PA (municipality balance sheets and spending information).

1.4 Identification strategy

The selective nature of PBIPs such as the IDAs makes identification of causal effects challenging. The locations targeted by these programs are not randomly picked but tend to differ from other areas in many dimensions, potentially unobserved and likely correlated with future economic outcomes. IDA municipalities were positively selected, as their choice was explicitly informed by their agglomeration potential. As a necessary condition for eligibility, the government imposed that the candidate area showed a "*propensity for industrial concentration*" (Ministerial Circular n. 21354/1959). Many years before the start of the program, IDA municipalities featured a larger density of workers and establishments relative to other EIM municipalities (Table 1.1). They were also more densely populated, their residents were more educated and less likely to work in agriculture, and their geography was more suited to industrialization.

These traits make IDA municipalities uncomparable to other municipalities in Southern Italy. Performing a causal evaluation of the IDA program requires isolating exogenous variation in IDA status to account for selection. To this end, we examine the criteria ruling the establishment of an IDA, which were set in the late 1950s. As explained in Section 1.2, IDAs were centered around a large city (a provincial capital) and then included municipalities in its surroundings up to a minimum population threshold.[14] Importantly, the government required that the

[14] The consortium could add more municipalities not farther than 25 km from the IDA center, a limit set to avoid the mechanic inclusion of more areas to meet the population requirements. This limit was respected, and there is no discontinuity in IDA status at the 25 km distance cutoff.

minimum set of municipalities forming an IDA should be the IDA center and all municipalities directly contiguous to it.

Table 1.1. IDA municipalities – descriptive statistics

	IDA muni.	IDA muni. excl. centers	Other EIM muni.
Employment density (1951)	48.57	39.88	9.69
	(119.24)	(89.05)	(19.30)
Establishment density (1951)	16.92	15.42	4.74
	(27.27)	(23.84)	(7.45)
Manuf. employment density (1951)	21.80	18.86	4.19
	(60.12)	(52.99)	(9.41)
Manuf. establishment density (1951)	5.90	5.46	2.08
	(9.46)	(8.60)	(2.63)
Population density (1951)	642.30	596.44	162.99
	(1025.90)	(918.83)	(325.32)
Agriculture share (%, 1951)	27.83	28.76	38.63
	(14.35)	(13.93)	(13.81)
High school education (%, 1951)	2.31	2.08	1.76
	(1.58)	(1.17)	(0.94)
Mean elevation	148.23	151.17	468.17
	(133.97)	(135.47)	(318.56)
Slope	381.77	382.39	725.14
	(412.46)	(416.94)	(468.80)
Coastal location	0.23	0.20	0.16
	(0.42)	(0.40)	(0.37)
Number of municipalities	326	312	2327

Sample restricted to the EIM region. Employment and establishments (total and manufacturing) are sourced from the 1951 industrial census. "Agriculture share" computed as the number of agriculture workers per 100 residents aged at least 15. "High school education" denotes the share of people aged at least 6 with high school education or more. "Mean elevation" measured in meters. "Slope" denotes the distance in meters between the highest and the lowest point in the municipality. "Coastal location" is a dummy equal to one for municipalities located by the sea. Standard deviations in parentheses.

The government imposition that municipalities bordering the center be automatically included in the IDA is exploited for identification. The outer boundaries of the contiguous municipalities trace a "minimum" IDA border $\mathcal{J}$ that separates two regions within ($\mathbb{W}$) and outside ($\mathbb{O}$) this boundary. Figure 1.3 Panel a) provides an illustration. Let the centroid of municipality m be denoted by the latitude-longitude pair $\ell_m = (l_{x,m}, l_{y,m})$. Let also $\delta_m \equiv d(\ell_m, \mathcal{J})$ denote the geodesic distance between municipality m's centroid and the minimum border of the closest IDA.

Negative values of the distance δ_m are assigned to municipalities in region $\mathbb{W}$, that is, the IDA centers and its bordering neighbors. To identify these municipalities, we define the binary instrument $W_m = \mathbb{1}[\ell_m \in \mathbb{W}] = \mathbb{1}[\delta_m \leq 0]$. Let also IDA_m be a treatment indicator taking value of one if municipality m belongs to any of the 14 IDAs. To the extent that the probability of belonging to an IDA changes discontinuously at the cutoff $\mathcal{J}$, the distance metric δ_m can be used as running variable in a fuzzy RD setting where IDA_m is the treatment variable and Y_m is the outcome:

$$IDA_m = \mu_{i(m)} + \vartheta \cdot W_m + \varphi(\delta_m) + u_m \tag{1.1a}$$

$$Y_m = \mu_{i(m)} + \pi \cdot W_m + \varphi(\delta_m) + v_m \tag{1.1b}$$

Where Equation 1.1a is the first-stage regression and Equation 1.1b is the reduced form. $\varphi(\delta_m)$ is a linear RD polynomial and $\mu_{i(m)}$ denotes IDA regions comprising all municipalities within 25 km of each of the IDA centers (the limit for IDA inclusion), regardless of whether they belong to the IDA. Y_m, IDA_m and W_m are defined above.

The peculiarities of this design pose restrictions on the choice of the bandwidth. Within the minimum IDA border, there are only 14 IDA centers and 137 bordering municipalities. The limited sample size requires picking a bandwidth wide enough to include all these municipalities, equivalent to 16 km. We then adopt a symmetric bandwidth of 16 km outside of the minimum IDA border, although results are robust to the choice of different bandwidths, as showed later.

This identification strategy rests on three main assumptions:

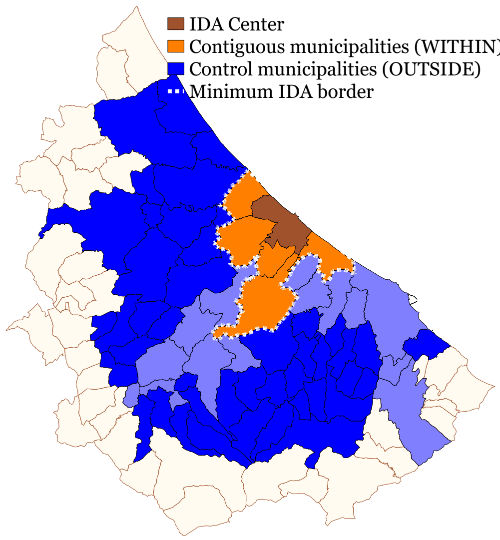
A1. Relevance. *The minimum IDA border induces a discontinuous jump in treatment status IDA_m:* $\lim_{\delta_m \to 0^+} Pr(IDA_m = 1 \mid \delta_m) < \lim_{\delta_m \to 0^-} Pr(IDA_m = 1 \mid \delta_m)$

Assumption A1 essentially requires that there is a first stage. To illustrate the idea, Figure 1.3 Panel b) plots the probability that municipality m belongs to an IDA as a function of the running variable (distance to the minimum IDA border), $Pr(IDA_m = 1 \mid \delta_m)$.[15] A neat drop in IDA status is detected at the boundary, which

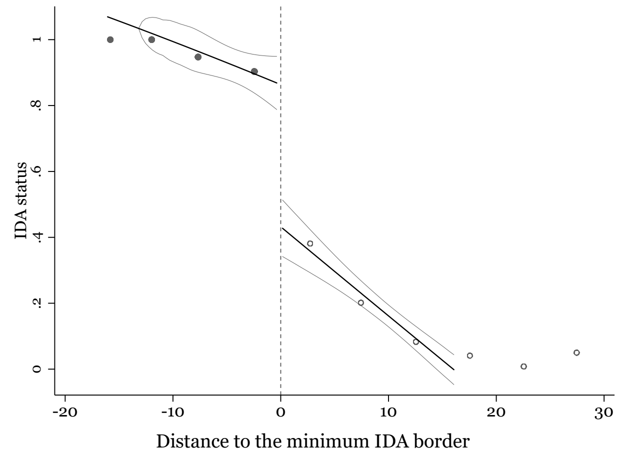
[15] Two IDAs (Napoli and Caserta) have been excluded from the sample due to the proximity of their centers (about 25 km). This reduces the sample within the minimum IDA border to 12 centers

Figure 1.3. The minimum IDA border

(a) Illustration



(b) IDAs – First stage



Panel a) shows the minimum IDA border for one of the IDAs (Pescara). The IDA center (the municipality of Pescara) is in brown and the contiguous municipalities are in orange. Their outer boundary traces the minimum IDA border (the dashed white line). Treated municipalities (those belonging to the Pescara IDA) are the center, the contiguous municipalities and the light blue municipalities outside of the minimum IDA border. The dark blue municipalities do not belong to the IDA. Panel b) shows the jump in IDA status at the cutoff. The outcome variable is $Pr(IDA_m = 1 \mid \delta_m)$. Negative distance denotes municipalities within the minimum IDA border. See Footnote 16 for an explanation of the non-unitary treatment probability within the cutoff. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

provides graphical evidence in favor of Assumption A1. IDA status is very close to one within the RD cutoff and drops to about 50 percentage points right outside of it.[16]

Table 1.2 reports the estimation output of the first-stage Equation 1.1a. The drop in IDA status detected in Figure 1.3 Panel b) is quantified at 39 percentage points, and associated with less generous EIM funding by €5,720 per capita. This discontinuity in EIM expenses is almost entirely driven by firm subsidies, although our data only capture the infrastructures expenses from the Cassa and not those borne by the IDA's consortium.

---

and 112 bordering municipalities. Results do not change when these two IDAs are included.

[16]The probability of belonging to an IDA is not exactly one within the cutoff, as very few (10) municipalities bordering IDA centers were not part of the IDA. The government admitted exceptions to the contiguity rule if "*a municipality of very large extension is contiguous to the main municipality for a limited stretch of the perimeter*" (Ministerial Circular n. 21354/1959).

Table 1.2. IDAs – First stage

	IDA status	EIM expenses
RD Estimate	0.39	5.72
	(0.09)***	(2.50)**
Mean around the border	0.36	7.41
Standard deviation	0.48	13.54
Observations	587	563
R^2	0.46	0.11

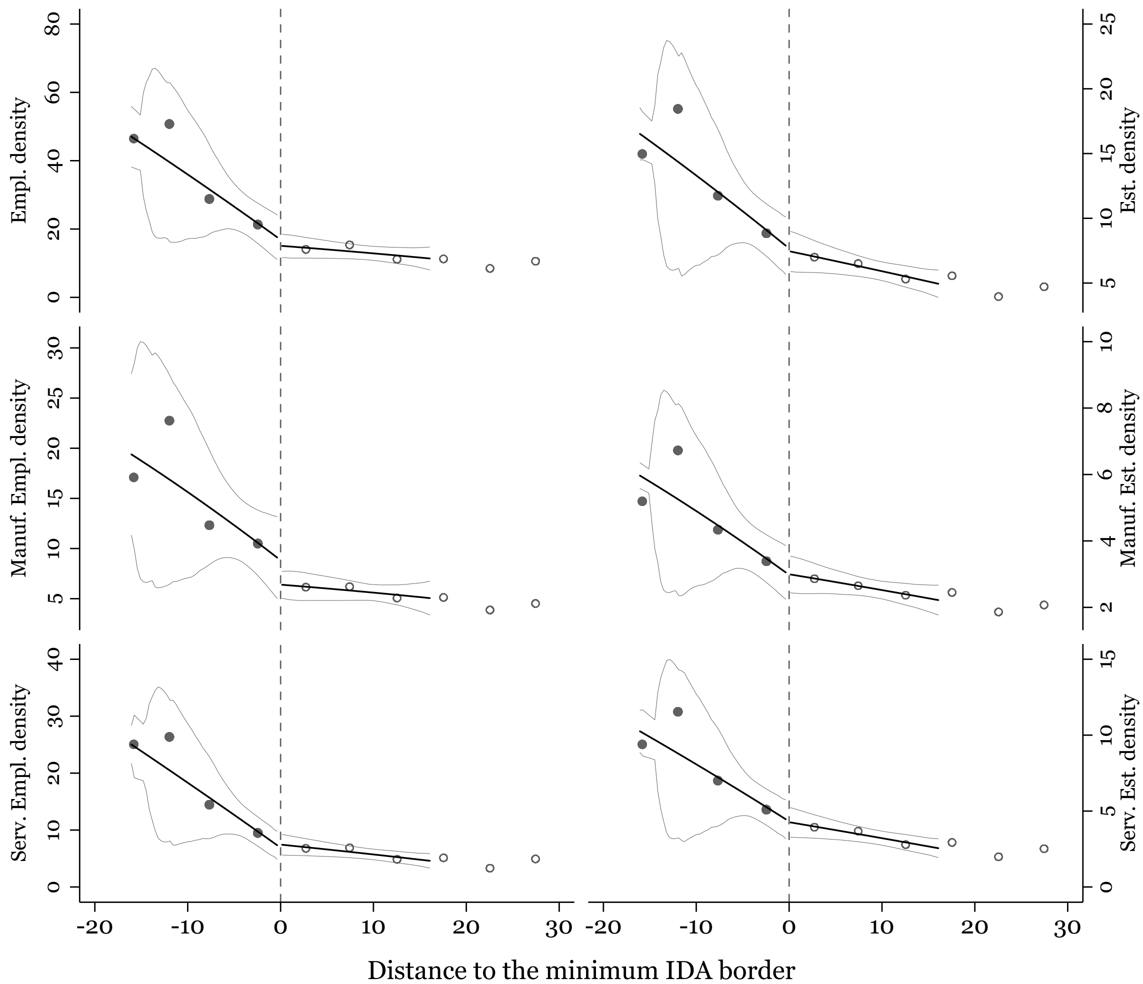
Estimation output of Equation 1.1a using a 16-km symmetric bandwidth around the minimum IDA border. The specification controls for a linear polynomial in the distance to the border and for IDA region effects. EIM expenses measured in thousand € (2011 prices) per 1951 resident, winsorized at 1 and 99 percent. Standard errors clustered by IDA region in parentheses. See text for details. * p<0.10, ** p<0.05, *** p<0.01

A2. Continuity. *Mean potential outcomes* $E[Y_m(0) \mid \delta_m]$ *and* $E[Y_m(1) \mid \delta_m]$ *are continuous at* $\delta_m = 0$.

Where $Y_m(0)$ and $Y_m(1)$ denote potential outcomes under control and treatment status, such that $Y_m = Y_m(0) + IDA_m \cdot (Y_m(1) - Y_m(0))$. Assumption A2 requires relevant factors other than IDA status not to jump at the minimum IDA border, thus enabling to causally attribute any observed change in outcomes to the treatment. This condition essentially becomes an exclusion restriction in a fuzzy RD setting (Cattaneo and Titiunik, 2022).

While the assumption is not testable, we argue that it is most likely satisfied in our analysis. The contiguity rule, which gives rise to the minimum IDA border, is an arbitrary choice of the government. While potential outcomes are certainly related to the distance to a large city (the IDA center), there are no reasons to expect discontinuous jumps in such relationship. To confirm this, we look for discontinuities in lagged outcomes at the cutoff. Figure 1.4 shows RD plots for employment and establishment density in 1951 (a decade before the introduction of the IDAs). Unsurprisingly, agglomeration in 1951 was larger 10-15 km within the boundary, corresponding to the IDA centers. Yet there is no discontinuity at the cutoff itself, as municipalities contiguous to the IDA center were very similar to those further away from the center before the start of the policy.

Figure 1.4. Balancing at the minimum IDA border, 1951



Number of workers and establishments are sourced from the 1951 industrial census. Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure B1.1.1 shows RD plots for many other pre-determined covariates and confirms little or no discontinuities in labor market and demographic characteristics including the employment rate, population density, education and population age and gender composition. There is also balancing in geographical traits and, importantly, in voting outcomes before the policy (measured as the votes share for the incumbent Christian Democratic party). The lack of a discontinuity in electoral preferences reassures that IDA inclusion was not driven by political considerations.[17] To address concerns about unobserved confounders jumping at the cutoff, we will test our results under an alternative identification design that, again exploiting the contiguity rule, uses a new control group composed of municipalities bordering provincial capitals in the Center-North of Italy.

A3. Local monotonicity (no defiers). *There exists a neighborhood* $\mathbb{S}$ *of the cutoff where no municipality is such that:* $IDA_m(\delta_m) = 1 - W_m$

Where $IDA_m(\delta_m)$ denotes potential treatment selection as a function of the running variable. Assumption A3 requires that there is no municipality that would belong to an IDA if and only if it was not contiguous to the IDA center. Three municipality types are therefore allowed to exist in the proximity of the cutoff: always-takers ($IDA_m(\delta_m) = 1$), never-takers ($IDA_m(\delta_m) = 0$) and compliers ($IDA_m(\delta_m) = W_m$).

Proposition 1. *Under A1, A2 and A3 the fuzzy RD estimand* $\beta = \pi/\vartheta$ *identifies the local average treatment effect (LATE) for the sub-population of compliers.*

Proof. See Appendix 1.B.2.

This empirical approach does not exploit the longitudinal dimension of our data. In fact, we observe the main outcomes (employment and firm density) at ten points in time (1911, 1927, 1951, 1961, 1971, 1981, 1991, 1996, 2001 and 2011) spanning one century. This allows us to corroborate our identification by accounting for unobserved, time-constant municipality characteristics. The regression form is a difference-in-discontinuities (Diff-in-Disc) design (Grembi et al., 2016) – a dynamic specification of the reduced-form Equation 1.1b:

$$Y_{m,t} = \mu_m + \sigma_t + \sum_{j \neq 1951} \rho_j \cdot \mathbb{1}[t = j] \cdot W_m + \epsilon_{m,t} \tag{1.2}$$

Where $Y_{m,t}$ is the outcome for municipality m and census year t, μ_m are munici-

[17] We also check for imbalances in other sources of government funding before the IDAs. First, there is no discontinuity in EIM infrastructure spending during the 1950s. Second, the intensity of allied bombing during World War II does not change at the cutoff, likely implying no difference in Marshall Plan funding (Gagliarducci et al., 2020; Bianchi and Giorcelli, 2023).

pality effects and σ_t are census year effects capturing aggregate shocks. The specification tracks municipalities contiguous to IDA centers over time (excluding the centers themselves) and compares them to municipalities 16 km away from the minimum IDA border. The coefficients of interest ρ_j capture the difference in outcomes between municipalities within and outside of the cutoff in census year j relative to the baseline difference in 1951, which is normalized to zero. Valid identification no longer requires continuity of potential outcomes at the cutoff, but hinges on the weaker assumption that outcomes in municipalities bordering IDA centers would have behaved similarly to municipalities right outside of the cutoff in the absence of the policy. An indirect test of this parallel trends assumption is provided by the coefficients ρ_{1911} and ρ_{1927}, which should be undistinguishable from zero.[18]

Other identification strategies. The paper leverages two more designs. First, we will again exploit the contiguity rule and focus on provincial capitals in the Center-North of Italy, which would have most likely been candidate IDA centers had they been part of the EIM region. In turn, municipalities bordering these cities can be used as an alternative control group in an event-study design. This source of variation will also be used to estimate the displacement effects of the IDA program, and will inspire a triple differences approach (Appendix 1.B.3). Second, we will compare our main results to those derived from a spatial RD design at the border separating the EIM jurisdiction from the rest of Italy (Appendix 1.B.4).

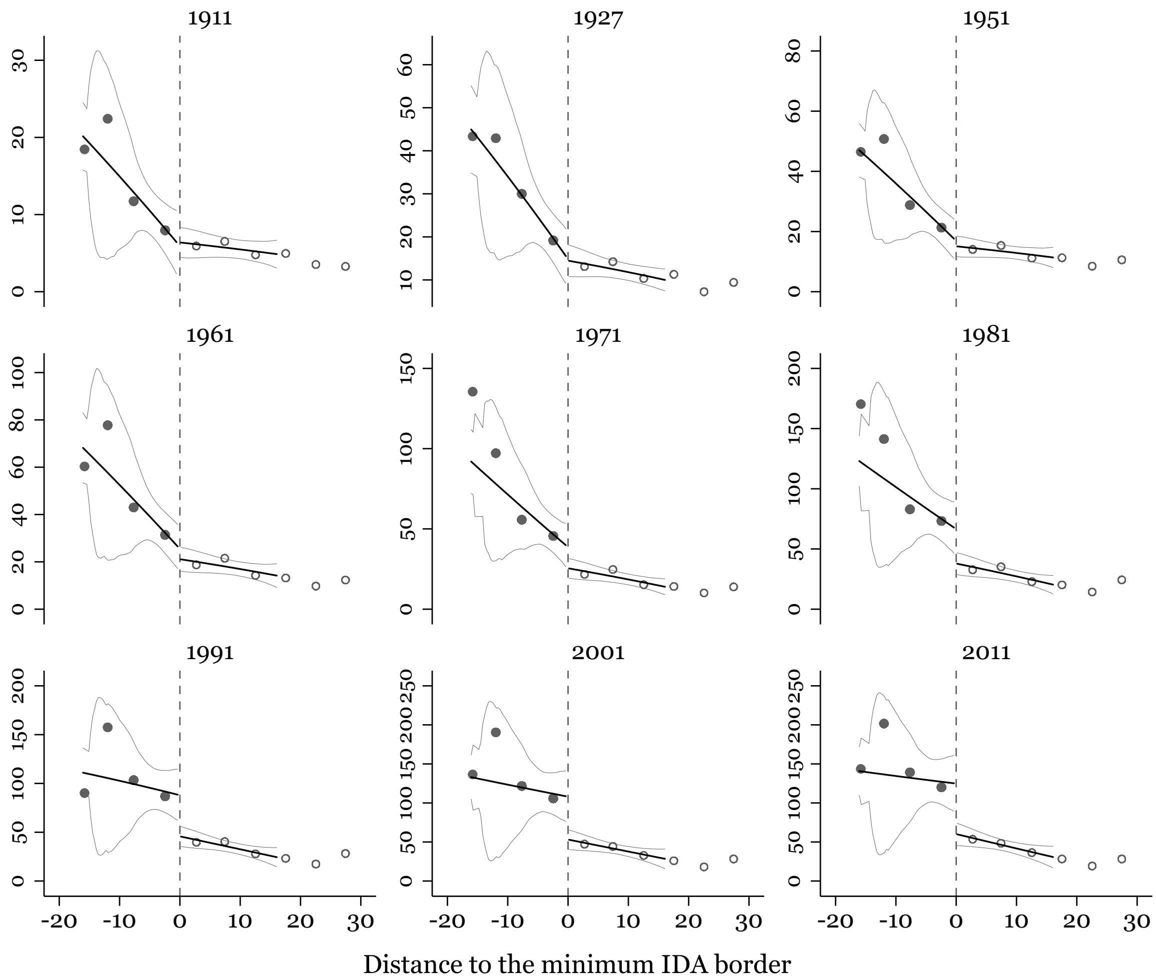
1.5 Results

How has the IDA policy affected local employment? Viewed through the lens of a simple model of spatial equilibrium, which we develop in Appendix 1.C.1, a place-based policy that alters the relative cost of capital across locations is expected to

[18]We focus on reduced-form estimates where W_m is the treatment, but our results easily extend to a fuzzy design under realistic assumptions. See Millán-Quijano (2020) and Appendix 1.B.2 for details.

shift up the (relative) labor demand curve and, in turn, raise employment in the targeted area.[19] To test this prediction, we first provide graphical evidence by plotting employment density around the minimum IDA border, then show regression estimates to quantify the discontinuities.

Figure 1.5. Employment density



Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Graphical evidence. Figure 1.5 shows RD plots for employment density around the minimum IDA border in each census year. There is no tangible difference in ag-

[19]The same effect would arise in response to other IDA measures, such as infrastructure works and training classes for workers, that would raise local productivity (Kline and Moretti, 2014b).

glomeration at the cutoff not only at the onset of the EIM in 1951 (as showed above) but also in the previous decades (1911 and 1927), which lends more evidence in favor of the continuity assumption. Starting in the 1970s a positive discontinuity emerges at the cutoff, as agglomeration increased in municipalities bordering IDA centers relative to those immediately outside of the cutoff. The jump at the border remains visible at the end of subsidies in 1991 and, importantly, also in the following decades. We document a very similar pattern for firm density, as showed in Appendix Figure C1.2.1.

Baseline estimates. Table 1.3 shows the baseline regression estimates for employment density separately for 1991 (right at the end of the intervention) and 2011 (the latest period we observe).[20] Column (1) reports the reduced-form estimates of the sharp RD design in Equation 1.1b. We quantify the discontinuity in 1991 at about 43 workers per km^2, or roughly half of a standard deviation in the estimation sample. By 2011, the RD coefficient rises to about 63 workers per km^2 (60 percent of a standard deviation). In logarithmic terms, these effects are equivalent to 51 percent in 1991 and 55 percent in 2011 and are comparable in magnitude to those in von Ehrlich and Seidel (2018). Column (2) reports the 2-SLS estimates for the LATE, which is estimated at 111 workers per km^2 in 1991 and 161 workers per km^2 in 2011. Column (3) replaces IDA status with EIM funding per municipality resident (as of 1951) as treatment variable. A rise in subsidies of €1000 (2011 prices) per 1951 resident (about 13 percent of the mean, see Table 1.2) leads to 7.2 more workers per km^2 in 1991 and 10.3 more in 2011. We interpret these estimates with more caution in light of the weak first stage.

[20]Appendix Table C1.2.1 shows results for firm density. Even though IDAs were effectively in place until the late 1970s, we consider 1991 as the end of the intervention as IDA municipalities continued to receive EIM transfers until the end of the EIM in 1992. In addition, we show the effect in 1991 rather than in 1981 to preserve consistency with the results (showed later) obtained from social security data, which are not available before 1990. That said, results for 1981 do not differ meaningfully from those for 1991.

Table 1.3. Employment density – Baseline

	Reduced form	2-SLS	
		IDA status	EIM subsidies
	(1)	(2)	(3)
	Contemporaneous effect (1991)		
RD Estimate	43.31	110.82	7.23
	(19.08)**	(43.03)**	(3.26)**
Mean around the border	47.62	47.62	46.63
Standard deviation	79.68	79.68	78.05
Observations	586	586	562
R^2	0.22		
KP F-stat		19.06	5.18
	Persistent effect (2011)		
RD Estimate	62.99	161.16	10.34
	(27.18)**	(63.14)**	(4.49)**
Mean around the border	62.97	62.97	61.42
Standard deviation	108.15	108.15	105.18
Observations	586	586	562
R^2	0.24		
KP F-stat		19.06	5.18

Column (1) shows the estimation output of Equation 1.1b. Column (2) reports the fuzzy RD estimates. Column (3) replaces IDA status with EIM subsidies as treatment variable. All regressions are estimated over a 16-km symmetric bandwidth around the minimum IDA border and control for a linear polynomial in the distance to the border and IDA region effects. Standard errors clustered by IDA region in parentheses. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Robustness tests. The baseline estimates are robust to several checks, presented in the Appendix. Table C1.2.2 reports robustness tests to i) more flexible polynomial specifications of the RD control function; ii) excluding IDA centers from the sample; iii) controlling for distance to the IDA center; iv) excluding IDA region effects from the specification. The estimated discontinuity moderately declines (but remains large and significant) when using a quadratic or cubic RD polynomial and when excluding IDA centers. The effect stays roughly unchanged both in magnitude and significance if controlling for the distance to the IDA center or excluding IDA region dummies. Tables C1.2.3 and C1.2.4 show that results are robust when allowing for spatial correlation in standard errors (Conley, 1999), or conducting local randomization inference (Cattaneo et al., 2016). Table C1.2.5 shows that results do not change if including two IDAs (Napoli and Caserta), which are excluded in the baseline analysis because of the short distance (about 25 km) between the two centers. Figure C1.2.2 shows that the fuzzy RD coefficient remains stable as we replicate the baseline estimation excluding one IDA region at a time, confirming that results are not driven by a specific IDA. Last, Table C1.2.6 presents non-parametric estimates obtained through the algorithm proposed in Calonico et al. (2014a). We weigh each municipality using a triangular kernel function giving more weight to places close to the cutoff. We also compute an MSE-optimal bandwidth that is allowed to differ within and outside of the cutoff. This procedure delivers indeed quite a narrow bandwidth within the cutoff (6-7 km), effectively focusing only on the contiguous municipalities. The RD coefficient rises in magnitude but is less precisely estimated – most likely because of the small number of observations within the cutoff.

Bandwidth choice and spillovers. Figure C1.2.3 shows the LATE estimate obtained over a varying range of bandwidths around the cutoff, both in 1991 and 2011. Deriving our effects on a narrower or broader sample is instructive as it helps assessing whether the baseline estimates incorporate spatial spillovers. It is indeed possible that the positive effects we find reflect displacement of workers and firms from control areas close to the cutoff. If driven by such displacement, coefficient estimates should shrink when using a broader control group farther away from the cutoff. The effect does decline as more and more municipalities are added to the sample outside of the border, but the impact of the policy remains large and overall stable. This suggests that displacement effects, albeit present, are likely of limited magnitude (as already clear from the RD plots in Figure 1.5).

In fact, the strong persistence we observe could hardly originate solely from displacement of economic activity. While spatial spillovers should be expected during the policy years, they should not be large (as control municipalities still had access to EIM subsidies) and are unlikely to persist in the very long run.[21] We confirm these points below, using a control group located far away from treated units – in fact, outside of the EIM area.

Difference-in-discontinuities. Figure 1.6 Panel (a) shows our most robust estimates – the ρ_j coefficients of the Diff-in-Disc design in Equation 1.2. First, we find evidence in favor of parallel trends, as there is no difference in employment density between treated and control municipalities in 1911 and 1927 relative to the difference in 1951 (which, as showed in Figure 1.5, is very close to zero itself). We then observe a steady increase in the coefficient during the policy years, reaching about 30 workers per km^2 at the end of the intervention. The effect continues to rise in the ensuing decades and is close to 50 workers per km^2 in 2011.

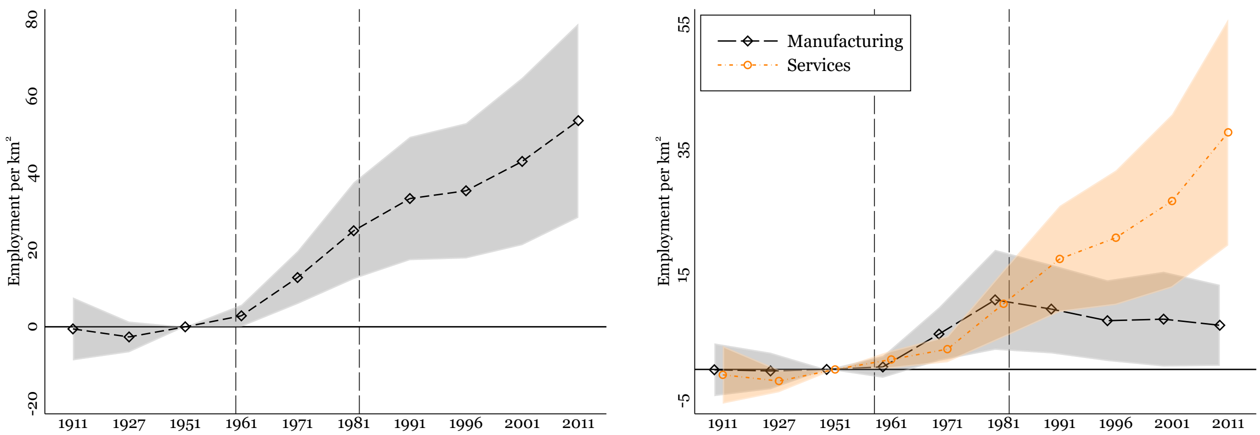
Manufacturing versus services. How does this stark persistence originate? To better inspect our results, we decompose employment density between manufacturing and services and show the corresponding coefficient estimates in Figure 1.6 Panel (b). The rising agglomeration during the policy years is driven in large part by manufacturing employment and, to a smaller extent, services. The manufacturing boost stabilizes towards the end of the policy in the 1980s and moderately declines afterwards. In contrast, the decades after the end of the EIM see a substantial increase in agglomeration in the services sector, which is at the basis of the persistent effect of the policy.[22]

[21] Data available from 1991 onwards show that migration and relocation rates did not differ significantly at the cutoff (Table C1.2.7), though we observe higher resident population in 1991 and 2011 (Table C1.2.8). The (reduced-form) effect on population density hovers around 40 percent, not far from the 50 percent effect on employment density (which reflects the municipality of work). This suggests that our results are not driven by commuting of workers into treated areas. Last, Table C1.2.9 shows that the employment effect of the policy came, at least in part, from increasing aggregate employment in treated areas, as the employment rate and labor market participation rose and the unemployment rate decreased during the 1970s and 1980s.

[22] Figure C1.2.4 reports the Diff-in-Disc results for firm density. Figures C1.2.5-C1.2.8 and Table C1.2.10 show the RD plots and the cross-sectional fuzzy RD estimates separately by manufacturing and services.

Figure 1.6. Difference-in-discontinuities

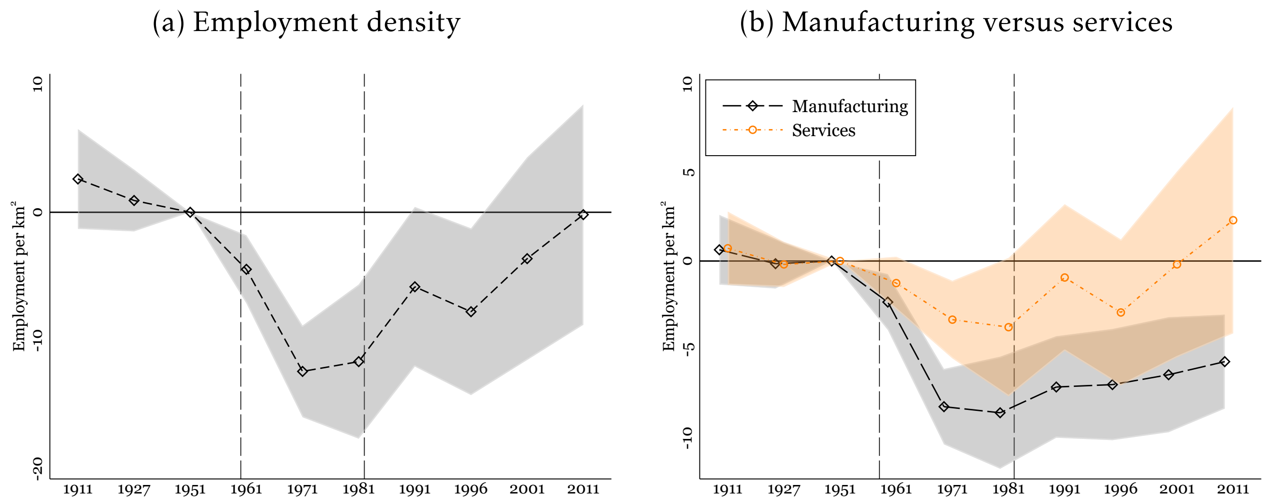
(a) Employment density (b) Manufacturing versus services



Coefficient estimates for Equation 1.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Alternative identification. We now conduct an additional analysis again exploiting the contiguity rule. We consider provincial capitals in the Center-North of Italy, which would have likely been IDA centers had they been part of the EIM region (to ease exposition, we refer to them as "placebo centers"; see Appendix 1.B.3 for details). We leverage this source of variation in three ways. First, we run a simple event study analysis comparing treated municipalities bordering IDA centers with control municipalities bordering placebo centers before and after the institution of the IDAs (Equation B3.1), and plot the coefficients in Figures C1.2.9 and C1.2.10. The two groups are on parallel trends before the policy. Once the IDAs are introduced, economic density increases in the treated areas and the long-term effect is largely concentrated in services, in line with the main results. While these coefficients cannot be directly compared to the baseline RD estimates, the choice of a new control group away from the IDAs is useful for two main reasons. First, it makes spatial spillovers to control units unlikely. Second, it does not suffer from concerns that control municipalities are not part of IDAs because of unobserved reasons.

Figure 1.7. Estimating the spatial spillovers of the IDA program



Coefficient estimates for Equation B3.1. Sample restricted to municipalities up to 16 km outside of the minimum IDA border (treatment group) and municipalities up to 16 km outside of the placebo border traced by municipalities bordering placebo centers (control group). The treatment group excludes IDA municipalities. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Estimating spatial spillovers. This design allows us to go one step further and directly estimate spatial spillovers. In a second exercise, we run the same event study as above but use municipalities up to 16 km outside of the minimum IDA border (the control group in the baseline RD design) as treatment group. As new control group, we consider their counterpart: municipalities up to 16 km outside of the "placebo" boundary traced by municipalities bordering placebo centers. This set-up enables us to investigate possible displacement effects to areas right outside of the minimum IDA border. Figure 1.7 shows the results. We document a negative effect on employment density while IDAs were in place, suggesting some displacement as a result of the policy. During the 1970s, these spillovers reached about 10 workers per km^2, vis-à-vis an estimated RD effect of 30 workers per km^2 in 1981 (Figure 1.6). According to these estimates, roughly one third of the effect of IDAs while they were in place reflects a shift of economic activity around the cutoff. These displacement effects are largely concentrated in manufacturing, and are instead barely noticeable in the non-targeted services sector. Most importantly, they tend to disappear in the long term. In 2011, we observe no spillover of the

IDA policy to nearby areas. The persistent effect of PBIP is therefore not driven by continued displacement of economic activity.[23]

Triple differences. Last, we pool these groups of municipalities together and estimate a triple differences specification (Equation B3.2). Essentially, we compare the double difference between municipalities within and outside of the minimum IDA border to a placebo double difference between municipalities bordering placebo centers and their neighbors. This approach allows for differential pretrends in the baseline Diff-in-Disc of Equation 1.2. We show the estimates in Appendix Figures C1.2.12 and C1.2.13. Although less precisely estimated, most likely as a result of the more demanding specification, the event study coefficients are very similar to those in the main findings at around 50 workers per km^2 in 2011.

1.6 Mechanisms

Our results indicate stark persistence in the effects of PBIP and highlight clear sectoral patterns. We document an immediate response of manufacturing (the only recipient of subsidies) and, to a lesser extent, services, during the policy years. As the intervention ceases, the effect on manufacturing stabilizes but employment in services continues to grow. How can the rise in services – not the target of the policy – be rationalized?

The increase in services while IDAs were in place is most likely a result of multiplier effects, as the stimulus to local manufacturing boosts demand for local goods and services (Moretti, 2010). This implies that the contemporaneous effects on services employment should occur mostly in non-tradables such as retail and hospitality. The (relative) slow stabilization in manufacturing employment, likely due to the end of subsidies and also reflecting the structural decline of industry starting in the 1980s, implies that multiplier effects cannot fully explain the continued response in services.

[23]The results for firm density are similar, and showed in Figure C1.2.11.

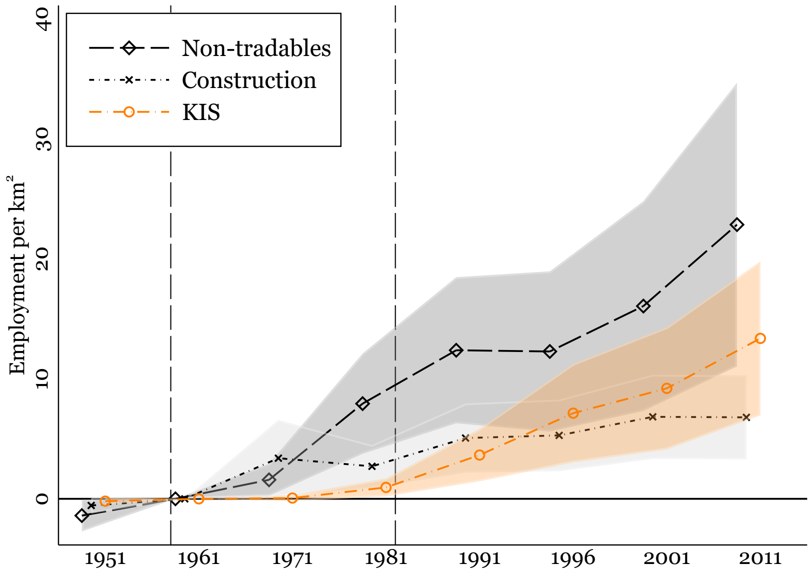
Instead, the enduring growth of the services sector after the end of PBIP is in line with the presence of agglomeration economies and suggests that the targeted locations have undergone a process of structural transformation. For example, IDAs might continue to benefit from knowledge spillovers and a specialized labor pool developed during the policy years, which would be reflected in a larger share of high-skill jobs. Long-term effects on employment in knowledge-intensive services (KIS) such as information technology, finance, or services to firms, would be consistent with these observations.

Non-tradables versus KIS. We now test the above predictions by decomposing the effect on services. As noted, the contemporaneous impact on services employment while IDAs were in place is most likely driven by multiplier effects. A boost to the local tradable sector translates into higher demand for local goods and services, which should raise labor demand in the local non-tradable sector. Performing simple calculations using our estimates, we find that one additional manufacturing job per km^2 is associated with 0.95 more services jobs per km^2 at the peak of the policy in 1981.[24]

As noted above, these pecuniary externalities can account for the contemporaneous rise in services but cannot by themselves explain our persistent effects. Assuming a multiplier of one also after 1981, higher manufacturing employment in treated areas after the end of the policy would account for 50 percent of the increase in services employment in 1991 and 20 percent in 2011.

[24] This number is obtained by dividing the point estimate for services by that for manufacturing in Figure 1.6. It is smaller than the long-term multiplier of 1.6 obtained for the United States in Moretti (2010). The smaller multiplier in our setting might be driven by different labor supply elasticity due, for example, to lower mobility (Moretti and Thulin, 2012).

Figure 1.8. Employment density – Sectoral breakdown



Coefficient estimates for Equation 1.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. "Non-tradables" include wholesale and retail trade, hotels and restaurants and other. KIS include communication, finance and insurance and services to firms. See text for details.

Figure 1.8 shows that, as expected, non-tradables (plus construction) account for most of the increase in services employment during the policy years. With time, however, we document a steady increase in KIS in treated areas.[25] To zoom into these developments we turn to the social security micro data, which are available at a much finer sectoral level and allow us to define KIS following the Eurostat/OECD classification (see Appendix 1.A.3 for details). We replicate the baseline municipality-level fuzzy RD design and show results in Table D1.1, which reports coefficient estimates separately for the shares of KIS and other services in 1991 and 2011. IDA status leads to a 8 percentage points larger share of workers and 6 percentage points larger share of firms in KIS. The effects are economically large and persist well after the end of the policy.

[25]The lack of an effect on KIS while IDAs were in place is not surprising: mean KIS employment density in the estimation sample in the 1960s-70s was still low, at 2-3 workers per km^2. The results for firm density, showed in Appendix Figure D1.1, are similar. We also observe continued agglomeration in non-tradable services. This result could be driven either by contemporaneous local multiplier effects (from either manufacturing or KIS), or by endogenous agglomeration forces in urban amenities (Leonardi and Moretti, 2022). These results are confirmed with the alternative approach using placebo centers – see Appendix Figures D1.2 and D1.3.

The role of high-technology manufacturing. Did the policy have any effect on the *composition* of manufacturing? Can this explain the rise of KIS? We inspect this in Table D1.2, where we distinguish between high- and low-technology manufacturing industries using the Eurostat/OECD classification. At the end of the EIM, treated municipalities had a much larger share of high-technology manufacturing workers and firms compared to control ones. The stimulus to high-technology industries might have contributed to the subsequent development of KIS in two ways. First, by establishing a pool of specialized, high-skilled workers in the local labor market. Second, by providing demand for business services such as consulting, legal and information technology.[26]

Both channels seem to be at play. Figure D1.4 plots the share of cumulative KIS hires (job-to-job) from high-technology manufacturing between 1991 and 2011.[27] In the two decades after the end of IDAs, the share of KIS new hires from high-technology manufacturing rapidly increased in treated municipalities relative to control ones. Examining the second channel is hard without input-output linkages between firms. In Appendix Tables D1.3 and D1.4, we zoom into the sub-sectors (within services) that were most stimulated by the policy and observe a higher incidence of business services such as human resources, computer programming, insurance, consulting, legal and other professional activities in treated municipalities.

Wages, skills and human capital. The higher incidence of KIS jobs in IDAs should be reflected in higher wages and a more skilled workforce. Table 1.4 shows a large positive effect on wages of about 13 percent in 1991, which persists in 2011 at 10 percent. The wage effect is present in both manufacturing and services, and

[26]Larger shares of high-technology manufacturing jobs also imply higher local multipliers, as workers in the local tradable sector command higher earnings and demand more local services (Moretti, 2010).

[27]The majority of KIS hires between 1991 and 2011 are from non-employment (including higher education). The share of KIS hires via job-to-job transitions is 30 percent in treated areas and 25 percent in control ones.

most pronounced in KIS at about 27 percent.[28] The IDA policy also stimulated human capital accumulation and workers' skills in the long term (Table 1.5). The share of high-school educated is 10-11 percentage points larger in 1991 and 2011, and the share of young people with a university degree is 5 and 9 points larger in 1991 and 2011, respectively. We also estimate a large positive effect (10-11 percentage points) on the share of high-skilled occupations (managers and professionals), at the expenses of low-skilled ones (routine jobs).

Firms. Do IDA firms differ from firms in control areas? Table D1.6 shows a prevalence of large and high-paying firms in IDAs in 1991 and 2011. Table D1.7 shows results for balance sheet outcomes in 2011.[29] For manufacturing and KIS firms, we estimate a positive long-run effect on labor productivity, investment and sales. Manufacturing firms also exhibit higher profits per worker. Finally, Figure D1.5 shows year-by-year estimates of the fuzzy RD coefficient when using cumulative firm entry and exit rates (starting in 1990) as outcome. While there are no systematically different patterns in aggregate firm dynamics, we notice interesting heterogeneity. Firm birth and death rates are affected positively in KIS, suggesting high business dynamism. The effect for manufacturing is instead negative, but imprecisely estimated.

[28]Table D1.5 uses AKM worker effects as outcome (Abowd et al., 1999). We estimate a positive and persistent effect of the policy, driven by services and especially KIS workers.

[29]The coverage of the income statements data from Cerved is quite low in the 1990s (less than 20 percent of the universe of firms). We therefore only show the more informative long-term effects.

Table 1.4. (Log) wages – Fuzzy RD estimates

	Total	By sector		Within services	
		Manufacturing	Services	KIS	Other serv.
		Contemporaneous effect (1991)			
RD Estimate	0.13	0.18	0.13	0.26	0.11
	(0.06)**	(0.10)*	(0.07)*	(0.17)	(0.07)
Mean around the border	7.11	7.09	7.13	7.13	7.12
Standard deviation	0.14	0.23	0.19	0.40	0.18
Observations	582	566	570	450	570
		Persistent effect (2011)			
RD Estimate	0.10	0.12	0.12	0.27	0.11
	(0.04)***	(0.06)**	(0.05)**	(0.13)**	(0.05)**
Mean around the border	7.10	7.09	7.01	7.05	7.00
Standard deviation	0.12	0.19	0.17	0.32	0.18
Observations	586	569	585	490	585

Replication of Table 1.3, Column (2). Outcome computed as the natural logarithm of the average monthly wage paid by the firm, then averaged across firms in a municipality. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Table 1.5. Education and occupations – Fuzzy RD estimates

	High school educ.	Univ. degree	Low-skill	High-skill
	Contemporaneous effect (1991)			
RD Estimate	11.04	5.42	-9.26	11.08
	(3.75)***	(2.20)**	(3.40)**	(4.27)**
Mean around the border	15.12	5.60	15.23	17.86
Standard deviation	5.60	3.57	7.81	6.93
Observations	587	587	587	587
	Persistent effect (2011)			
RD Estimate	10.58	9.02	-11.36	9.84
	(3.63)***	(3.10)***	(3.02)***	(3.39)***
Mean around the border	35.22	18.56	21.95	25.02
Standard deviation	6.93	5.90	8.10	6.51
Observations	587	587	587	587

Replication of Table 1.3, Column (2). “High school educ.” is the share of people aged at least 6 with high school education or more. "Univ. degree" is the share of the resident population aged 30-34 years old with a university degree. "Low-skill" denotes the employment share of those in low-skill jobs (unskilled occupations – Isco08 code 8). "High-skill" denotes the employment share of those in high-skill jobs (Legislators, Entrepreneurs, High Executives, Scientific and Highly Specialized Intellectual Professions, Technical Professions – Isco08 codes 1, 2 and 3). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Agglomeration economies. Precisely identifying the market failure tackled by government policy is challenging, as market failures are rarely observed directly. Our evidence suggests that the IDA policy has addressed agglomeration economies in the targeted areas. We present additional findings consistent with the presence of agglomeration economies in Tables D1.8 and D1.9. First, we document sizable long-term effects on local incomes and house prices.[30] Second, sectoral specialization within manufacturing measured with the Krugman Specialization Index (Krugman, 1992) has *decreased* following the policy, suggesting that subsidies did not benefit targeted industries exclusively. Third, we rule out an alternative channel of persistence related to continued public investment in the treated areas after the end of the policy. We test this hypothesis by estimating our fuzzy RD model for the (log of) municipal expenditures sourced from municipal balance sheets between 2000 and 2010, broken down into different items. We add two more outcomes: the cumulative EU structural funds received between 2007 and 2013 and the total subsidies within Law n. 488/1992, which was introduced right at the end of the EIM. We find no meaningful discontinuity in any of these variables, which points to agglomeration economies as the main source of persistence (Garin and Rothbaum, 2022; von Ehrlich and Seidel, 2018).

1.7 Cost-benefit analysis

While our findings clearly highlight a positive impact of the policy, whether these benefits outweigh the very high costs remains to be addressed. We now use our reduced-form estimates to inform a cost-benefit analysis of the IDA program and assess its cost-effectiveness in the long run. Appendix 1.E provides more detail.

[30] As in Lang et al. (2022), we also find that PBIP has not promoted equality, as evidenced by the higher Gini coefficient. In fact, the policy does not seem to have improved equality not only within municipalities but also between them. Figure D1.6 reports quantile treatment effects estimated following Frandsen et al. (2012) and shows higher effects on employment and firm density at higher deciles of the distribution.

Cost per job. We begin by calculating the cost per job. While relatively straightforward, this measure provides an easy way to compare policies with each other. We first use the empirical estimates of Table 1.3, Column (3), suggesting that an increase in EIM funding of €1000 per 1951 resident leads to 10.3 more workers per km^2 in 2011. For the average municipality in the estimation sample, these estimates translate in a cost per job of €17,989 or $25,048 (2011 prices), which rises to $37,571 assuming a deadweight loss of taxation of 50 percent.[31] Using the long-run Diff-in-Disc estimate delivers a very similar cost per job of $21,716 ($32,575 including deadweight loss), which remains roughly stable when substituting the estimates from our alternative identification strategies (Equations B3.1 and B3.2). The cost per job of the IDA policy falls in the range of estimates of similar programs in the US (Busso et al., 2013a), Germany (Siegloch et al., 2022), Japan (Lapoint and Sakabe, 2022) and the UK (Criscuolo et al., 2019).[32]

Cost-benefit analysis. We then move beyond cost-per-job estimates and conduct a back-of-the-envelope analysis of the cost-effectiveness of the IDA policy. Our approach builds on the methods proposed in Busso et al. (2013a) and applied in Chaurey (2017), Lu et al. (2019) and Lapoint and Sakabe (2022). In contrast to these studies, our extended time horizon allows us to evaluate the benefits of the program long after its termination, and compare them with the total costs.

The gains of the IDA policy accrue to workers, firms and landlords in the form of higher wages, profits and rents, respectively. To compute the benefits of the policy, we proceed in five steps: *i*) for each of the outcomes of interest (wage bill, firm profits and housing rents), we calculate the observed amount each year from 1991 to 2011; *ii*) we estimate the impact of the policy on (the log of) each out-

[31] For a similar analysis see Freedman (2012). The magnitude of the deadweight loss largely depends on the effect of place-based policy on location decisions (Busso et al., 2013a). While we estimate no migration effects in the long-run, we cannot rule out that the IDAs induced immigration while they were in place as we find significant differences in current population. We therefore impose a 50 percent deadweight loss as in Criscuolo et al. (2019) and Siegloch et al. (2022).

[32] Our cost per job estimate is smaller than those in Cerqua and Pellegrini (2014) and Cingano et al. (2022) for the investment subsidy program introduced in Italy right after the EIM (Law n.488/1992).

come j over the 1991-2011 period, $\hat{\pi}_j$; iii) we use these estimates to compute the counterfactual amount that would have obtained in the absence of the policy: $counterfactual_j = observed_j/(1+\hat{\pi}_j)$; iv) for each year and outcome, we obtain the net benefit as the difference between the observed and the counterfactual flow; v) we aggregate these yearly amounts between 1991 and 2011 and apply a 10 percent discount rate (roughly the one-year interest rate in Italy in the early 1990s) to derive their present discounted value.

We find that IDAs generated a gain of €86 billion in the two decades after 1991, with most of the benefits accruing to workers (€52 billion) and firms (€33 billion).[33] Total IDA expenses can be directly computed in the ASET data and amount to €88 billion. The gains generated by the IDAs after their termination thus roughly cover the full cost of the program. In turn, this suggests net positive effects assuming that the policy generated surplus also while it was in place or after 2011.

1.8 Discussion and further implications

What features of the IDAs made them a successful example of PBIP? How can these interventions not only stimulate the targeted industries, but also foster long-run development?

Heterogeneity. We first explore possible heterogeneity of the effects across IDAs, asking whether persistence is linked to specific characteristics of an area. We split the group of 12 IDA regions in our sample into two sub-groups based on whether each IDA region is above or below the median of the following six variables: mean elevation, slope, cumulative EIM subsidies per capita, services share in 1951, share of high-technology manufacturing in 1991 and high-school education in 1951. We then conduct analysis separately for IDAs above and below the median. Figure

[33]Landlords capture only a small portion of the gains in the form of housing rents. We show in Appendix 1.E that further €10 billion add to the landlords' surplus coming from the long-run increase in housing value.

F1.1 shows the resulting Diff-in-Disc coefficients.

We measure no significant difference in employment effects between IDAs based on their geographical traits or funding within the EIM. A larger share of services at the onset of the policy seems to lead to higher long-run effects, but the difference between the estimated coefficients is small. The most striking differential effects are found when splitting the sample of IDAs based on the incidence of high-technology manufacturing in 1991 (clearly an outcome of the policy) and education levels in 1951. IDAs where the policy stimulated high-technology industries more, and IDAs with larger initial human capital endowment, are also those where the policy had a larger employment impact in the long term.[34] Still, some persistence in the effect of the IDA policy remains visible across all these heterogeneity cuts. Admittedly, our set-up is not very well suited to heterogeneity analysis because of the relatively small sample size and the RD design. To investigate the sources of persistence further, we outline next the results of our analysis in other areas of the South, which also received EIM subsidies.

The EIM border. As summarized in Appendix 1.B.4 and detailed in Albanese et al. (2023), the northern boundary separating the EIM area from the rest of Italy gives rise to a spatial RD design that compares municipalities south of the border, which were subsidized by the Cassa, to municipalities north of it. In the interest of brevity, we show in Figure 1.9 the most robust estimates from a Diff-in-Disc design run at the EIM border (Equation B4.2).[35] Areas north and south of the border were on parallel trends before the beginning of the policy. A positive effect emerged starting in the 1970s, albeit not statistically significant. The coefficient peaked at the end of the EIM in 1991 but eventually declined, suggesting lack of

[34]The results on human capital resonate with Gagliardi et al. (2023), who find that the effects of deindustrialization on local employment vary greatly depending on the share of college-educated in the local workforce.

[35]We show raw RD plots at the border in Appendix 1.F, Figures F1.2 to F1.7. The regression function is smooth in the decades before the EIM, supporting the continuity assumption also in this RD design. A positive discontinuity emerges in the 1970s and then more clearly in the 1980s and 1990s. As the policy ended, however, the jump at the cutoff becomes barely noticeable. We report cross-sectional RD regression estimates for 1991 and 2011 in Appendix Tables F1.1 and F1.2.

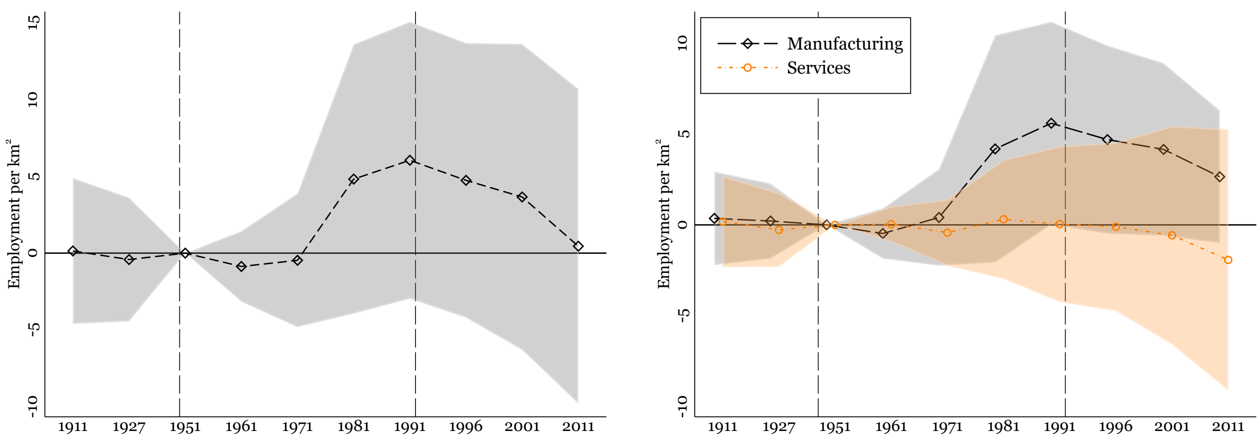
persistence in the impact of the intervention at the EIM border.

Panel (b) breaks down the effect on employment density into manufacturing and services. Similarly to what was found for IDAs, manufacturing employment rose during the policy years but stabilized as the incentives terminated. However, services did not respond to government subsidies, thus not contributing to long-run agglomeration as instead observed in the case of IDAs. We also observe no effect on firm density (Figure F1.8).

Figure 1.9. The EIM border - Difference-in-discontinuities

(a) Employment density

(b) Manufacturing versus services



Coefficient estimates for Equation B4.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the EIM. See text for details.

The results listed in the previous sections tend not to hold at the EIM border (Appendix 1.F). There is no differential incidence of KIS workers and firms south of the border, nor any effect on the share of high-technology manufacturing.[36] Wages are significantly higher south of the border in 1991, but exclusively for manufacturing and other services. By 2011, the wage effect has disappeared. We find no discontinuities in human capital, and even a small negative effect on the share of high-skill occupations. There is a higher share of large firms south of the border, but not of high-paying firms. Firm value added, sales and profits are

[36]EIM firm subsidies at the border went disproportionately towards low-technology industries such as textiles and food (Figure F1.10), as opposed to more advanced industries in the case of IDAs (Figure A1.1.1).

positively affected, but exclusively for manufacturing and other services and not in KIS (as observed for the IDAs). Last, we find no effects on local incomes and even negative long-run effects on house prices.

The IDAs vs the EIM border. While government intervention brought enduring agglomeration and structural transformations in the IDAs, its effects at the EIM border were concentrated in manufacturing and dissipated in the long run.[37] Contrasting these two experiences can be instructive. Table F1.10 compares municipalities bordering IDA centers to municipalities up to 50 km south of the EIM border. The two groups do not differ much in the amount of funding from the Cassa. There are however substantial differences in pre-existing agglomeration of workers and firms, which was about three times as large for IDAs. Places south of the border had instead less favorable geography, a larger share of people employed in agriculture and slightly less educated population before the policy. Put differently, the IDAs were explicitly selected as hubs where agglomeration forces could be stimulated; the EIM border was instead located in peripheral areas of Central Italy – an environment less suitable to the formation of local clusters. This evidence, albeit suggestive, points to the fact that PBIP can have persistent effects when it targets areas with better initial conditions, while its effects are more likely to be short-lived (and limited to the targeted industries) in peripheral regions.[38]

1.9 Conclusion

The shift away from manufacturing employment experienced by most industrialized countries has come at the cost of substantial increases in regional inequality. As place-based industrial policies (PBIPs) aimed at assisting "left-behind" indus-

[37]These considerations relate to the external validity of our results, which we discuss more systematically in Appendix 1.G using the insights of Angrist and Rokkanen (2015) and Bertanha and Imbens (2020).

[38]While we stress the role of initial conditions, another explanation for these findings lies in the role of expectations. In models with multiple steady states, agents' expectations that a community will be in a developed equilibrium can become self-fulfilling (Kline, 2010). The policymaker committed to establishing local hubs in IDAs, while there was no such explicit commitment for the areas around the border.

trial districts grow in popularity, several questions arise about their effectiveness in fostering long-run development in the subsidized areas. Can policies targeting the formation of industrial clusters successfully promote structural change? What role do they play in the transition of clusters out of industry and into knowledge-based local economies?

We tackle these questions by analyzing a PBIP conducted in Italy during the 1960s and the 1970s. Our findings illustrate that PBIPs can indeed generate virtuous cycles in the targeted communities, by promoting agglomeration of workers and firms that persists well after the end of the intervention. We show that the success of PBIPs is intertwined with the response of the services sector, as the initial boost to manufacturing stabilizes when government incentives are phased out. In particular, the development of services jobs with high knowledge content suggests that PBIP expedited structural change and technological adaptation. We stress that the policy-induced promotion of high-technology manufacturing has played a fundamental role in this process, through both increased demand of business-oriented services and the establishment of a high-human capital local labor force that persisted in the long run.

As advocated in Rodrik and Stantcheva (2021) and Rodrik (2022), the success of industrial policy hinges on the creation of "good jobs" and "good jobs externalities". While our analysis of an historical program resonates with these views, we also illustrate how initial conditions matter, as the stimulus to high-skill services jobs appears more likely in places with higher agglomeration potential. We observe instead a short-lived effect, limited to the initial boost to manufacturing, in peripheral areas. Taken together, our evidence has relevant implications for the future of industrial policy, but also warrants further investigation and provides ground for future research.

1.A Appendix A

1.A.1 Appendix A1: The EIM subsidies

As described in Section 1.2, the two main policy items managed by the Cassa were infrastructure spending and firm investment grants (starting in the 1960s).

Infrastructure spending. The Cassa was in charge of planning, execution and monitoring of initiatives in four domains (agriculture, drains and aqueducts, transport and tourism development) subject to the government's allocation of the overall endowment across them. Project proposals were transmitted by local bodies to the Cassa for investigation and approval. Upon approval, the Cassa launched a public tender to procure the execution of the infrastructure. Often, both the formulation and execution of the initiatives were performed directly by the Cassa.

Firm grants. Grant applications were submitted by firms to special credit institutions, which were in charge of investigating the merit and feasibility of the proposed investment including, importantly, the projected increase in employment. The results of the investigation were then forwarded to the Cassa, which decided on the application outcome and the amount of the subsidy. The maximum subsidy rate, originally set at 20 percent of the investment, has been periodically increased and reached up to 45 percent by 1971. Firms could apply for concessional loans, too. The sum of grants and loans conceded by the Cassa to a single firm could not exceed 85 percent of the total investment by the firm.

The ASET data. The ASET archives record detailed information on the universe of transfers by the Cassa, separately by type of intervention: 76,445 infrastructure projects (49,579 public works and 26,866 agricultural improvements), 112,622 investment subsidies and 62,902 concessional loans to firms. Each dataset reports the (current euro) amount, date and location of the intervention. We drop interventions for which information on date, amount or location is missing, along with those with negative amount or for which the date lies outside of the EIM lifespan

(1950-1992). We also drop interventions whose location is not a single municipality but a province or a region. The amounts are converted to 2011 prices using the GDP deflator. Table A1.1.1 reports EIM expenses cumulated by decade and split between infrastructure spending and subsidies to firms, both in raw amounts and per 1951 resident.

Figure A1.1.1 shows the breakdown of firm investment subsidies and low-interest loans across sectors. Panel (a) shows that about 30 percent of the total subsidies went to the chemical sector, while between 7 and 15 percent was absorbed by other industries such as metallurgy, food and textile. Within IDAs (Panel (b)), chemicals remain the most subsidized sector at almost 30 percent of total subsidies, followed by other heavy industries such as metals (20 percent) and transportation manufacturing (10 percent). We notice that incentives to firms are almost entirely in the form of grants, while concessional loans are relatively limited. Also, the share of subsidies to services firms is negligible.

Last, Figure A1.1.2 plots the spatial distribution of EIM expenses across the roughly 3,000 municipalities in the EIM area, separately by expenditure item. The EIM jurisdiction included ten regions: Abruzzo, Basilicata, Calabria, Campania, Lazio, Marche, Molise, Apulia, Sardinia and Sicily. The territories of all these regions, except for Lazio and Marche, traditionally define the Italian South.[39] While firm subsidies are largely concentrated in the IDAs, infrastructure spending is most pronounced in the internal areas.[40]

[39]The EIM's jurisdiction also included some small islands of Tuscany, which we exclude from the sample.

[40]The 14 IDA centers were Latina, Frosinone, Caserta, Napoli, Salerno, Pescara, Foggia, Bari, Taranto, Brindisi, Palermo, Catania, Siracusa and Cagliari. IDAs do not include the so-called *Industrialization Nuclei* – less extensive areas whereby a small number of firms could take advantage of local raw materials and a specialized workforce.

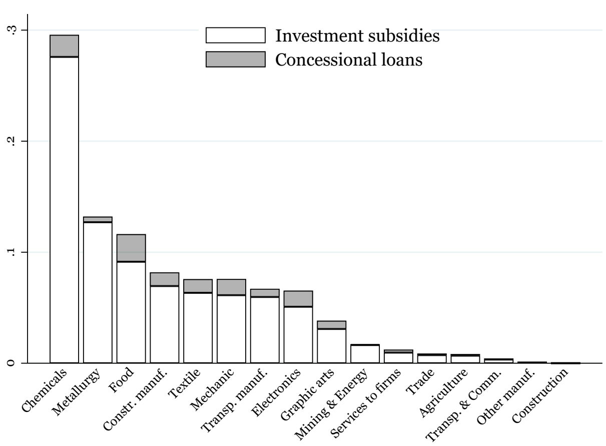
Appendix Table A1.1.1. Cumulative Cassa's expenses per decade

	Total expenses		Infrastructure spending		Firm subsidies	
	Raw amount	Per capita	Raw amount	Per capita	Raw amount	Per capita
1950-1959	5,309	236.4	5,290	235.5	19	0.8
1960-1969	29,990	1,335.2	8,607	383.2	21,382	952.0
1970-1979	79,439	3,536.9	26,368	1,174.0	53,071	2,362.9
1980-1989	37,270	1,659.4	16,781	747.2	20,489	912.3
1990-1992	13,494	600.8	3,635	161.8	9,859	439.0
Total	165,502	7,368.7	60,681	2701.7	104,821	4,667.0

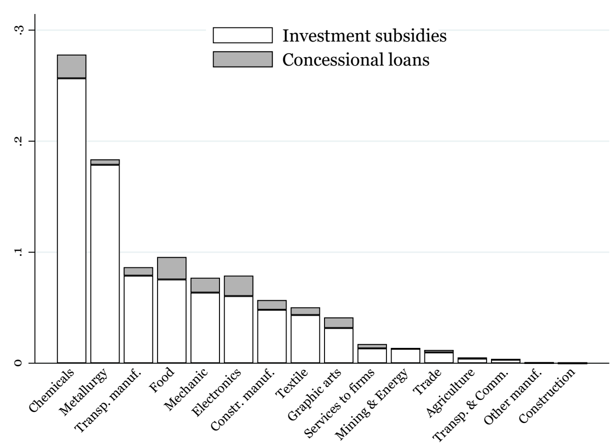
Raw amounts in € million (2011 prices). Per capita amounts in € (2011 prices) per 1951 inhabitant in the Cassa's region. Amounts computed only from geo-coded interventions available in the ASET database.

Appendix Figure A1.1.1. Incentives to firms – breakdown

(a) Full EIM area

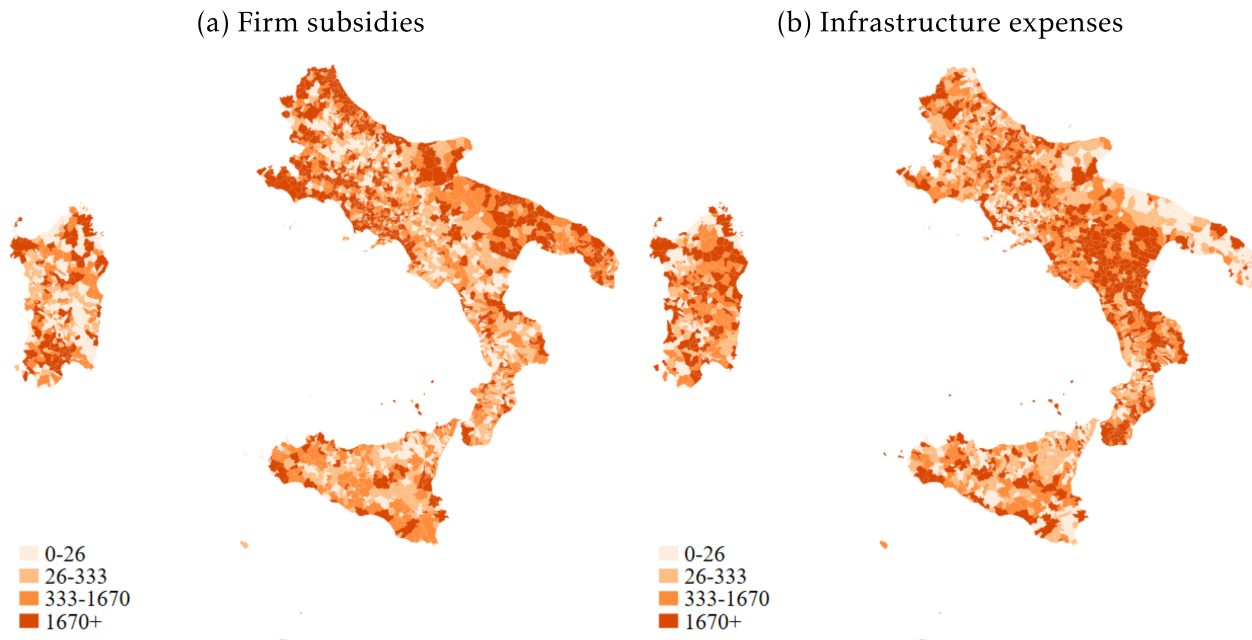


(b) IDAs



Sector breakdown of firm investment subsidies and concessional loans. Panel (a) includes all EIM municipalities. Panel (b) includes IDAs only.

Appendix Figure A1.1.2. Cassa's expenses (1950-1992)



Panel (a) shows firm investment subsidies in € (2011 prices) per 1951 inhabitant, cumulated between 1950 and 1992. Panel (b) shows infrastructure spending in € (2011 prices) per 1951 inhabitant, cumulated between 1950 and 1992.

1.A.2 Appendix A2: Industrial censuses

We collect data on the number of workers and establishments by sector across Italian municipalities from decennial industrial censuses between 1951 and 2011 (including an intermediate census in 1996), sourced from the Istat website. We complement the data by manually digitizing the 1911 and 1927 industrial censuses, available only in pdf format at the Istat historical archives. We match post-World War II censuses with the historical censuses using municipality names. To account for name changes, annexations and mergers between municipalities we rely on a database reporting all administrative changes since Italy's unification in 1861 (`www.elesh.it`). We exclude municipalities reported in the 1911 and/or the 1927 census that are subsequently split into two or more municipalities in the post-War censuses.

Table A1.2.1 shows descriptive statistics for employment and firm density (computed as the number of workers and establishments per km^2) across census years, separately for the EIM area and the rest of Italy. The data also report a broad sector breakdown, which allows to differentiate between manufacturing (food, textile, wood, metallurgy, mechanic, mineral, chemical, rubber, plastic and others), construction, mining, energy and services (wholesale and retail trade, hotels and restaurants, transport, communications, finance and insurance, firm services and other services).[41]

We exploit the within-manufacturing sectoral breakdown to compute a measure of sectoral concentration – the Krugman Specialization Index (KSI) – following Krugman (1992):

$$KSI_{m,t} = \sum \left| \frac{y^s_{m,t}}{y_{m,t}} - \frac{y^s_t}{y_t} \right| \tag{A2.1}$$

Where $y^s_{m,t}$ is the number of manufacturing workers in municipality m, census year t and sector s, $y_{m,t}$ is the total number of manufacturing workers in municipality

[41]The 1927 and 1911 censuses only allow a broad distinction between manufacturing and services. In particular the 1911 data, sourced from the Census of Factories and Industrial Enterprises, only covered firms in manufacturing and "collective needs" services.

m and census year t, y_t^s is the number of manufacturing workers in the reference group in census year t and sector s and y_t is the total number of manufacturing workers in the reference group in census year t. The index provides a simple measure of sectoral specialization in municipality m relative to a reference group, which we set here as all Italian regions except for the more advanced regions of the North (Lombardy, Veneto and Piemonte), as well as smaller regions close to the Alps (Valle d'Aosta, Friuli Venezia Giulia and Trentino Alto Adige) – areas with likely uncomparable industrial structure to that of the EIM regions.

Appendix Table A1.2.1. Industrial census – descriptive statistics

	1911	1927	1951	1961	1971	1981	1991	1996	2001	2011
					Panel (a): Employment density					
EIM area										
Mean	5.70	12.39	13.81	18.18	21.27	31.11	35.35	34.31	40.45	43.91
S.D.	(14.73)	(26.11)	(31.55)	(46.85)	(59.39)	(80.52)	(85.55)	(86.50)	(99.42)	(104.39)
Rest of Italy										
Mean	14.87	25.76	29.00	41.46	54.67	70.23	75.06	76.45	84.90	84.94
S.D.	(29.60)	(47.26)	(60.68)	(84.46)	(104.40)	(125.18)	(130.86)	(133.14)	(145.25)	(142.54)
					Panel (b): Establishment density					
EIM area										
Mean	0.98	5.66	5.84	6.89	7.54	9.52	11.26	12.76	14.46	16.21
S.D.	(1.42)	(8.33)	(8.78)	(11.44)	(13.72)	(18.22)	(21.65)	(26.70)	(30.77)	(34.53)
Rest of Italy										
Mean	1.18	6.51	6.65	8.42	10.68	15.09	16.50	18.05	21.12	22.71
S.D.	(1.39)	(7.29)	(8.46)	(11.85)	(15.67)	(22.10)	(24.57)	(28.59)	(33.72)	(36.41)

Descriptive statistics for worker and firm density separately for the EIM area and the rest of Italy. Variables winsorized at 1 and 99 percent.

1.A.3 Appendix A3: Administrative data

Firm-level data. We collect data on the universe of firms in the Italian private sector from the Social Security archives (INPS) between 1990 and 2015, available at the Bank of Italy. For each firm, the dataset reports the number of employees, the average monthly earnings, the 6-digit sector (classified according to Eurostat's NACE Rev. 2 groups) and the location (municipality). Using firm tax identifiers, we match this dataset with balance sheet information from the Cerved group, available for limited liability corporations since 1995. The Cerved data report detailed income statements and include information on firm sales, value added, profits and investment. We narrow our focus to firms in the non-agricultural private sector and exclude NACE codes 1 to 3, 84 to 88 and 97 to 99, corresponding to agriculture, public sector and families as employers. This selection is standard for the Italian data, as these industries are only partially represented in the social security archives. The detailed sector information allows us to perform further classifications. Specifically, we break down services into knowledge-intensive and other services, and manufacturing into high- and low-technology according to the Eurostat/OECD classification.[42]

Worker-level data. In addition to the firm-level information, we use administrative worker-level data from the INPS archives consisting of the work and pay history between 1990 and 2011 of a random sample of employees, linked with the identifiers of firms where they work. The data cover more than 6.5 percent of the universe of Italian employees in the non-agricultural private sector. For the period of analysis and for each worker-firm match, we observe all the information related to the social security contributions on a yearly basis (earnings, weeks worked, contract type) and some demographic characteristics (gender, year of birth, region of residence). The contract information includes the annual gross earnings, the number of weeks and days worked, whether the schedule is part-time or full-time,

[42]See here: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Glossary:Knowledge-intensive_services_(KIS) and here https://www.oecd.org/sti/ind/48350231.pdf

whether the contract is fixed-term or open-ended (since 1998), and the broad occupation (apprentice, blue-collar, white-collar, middle manager, executive). Through the firm identifiers, we merge the worker- and firm-level administrative data to gather information on the sector of employment and the municipality where the firm is located.

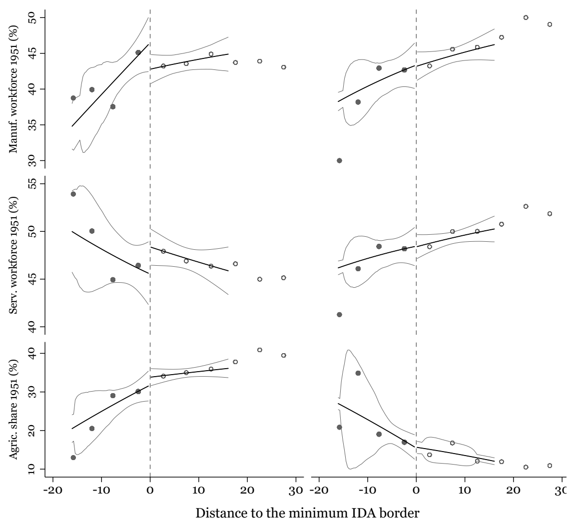
The data record all labor market transitions of workers included in the sample. Therefore, they can be used to compute hiring at the municipality level, as discussed in Section 1.6 and showed for example in Figure D1.4. We define hirings in a given year t as the municipality-level sum of non-employment to employment and firm-to-firm transitions happening between $t-1$ and t. We also exploit the data to compute the AKM worker fixed effects (Abowd et al., 1999). Specifically, for the period 1990-2011, we estimate a two-way fixed effects regression of log weekly earnings on worker and firm fixed effects, controlling for a cubic polynomial in age, a dummy for white-collar workers, a dummy for part-time workers – all interacted with a dummy for female workers – and year dummies. The estimation of the AKM regression requires to restrict the sample to the largest connected group of workers and firms linked by worker mobility. Connected groups contain all workers that have ever been employed by one of the firms in the group, and all firms that have employed one of the workers in the group. We use the full sample between 1990 and 2011 in order to maximize the size of the largest connected group, which comprises around 97 percent of workers in the full sample.

1.B Appendix B

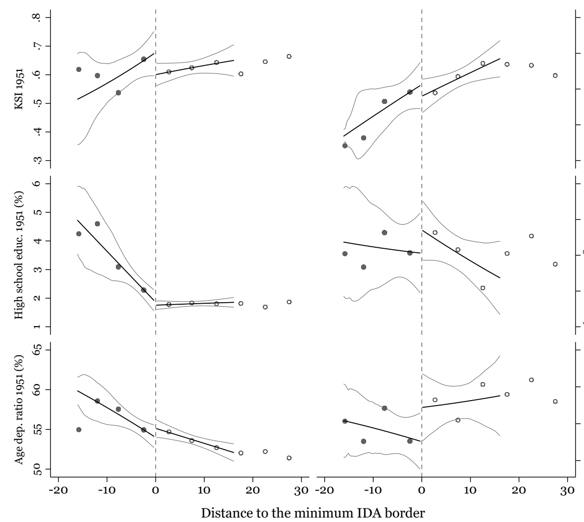
1.B.1 Appendix B1

Appendix Figure B1.1.1. Balancing at the minimum IDA border

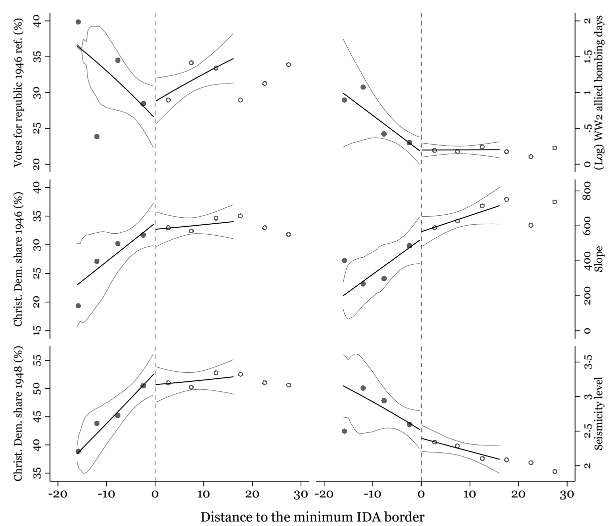
(a)



(b)



(c)



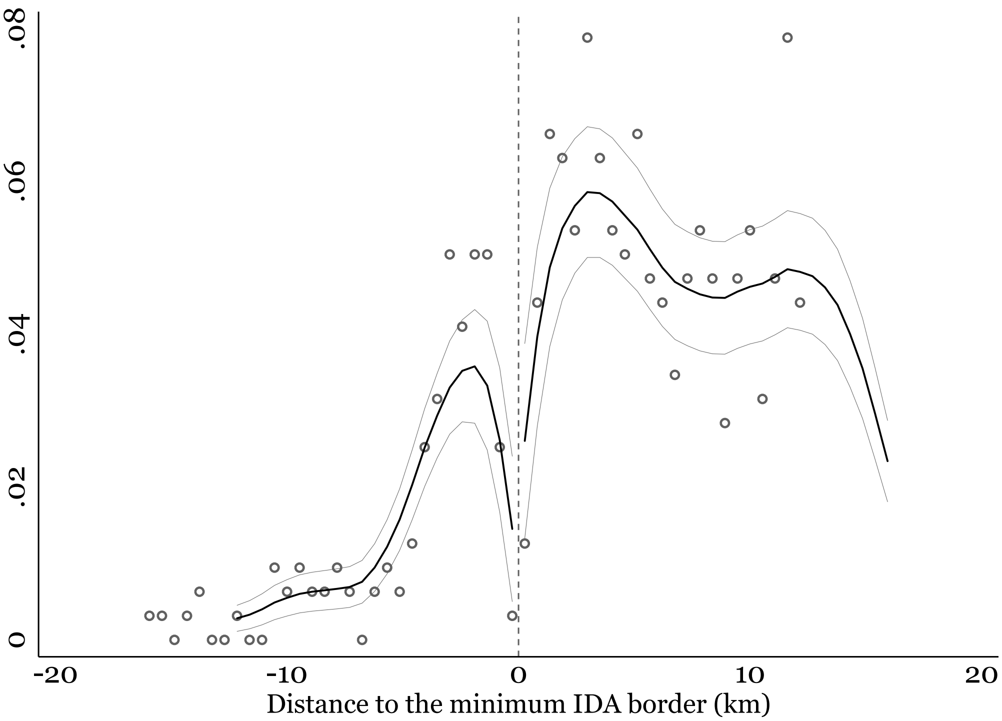
Panel (a): "Manuf. workforce" and "Serv. workforce" are the shares of manufacturing and services workers in the 1951 industrial census. "Agric. share" computed as the number of agriculture workers per 100 residents aged at least 15. "Empl. rate" is the ratio of employed people to total residents aged 15 years and older. "Part. rate" is the ratio of the resident working population to the resident population of the same age group. "Pop. density" measured as number of inhabitants per km^2. Panel (b): "KSI 1951" is the Krugman Specialization Index computed within manufacturing in 1951 (see Appendix 1.A.2). "High school educ." denotes the share of people aged at least 6 with high school education or more. "Age dep. ratio" is the share of those aged below 14 and above 65 to those aged 15-64. "Urban pop." is the share of resident population living in cities. "Gender comp." is the ratio of male to female population. "EIM funding pre IDAs" is total EIM infrastructure spending per capita during the 1950s. Panel (c): "Votes for republic" is the votes share in favor of republic versus monarchy at the 1946 referendum. "Christ. Dem. share" is the votes share for Christian Democrats, showed separately for the 1946 and 1948 election. "WW2 allied bombing days" is the (log) number of days of allied bombing during World War II (Gagliarducci et al., 2020). "Slope" is the difference in meters between the highest and lowest point of the municipality. "Seismicity level" is a categorical variable ranging from 1 "High seismicity" to 4 "Very low seismicity". Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. Appendix Table B1.1.1 shows the corresponding coefficient estimates. See text for details.

Appendix Table B1.1.1. Balancing tests, minimum IDA border

	(1)	(2)	(3)	(4)	(5)	(6)
(a)	Empl.	Manuf. Empl.	Serv. Empl.	Est.	Manuf. Est.	Serv. Est.
RD Estimate	6.50	4.12	2.19	1.49	0.41	0.90
	(3.17)*	(1.40)**	(1.97)	(1.52)	(0.52)	(0.91)
Mean	15.75	7.01	7.24	7.03	2.87	3.95
S.D.	25.09	11.85	12.05	9.23	3.30	5.80
Observations	586	586	586	586	586	586
R^2	0.15	0.16	0.16	0.20	0.20	0.20
(b)	Manuf. work.	Serv. work.	Agric. share	Empl. rate	Part. rate	Pop. dens.
RD Estimate	1.67	-2.16	-3.80	-0.70	-0.53	34.26
	(1.83)	(1.36)	(1.86)*	(1.01)	(1.02)	(80.33)
Mean	43.76	47.01	33.73	50.21	52.10	267.44
S.D.	12.57	11.84	12.97	9.51	9.23	602.66
Observations	563	563	563	563	563	563
R^2	0.20	0.17	0.28	0.42	0.46	0.09
(c)	KSI	High school	Age dep.	Urban pop.	Gender	Pre-IDA exp.
RD Estimate	0.06	0.57	-0.85	2.52	-0.58	-0.06
	(0.05)	(0.23)**	(0.54)	(3.90)	(0.59)	(0.07)
Mean	0.63	1.97	54.05	21.95	98.05	0.24
S.D.	0.26	1.20	5.95	25.05	4.78	0.46
Observations	587	563	563	537	563	563
R^2	0.12	0.17	0.46	0.63	0.25	0.07
(d)	Rep. 1946	CD 1946	CD 1948	Bomb.	Slope	Seism.
RD Estimate	1.03	-0.71	-0.68	0.13	-27.45	-0.03
	(2.14)	(2.67)	(2.49)	(0.13)	(57.73)	(0.04)
Mean	31.26	32.83	50.85	0.24	598.33	2.34
S.D.	17.43	15.09	15.73	0.63	515.50	1.03
Observations	550	545	545	587	587	513
R^2	0.32	0.12	0.18	0.20	0.26	0.85

All outcomes as of 1951, unless noted otherwise. Estimation output of Equation 1.1b using a 16-km symmetric bandwidth around the minimum IDA border. The specification controls for a linear polynomial in the distance from the border and IDA region effects. Standard errors clustered by IDA region in parentheses. See Figure 1.4, Figure B1.1.1 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure B1.1.2. McCrary Test at the minimum IDA border



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

1.B.2 Appendix B2

Proof of Proposition 1. Here we show that, under Assumptions A1, A2 and A3, the fuzzy RD estimand $\beta = \pi/\vartheta$ identifies the average causal effect for compliers at the cutoff (Imbens and Lemieux, 2008; Hahn et al., 2001):

$$\beta = \frac{\lim_{\delta_m \to 0^-} E[Y_m \mid \delta_m] - \lim_{\delta_m \to 0^+} E[Y_m \mid \delta_m]}{\lim_{\delta_m \to 0^-} Pr(IDA_m = 1 \mid \delta_m) - \lim_{\delta_m \to 0^+} Pr(IDA_m = 1 \mid \delta_m)} \tag{B2.1}$$

$$= E[Y_m(1) - Y_m(0) \mid \theta = \theta_C,\ \delta_m = 0]$$

where θ denotes municipality types, so that $\theta = \theta_A$ if $IDA_m(\delta_m) = 1$ (always-takers), $\theta = \theta_N$ if $IDA_m(\delta_m) = 0$ (never-takers) and $\theta = \theta_C$ if $IDA_m(\delta_m) = W_m$ (compliers). Also define $\epsilon > 0$ small enough that $-\epsilon$ and $+\epsilon$ belong to neighborhood $\mathbb{S}$ of the cutoff where there are no defier municipalities, as per Assumption A3.

1) We first focus on the numerator in B2.1. Consider $\delta_m = \epsilon$, so that we are slightly outside of the minimum IDA border:

$$E[Y_m \mid \delta_m = \epsilon] = E[Y_m \mid IDA_m = 1,\ \delta_m = \epsilon] \cdot Pr(IDA_m = 1 \mid \delta_m = \epsilon) +$$
$$+ E[Y_m \mid IDA_m = 0,\ \delta_m = \epsilon] \cdot Pr(IDA_m = 0 \mid \delta_m = \epsilon)$$

And

$$\begin{aligned} Pr(Y_m \le y,\ IDA_m = 1 \mid \delta_m = \epsilon) &= Pr(Y_m(1) \le y,\ IDA_m(\epsilon) = 1 \mid \delta_m = \epsilon) \\ &= Pr(Y_m(1) \le y,\ \theta = \theta_A \mid \delta_m = \epsilon) \\ &= Pr(Y_m(1) \le y \mid \theta = \theta_A,\ \delta_m = \epsilon) \cdot Pr(\theta = \theta_A \mid \delta_m = \epsilon) \end{aligned}$$

where the second equality uses Assumption A3. Similarly,

$$
\begin{aligned}
Pr(Y_m \leq y,\ IDA_m = 0 \mid \delta_m = \epsilon) &= Pr(Y_m(0) \leq y,\ IDA_m(\epsilon) = 0 \mid \delta_m = \epsilon) \\
&= Pr(Y_m(0) \leq y,\ \theta = \theta_N \mid \delta_m = \epsilon) + Pr(Y_m(0) \leq y,\ \theta = \theta_C \mid \delta_m = \epsilon) \\
&= Pr(Y_m(0) \leq y \mid \theta = \theta_N,\ \delta_m = \epsilon) \cdot Pr(\theta = \theta_N \mid \delta_m = \epsilon) + \\
&+ Pr(Y_m(0) \leq y \mid \theta = \theta_C,\ \delta_m = \epsilon) \cdot Pr(\theta = \theta_C \mid \delta_m = \epsilon)
\end{aligned}
$$

Hence:

$$
\begin{aligned}
E[Y_m \mid \delta_m = \epsilon] = {} & E[Y_m(1) \mid \theta = \theta_A,\ \delta_m = \epsilon] \cdot Pr(\theta = \theta_A \mid \delta_m = \epsilon) + \\
& E[Y_m(0) \mid \theta = \theta_N,\ \delta_m = \epsilon] \cdot Pr(\theta = \theta_N \mid \delta_m = \epsilon) + \\
& E[Y_m(0) \mid \theta = \theta_C,\ \delta_m = \epsilon] \cdot Pr(\theta = \theta_C \mid \delta_m = \epsilon)
\end{aligned}
$$

and, using the continuity assumption A2:

$$
\begin{aligned}
\lim_{\epsilon \to 0} E[Y_m \mid \delta_m = \epsilon] = {} & E[Y_m(1) \mid \theta = \theta_A,\ \delta_m = 0] \cdot Pr(\theta = \theta_A \mid \delta_m = 0) + \\
& E[Y_m(0) \mid \theta = \theta_N,\ \delta_m = 0] \cdot Pr(\theta = \theta_N \mid \delta_m = 0) + \\
& E[Y_m(0) \mid \theta = \theta_C,\ \delta_m = 0] \cdot Pr(\theta = \theta_C \mid \delta_m = 0)
\end{aligned} \tag{B2.2}
$$

Consider now $\delta_m = -\epsilon$, so that we are slightly within the minimum IDA border and focus on municipalities contiguous to the IDA center:

$$
\begin{aligned}
E[Y_m \mid \delta_m = -\epsilon] = {} & E[Y_m \mid IDA_m = 1,\ \delta_m = -\epsilon] \cdot Pr(IDA_m = 1 \mid \delta_m = -\epsilon) + \\
& + E[Y_m \mid IDA_m = 0,\ \delta_m = -\epsilon] \cdot Pr(IDA_m = 0 \mid \delta_m = -\epsilon)
\end{aligned}
$$

And

$$
\begin{aligned}
Pr(Y_m \le y,\ IDA_m = 1 \mid \delta_m = -\epsilon) &= Pr(Y_m(1) \le y,\ IDA_m(-\epsilon) = 1 \mid \delta_m = -\epsilon) \\
&= Pr(Y_m(1) \le y,\ \theta = \theta_A \mid \delta_m = -\epsilon) + \\
&+ Pr(Y_m(1) \le y,\ \theta = \theta_C \mid \delta_m = -\epsilon) \\
&= Pr(Y_m(1) \le y \mid \theta = \theta_A,\ \delta_m = -\epsilon) \cdot Pr(\theta = \theta_A \mid \delta_m = -\epsilon) + \\
&+ Pr(Y_m(1) \le y \mid \theta = \theta_C,\ \delta_m = -\epsilon) \cdot Pr(\theta = \theta_C \mid \delta_m = -\epsilon)
\end{aligned}
$$

Similarly,

$$
\begin{aligned}
Pr(Y_m \le y,\ IDA_m = 0 \mid \delta_m = -\epsilon) &= Pr(Y_m(0) \le y,\ IDA_m(-\epsilon) = 0 \mid \delta_m = -\epsilon) \\
&= Pr(Y_m(0) \le y,\ \theta = \theta_N \mid \delta_m = -\epsilon) \\
&= Pr(Y_m(0) \le y \mid \theta = \theta_N,\ \delta_m = -\epsilon) \cdot Pr(\theta = \theta_N \mid \delta_m = -\epsilon)
\end{aligned}
$$

Where the second equality again uses Assumption A3. Then:

$$
\begin{aligned}
E[Y_m \mid \delta_m = -\epsilon] = \; & E[Y_m(1) \mid \theta = \theta_A,\ \delta_m = -\epsilon] \cdot Pr(\theta = \theta_A \mid \delta_m = -\epsilon) + \\
& E[Y_m(1) \mid \theta = \theta_C,\ \delta_m = -\epsilon] \cdot Pr(\theta = \theta_C \mid \delta_m = -\epsilon) + \\
& E[Y_m(0) \mid \theta = \theta_N,\ \delta_m = -\epsilon] \cdot Pr(\theta = \theta_N \mid \delta_m = -\epsilon)
\end{aligned}
$$

Taking the limit and using the continuity assumption A2:

$$
\begin{aligned}
\lim_{\epsilon \to 0} E[Y_m \mid \delta_m = -\epsilon] = \; & E[Y_m(1) \mid \theta = \theta_A,\ \delta_m = 0] \cdot Pr(\theta = \theta_A \mid \delta_m = 0) + \\
& E[Y_m(1) \mid \theta = \theta_C,\ \delta_m = 0] \cdot Pr(\theta = \theta_C \mid \delta_m = 0) + \\
& E[Y_m(0) \mid \theta = \theta_N,\ \delta_m = 0] \cdot Pr(\theta = \theta_N \mid \delta_m = 0)
\end{aligned} \tag{B2.3}
$$

Subtracting Equation B2.2 from B2.3:

$$\lim_{\epsilon \to 0} E[Y_m \mid \delta_m = -\epsilon] - \lim_{\epsilon \to 0} E[Y_m \mid \delta_m = \epsilon] = E[Y_m(1) - Y_m(0) \mid \theta = \theta_C, \delta_m = 0] \cdot Pr(\theta = \theta_C \mid \delta_m = 0)$$

2) We now focus on the denominator in B2.1. For $\delta_m = \epsilon$, and using A3:

$$Pr(IDA_m = 1 \mid \delta_m = \epsilon) = Pr(\theta = \theta_A \mid \delta_m = \epsilon)$$

Taking the limit and using A2:

$$\lim_{\epsilon \to 0} Pr(IDA_m = 1 \mid \delta_m = \epsilon) = Pr(\theta = \theta_A \mid \delta_m = 0) \tag{B2.4}$$

Similarly for $\delta_m = -\epsilon$:

$$Pr(IDA_m = 1 \mid \delta_m = -\epsilon) = Pr(\theta = \theta_A \mid \delta_m = -\epsilon) + Pr(\theta = \theta_C \mid \delta_m = -\epsilon)$$

And:

$$\lim_{\epsilon \to 0} Pr(IDA_m = 1 \mid \delta_m = -\epsilon) = Pr(\theta = \theta_A \mid \delta_m = 0) + Pr(\theta = \theta_C \mid \delta_m = 0) \tag{B2.5}$$

Subtracting B2.4 from B2.5:

$$\lim_{\epsilon \to 0} Pr(IDA_m = 1 \mid \delta_m = -\epsilon) - Pr(IDA_m = 1 \mid \delta_m = \epsilon) = Pr(\theta = \theta_C \mid \delta_m = 0)$$

Taking things together:

$$\beta = \frac{\lim_{\delta_m \to 0^-} E[Y_m \mid \delta_m] - \lim_{\delta_m \to 0^+} E[Y_m \mid \delta_m]}{\lim_{\delta_m \to 0^-} Pr(IDA_m = 1 \mid \delta_m) - \lim_{\delta_m \to 0^+} Pr(IDA_m = 1 \mid \delta_m)}$$

$$= \frac{E[Y_m(1) \; - \; Y_m(0) \mid \theta = \theta_C, \; \delta_m = 0] \; \cdot \; Pr(\theta = \theta_C \mid \delta_m = 0)}{Pr(\theta = \theta_C \mid \delta_m = 0)}$$

$$= E[Y_m(1) \; - \; Y_m(0) \mid \theta = \theta_C, \; \delta_m = 0]$$

Which proves the result.

(Fuzzy) Difference in discontinuities. We now discuss identification for the Diff-in-Disc design introduced at the end of Section 1.4, drawing on the analysis in Grembi et al. (2016) and Millán-Quijano (2020). Let the indicator $P = \mathbb{1}[year \geq 1960]$ denote the census years after the introduction of the IDAs. Also introduce two treatments W_m^p and IDA_m^p where the superscript $p \in \{0,1\}$ denotes the period. In particular:

$$W_m^p = \begin{cases} \text{if } \delta_m > 0 : \; 0 \quad \forall p \\ \text{if } \delta_m \leq 0 : \; 1 \quad \forall p \end{cases}$$

$$IDA_m^p = \begin{cases} \text{if } p = 0 : \; 0 \\ \text{if } p = 1 : \; \lim_{\delta_m \to 0^+} Pr(IDA_m = 1 \mid \delta_m) < \lim_{\delta_m \to 0^-} Pr(IDA_m = 1 \mid \delta_m) \end{cases}$$

In words, W_m^p denotes whether a municipality borders a provincial capital and depends solely on the running variable δ_m and not on the time period. IDA_m^p denotes IDA status and is equal to zero for all municipalities at $p = 0$. After the introduction of the policy, imperfect compliance is such that IDA status jumps discontinuously (but not sharply) at the cutoff (Assumption A3). Define potential outcomes $Y_m^p(i, w)$ with $IDA_m^p = i \in \{0,1\}$ and $W_m^p = w \in \{0,1\}$, such that the observed outcome

$Y_m^p = Y_m^p(1,1) \cdot IDA_m^p \cdot W_m^p + Y_m^p(1,0) \cdot IDA_m^p \cdot (1 - W_m^p) + Y_m^p(0,1) \cdot (1 - IDA_m^p) \cdot W_m^p + Y_m^p(0,0) \cdot (1 - IDA_m^p) \cdot (1 - W_m^p)$.

The Diff-in-Disc set-up is more robust than the cross-sectional fuzzy RD design in that it allows bordering a large city (the IDA center) to affect the outcome independently of IDA status (the treatment of interest). To show this, we first posit a new continuity assumption (instead of A2 in the main text) implying that, once accounting for IDA treatment and for contiguity to an IDA center, no other relevant factors jump at the minimum IDA border.

A2b. Continuity. *Mean potential outcomes* $E[Y_m^p(i,w) \mid \delta_m]$ *are continuous at* $\delta_m = 0$ *for* $p = 0,1$, $i = 0,1$ *and* $w = 0,1$.

With derivations similar to those above, and using Assumption A2b, one can show that the numerator in Equation B2.1 at time $p = 1$ (when the IDAs are in place) is now:

$$\begin{aligned}\lim_{\delta_m \to 0^-} E[Y_m^1 \mid \delta_m] - \lim_{\delta_m \to 0^+} E[Y_m^1 \mid \delta_m] = {} & E[Y_m^1(1,1) - Y_m^1(0,0) \mid \theta = \theta_C,\ \delta_m = 0] \cdot Pr(\theta = \theta_C \mid \delta_m = 0) + \\ & E[Y_m^1(1,1) - Y_m^1(1,0) \mid \theta = \theta_A,\ \delta_m = 0] \cdot Pr(\theta = \theta_A \mid \delta_m = 0) + \\ & E[Y_m^1(0,1) - Y_m^1(0,0) \mid \theta = \theta_N,\ \delta_m = 0] \cdot Pr(\theta = \theta_N \mid \delta_m = 0)\end{aligned}$$

The cross-sectional reduced-form estimator identifies not only the treatment effect of interest (that of IDA status, on the first row), but also that of simply being contiguous to an IDA center. The contiguity effect is expressed as a weighted average of the effect for IDA always-takers and never-takers, on the second and third row above. To correctly identify the impact of IDA status, the confounding effect due to contiguity to IDA centers has to be cancelled out. To do so, one can exploit the discontinuity at $p = 0$ when IDAs had not yet been introduced, implying that any difference in outcomes at $p = 0$ derives from the contiguity treatment. Let us assume:

A4. Parallel trends. *The effect of contiguity at* $\delta_m = 0$ *does not change over time:* $Y_m^1(\cdot,1) - Y_m^1(\cdot,0) = Y_m^0(\cdot,1) - Y_m^0(\cdot,0)$.

Assumption A4 imposes that the effect of bordering IDA centers is time-constant and therefore cancels out when taking first differences.[43] In turn, the fuzzy Diff-in-Disc estimand:

$$\rho = \frac{(\lim_{\delta_m \to 0^-} E[Y_m^1 \mid \delta_m] - \lim_{\delta_m \to 0^+} E[Y_m^1 \mid \delta_m]) - (\lim_{\delta_m \to 0^-} E[Y_m^0 \mid \delta_m] - \lim_{\delta_m \to 0^+} E[Y_m^0 \mid \delta_m])}{\lim_{\delta_m \to 0^-} Pr(IDA_m = 1 \mid \delta_m) - \lim_{\delta_m \to 0^+} Pr(IDA_m = 1 \mid \delta_m)}$$

identifies again the LATE for compliers at the cutoff.

[43]The "invariant participation" assumption introduced in Millán-Quijano (2020) is redundant in our case as the probability of bordering the IDA center is constant over time and jumps sharply from zero to one at the cutoff.

1.B.3 Appendix B3: Alternative identification strategy

In this strategy we again exploit the exogenous imposition that municipalities bordering IDA centers be automatically included in IDAs and compare these with municipalities bordering provincial capitals in the Center-North of Italy, which would have likely been IDA centers if they were part of the program's jurisdiction. To ease exposition, we will refer to provincial capitals in the Center-North as "placebo centers". Figure B1.3.1 provides an illustration. Placebo centers are in black and their bordering municipalities are in grey. For comparability purposes, we exclude the most industrialized regions in the North of Italy (Lombardy, Veneto and Piemonte), as well as smaller regions close to the Alps (Valle d'Aosta, Friuli Venezia Giulia and Trentino Alto Adige).

Simple event study. In a first approach, we pool together the 120 municipalities bordering IDA centers (in orange) and the 243 municipalities bordering placebo centers (in grey). We compare these two groups before and after the institution of IDAs in a simple event study design. Let T_m be a treatment indicator denoting municipalities in the EIM area (those bordering IDA centers) and let $P = \mathbb{1}[year \geq 1960]$ be the time indicator defined above. Define again potential outcomes $Y_m(t)$ with $T_m = t \in \{0,1\}$, so that the observed outcome $Y_m = Y_m(1) \cdot T_m \cdot P + Y_m(0) \cdot (1 - T_m \cdot P)$. The causal effect of interest is $E[Y_m(1) - Y_m(0) \mid T_m = 1,\ P = 1]$. In the standard difference-in-differences (DID) regression:

$$Y_m = \beta_0 + \beta_1 \cdot T_m + \beta_2 \cdot P + \rho \cdot T_m \cdot P + \epsilon_m$$

The DID coefficient ρ identifies:

$$\begin{aligned}
\rho &= (E[Y_m \mid T_m = 1,\ P = 1] - E[Y_m \mid T_m = 1,\ P = 0]) - (E[Y_m \mid T_m = 0,\ P = 1] - E[Y_m \mid T_m = 0,\ P = 0]) \\
&= (E[Y_m(1) \mid T_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 1,\ P = 0]) \\
&- (E[Y_m(0) \mid T_m = 0,\ P = 1] - E[Y_m(0) \mid T_m = 0,\ P = 0]) \\
&= E[Y_m(1) - Y_m(0) \mid T_m = 1,\ P = 1] \\
&+ (E[Y_m(0) \mid T_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 1,\ P = 0]) \\
&- (E[Y_m(0) \mid T_m = 0,\ P = 1] - E[Y_m(0) \mid T_m = 0,\ P = 0])
\end{aligned}$$

Under the standard assumption:

B3.1. Parallel trends 1. *There are common time trends in the control outcome across the two groups defined by* T_m : $E[Y_m(0) \mid T_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 1,\ P = 0] = E[Y_m(0) \mid T_m = 0,\ P = 1] - E[Y_m(0) \mid T_m = 0,\ P = 0]$.

the DID coefficient identifies the causal effect of interest.

In practice, we estimate a dynamic version of the standard DID model that allows to empirically verify the parallel trends assumption:

$$Y_{m,t} = \mu_m + \sigma_t + \sum_{j \neq 1951} \rho_j \cdot \mathbb{1}[t = j] \cdot T_m + \epsilon_{m,t} \tag{B3.1}$$

Where $Y_{m,t}$ is the outcome of interest for municipality m and census year t, μ_m are municipality fixed effects and σ_t are census year effects. The coefficients of interest ρ_j capture the difference in outcomes between municipalities bordering IDA centers and those bordering placebo centers, relative to the difference in 1951. Inspection of the ρ_{1911} and ρ_{1927} coefficients provides a test of the parallel trends assumption.

Testing for displacement. This source of variation can also be exploited to investigate possible spillover effects of the IDA policy to the control group in the baseline identification strategy. Specifically, we use municipalities up to 16 km outside of the "placebo" boundary traced by municipalities bordering placebo centers as a counterfactual for municipalities up to 16 km outside of the minimum IDA border (the control group in the baseline design). We estimate the same specification of Equation B3.1, where again $T_m = 1$ for municipalities in the EIM area.[44]

Triple differences. In a last approach, we estimate an unified model that pools together municipalities (i) bordering IDA centers; ii) bordering placebo centers; and iii) up to 16 km away from the first two groups. The resulting sample comprises 1478 municipalities, 622 of which are in the EIM area. Let W_m be an indicator denoting municipalities bordering either IDA centers or placebo centers (the union of the orange and grey municipalities). Let also T_m be the indicator denoting municipalities in the EIM area, defined above, and $P = \mathbb{1}[year \geq 1960]$. The observed outcome can again be defined as a function of potential outcomes $Y_m = Y_m(1) \cdot T_m \cdot W_m \cdot P + Y_m(0) \cdot (1 - T_m \cdot W_m \cdot P)$. The causal effect of interest is now $E[Y_m(1) - Y_m(0) \mid T_m = 1,\ W_m = 1,\ P = 1]$. The fully saturated model is:

$$Y_m = \beta_0 + \beta_1 \cdot T_m + \beta_2 \cdot W_m + \beta_3 \cdot P + \beta_4 \cdot T_m \cdot W_m + \beta_5 \cdot T_m \cdot P + \beta_6 \cdot W_m \cdot P + \rho \cdot T_m \cdot W_m \cdot P + \epsilon_m$$

[44]To identify spillover effects, the treatment group of this design excludes municipalities outside of the minimum IDA border that were part of the IDA (the always-takers).

The triple DID coefficient ρ now identifies:

$$
\begin{aligned}
\rho = \{&(E[Y_m \mid T_m = 1,\ W_m = 1,\ P = 1] - E[Y_m \mid T_m = 1, W_m = 0,\ P = 1]) \\
&- (E[Y_m \mid T_m = 1,\ W_m = 1,\ P = 0] - E[Y_m \mid T_m = 1, W_m = 0,\ P = 0])\} \\
&- \{(E[Y_m \mid T_m = 0,\ W_m = 1,\ P = 1] - E[Y_m \mid T_m = 0, W_m = 0,\ P = 1]) \\
&- (E[Y_m \mid T_m = 0,\ W_m = 1,\ P = 0] - E[Y_m \mid T_m = 0, W_m = 0,\ P = 0])\} \\
&= \{(E[Y_m(1) \mid T_m = 1,\ W_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 1, W_m = 0,\ P = 1]) \\
&- (E[Y_m(0) \mid T_m = 1,\ W_m = 1,\ P = 0] - E[Y_m(0) \mid T_m = 1, W_m = 0,\ P = 0])\} \\
&- \{(E[Y_m(0) \mid T_m = 0,\ W_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 0, W_m = 0,\ P = 1]) \\
&- (E[Y_m(0) \mid T_m = 0,\ W_m = 1,\ P = 0] - E[Y_m(0) \mid T_m = 0, W_m = 0,\ P = 0])\} \\
&= E[Y_m(1) - Y_m(0) \mid T_m = 1,\ W_m = 1,\ P = 1] \\
&+ \{(E[Y_m(0) \mid T_m = 1, W_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 1, W_m = 0,\ P = 1]) \\
&- (E[Y_m(0) \mid T_m = 1,\ W_m = 1,\ P = 0] - E[Y_m(0) \mid T_m = 1, W_m = 0,\ P = 0])\} \\
&- \{(E[Y_m(0) \mid T_m = 0,\ W_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 0, W_m = 0,\ P = 1]) \\
&- (E[Y_m(0) \mid T_m = 0,\ W_m = 1,\ P = 0] - E[Y_m(0) \mid T_m = 0, W_m = 0,\ P = 0])\}
\end{aligned}
$$

In this case, identification of the effect of interest requires an even weaker assumption than either A4 or B3.1. Namely:

B3.2. Parallel trends II. *Any differential time trends in the control outcome between contiguous and not contiguous municipalities must be the same in the EIM area and in the Center-North:*

$$
\begin{aligned}
&(E[Y_m(0) \mid T_m = 1, W_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 1, W_m = 0,\ P = 1]) \\
&- (E[Y_m(0) \mid T_m = 1,\ W_m = 1,\ P = 0] - E[Y_m(0) \mid T_m = 1, W_m = 0,\ P = 0]) \\
&= (E[Y_m(0) \mid T_m = 0,\ W_m = 1,\ P = 1] - E[Y_m(0) \mid T_m = 0, W_m = 0,\ P = 1]) \\
&- (E[Y_m(0) \mid T_m = 0,\ W_m = 1,\ P = 0] - E[Y_m(0) \mid T_m = 0, W_m = 0,\ P = 0])
\end{aligned}
$$

By allowing for differential pre-trends, this approach imposes less restrictive identifying assumptions than both the Diff-in-Disc design comparing municipalities within and outside of the minimum IDA border, as well as the event study design comparing municipalities bordering IDA centers to municipalities bordering placebo centers. Valid identification requires that any differential time trend in the control outcome is the same across the two groups, so that it would cancel out when taking the triple difference.

We specify the following dynamic triple differences specification:

$$Y_{m,t} = \mu_m + \sum_{j \neq 1951} \gamma_j \cdot \mathbb{1}[t = j] \cdot W_m + \sum_{j \neq 1951} \eta_j \cdot \mathbb{1}[t = j] \cdot T_m + \sum_{j \neq 1951} \rho_j \cdot \mathbb{1}[t = j] \cdot W_m \cdot T_m + \epsilon_{m,t} \tag{B3.2}$$

Where $Y_{m,t}$ is the outcome of interest for municipality m and census year t and μ_m are municipality fixed effects. The coefficients of interest ρ_j capture the difference between two differences in census year j relative to the baseline difference in 1951: the difference in outcomes between municipalities bordering IDA centers and those right outside of the minimum IDA border (the baseline results showed in the paper, see Figure 1.6); and the difference in outcomes between municipalities bordering placebo centers and those farther away. If Assumption B3.2 holds, the event study coefficients before the introduction of IDAs ρ_{1911} and ρ_{1927} should be undistinguishable from zero.

Last, we notice that the triple difference design automatically accounts for the possible spillover effects described above. Re-arranging the expression for the ρ parameter in the fully saturated model:

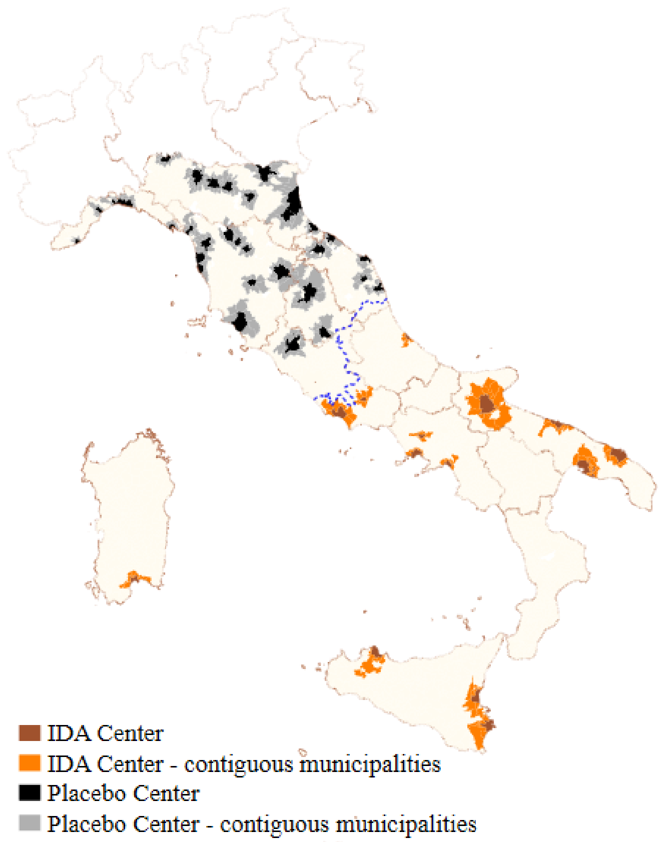
$$\rho = \underbrace{\{(E[Y_m \mid T_m = 1,\ W_m = 1,\ P = 1] - E[Y_m \mid T_m = 1, W_m = 1,\ P = 0]) - (E[Y_m \mid T_m = 0,\ W_m = 1,\ P = 1] - E[Y_m \mid T_m = 0, W_m = 1,\ P = 0])\}}_{\text{"Within" effect}}$$

$$-$$

$$\underbrace{\{(E[Y_m \mid T_m = 1,\ W_m = 0,\ P = 1] - E[Y_m \mid T_m = 1, W_m = 0,\ P = 0]) - (E[Y_m \mid T_m = 0,\ W_m = 0,\ P = 1] - E[Y_m \mid T_m = 0, W_m = 0,\ P = 0])\}}_{\text{"Outside" (spillover) effect}}$$

Where the "within" difference is identified by the event study in B3.1, while the "outside" difference is an estimate of possible spillovers of the IDA policy to nearby control areas.

Appendix Figure B1.3.1. Alternative identification – graphical illustration



The map shows municipalities bordering IDA centers in orange and municipalities bordering placebo centers in gray. Placebo centers are provincial capitals in the Center-North of Italy. The dashed blue line is the EIM border. See text for details.

1.B.4 Appendix B4: The EIM border

We describe briefly the second identification strategy of the paper, which exploits the discontinuity taking place at the northern boundary of the EIM jurisdiction.[45] When the EIM began in 1950, the policymaker had to separate the area of intervention from the rest of Italy, splitting the country in two halves. This border was set above the traditional boundaries of the Southern Italian regions and extended towards Central Italy to include areas of Lazio and Marche (Figure B1.4.1, Panel (a)). The list of the additional municipalities was set in 1950 and the EIM area remained since unchanged until the termination of the policy in 1992. Panel (b) of Figure B1.4.1 plots Cassa's expenses around the border, clearly showing a stark jump equivalent to roughly 15,000 euros per capita.[46]

As described in Albanese et al. (2023), the RD continuity assumption is likely satisfied at the EIM border. A close inspection of the historical parliamentary discussions that led to the drawing of the border reveals that this choice was informed by technical details related to the execution of infrastructure projects, such as land reclamations and river engineering, without much consideration of the economic conditions of those areas. In addition, the border does not systematically coincide with regional boundaries, nor does it matter for other place-based policies realized before, during or after the EIM. Balancing tests in Albanese et al. (2023) reveal no meaningful discontinuity in pre-determined municipality characteristics, lending further credibility to this strategy.

The baseline specification is a sharp RD design (Dell, 2010) that uses distance to the border ι_m as running variable (with negative values denoting control municipalities north of the border) and $B_m = \mathbb{1}[\iota_m \geq 0]$ as treatment indicator:

$$Y_m = \lambda_b + \kappa \cdot B_m + \varphi(\iota_m) + \epsilon_m \tag{B4.1}$$

[45]More details on the EIM border and its suitability as a RD cutoff are available in Albanese et al. (2023).

[46]The slightly positive amounts north of the border denote infrastructure spending in some small islands of Tuscany and grants to firms located in neighborhoods of four municipalities in Lazio.

Where Y_m is the outcome of interest for municipality m, λ_b are border-segment fixed effects denoting the segment of the border closest to municipality m and $\varphi(\iota_m)$ is a linear RD polynomial. The specification is estimated on a baseline bandwidth of 50 km north and south of the EIM border.[47] Standard errors allow for arbitrary correlation across space following Conley (1999). Under the continuity assumption, the RD coefficient κ estimates the causal effect of the treatment at the cutoff (Imbens and Lemieux, 2008). Proving this result is easy when considering in the proof of Appendix 1.B.2 that a sharp RD design is a special case of fuzzy RD with perfect compliance: $\lim_{\iota_m \to 0^-} Pr(B_m = 1 \mid \iota_m) - \lim_{\iota_m \to 0^+} Pr(B_m = 1 \mid \iota_m) = 1$.

To further improve on internal validity, we can again specify a dynamic version of Equation B4.1 in the form of a Diff-in-Disc design:

$$Y_{m,t} = \mu_m + \sigma_t + \sum_{j \neq 1951} \rho_j \cdot \mathbb{1}[t = j] \cdot B_m + \epsilon_{m,t} \qquad \text{(B4.2)}$$

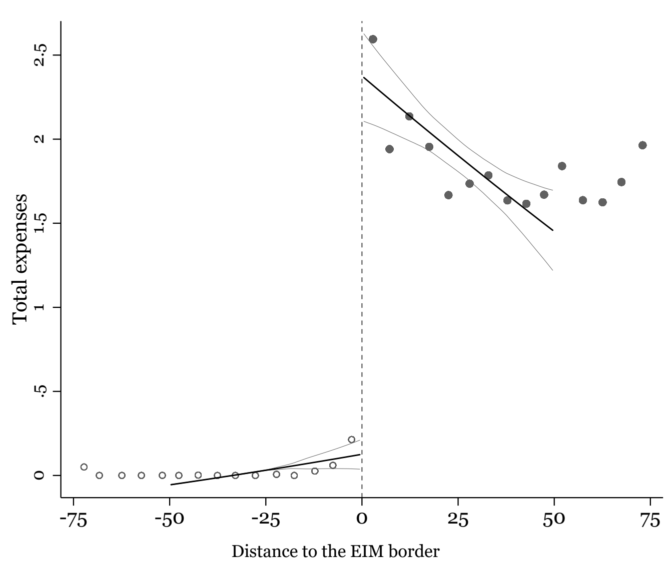
Where notation is the same as in Equation 1.2. The sample uses a 50-km symmetric bandwidth around the border and standard errors are clustered at the municipality level.

[47] We obtain this bandwidth as a simple average of MSE-optimal bandwidths, derived following Calonico et al. (2014a) using employment density across sectors and census years as outcome.

Appendix Figure B1.4.1. The EIM border

(a) The border

(b) Total EIM expenses



Panel (a) shows the EIM border as the dashed blue line. Panel (b) shows (log) total EIM expenses in thousand € (2011 prices) per 1951 resident, cumulated between 1950 and 1992. Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

1.C Appendix C

1.C.1 Appendix C1: Theory

We sketch here a simple spatial equilibrium model drawing on Kline (2010) and Kline and Moretti (2014b). The model describes the direct effect on employment of a place-based policy that changes the relative cost of capital across locations. We consider two cities indexed by $j \in \{A, B\}$ and workers making location decisions.

Workers. There is a continuum of workers (each indexed by i) of measure one. Location decisions between the two cities are free. Each worker inelastically supplies one unit of labor (every worker is employed) and demands one unit of housing. For worker i, the utility of locating in city j is:

$$u_{ij} = w_j - r_j + A_j + \epsilon_{ij} = \bar{u}_j + \epsilon_{ij} \tag{C1.1}$$

where w_j is the local wage, r_j is the rental rate of housing, A_j are amenities in city j and the (mean zero) error term ϵ_{ij} denotes worker i's preferences for city j.[48] The systematic component $\bar{u}_j$ denotes utility of residence in city j that is independent of a worker's idiosyncratic taste. Worker i locates in city A (and not in city B) if $u_{iA} \geq u_{iB}$, or $\epsilon_{iB} - \epsilon_{iA} \leq \bar{u}_A - \bar{u}_B$. The measure of workers locating in city A is thus:

$$L_A = G(\bar{u}_A - \bar{u}_B) \tag{C1.2}$$

where $G(\cdot)$ is the cdf of $\epsilon_{iB} - \epsilon_{iA}$.

Firms. Firms produce a single good Y with a constant returns to scale Cobb Douglas production function $Y_j = X_j L_j^{\alpha} K_j^{1-\alpha}$, where L_j and K_j denote production inputs (labor and capital) and X_j denotes productivity in city j. Firms sell their product on the international market at price one and make zero profits. The marginal

[48] Because there are no barriers to worker movement, without idiosyncratic tastes for location workers will be perfectly mobile and any benefit of place-based subsidies will capitalize into housing rents (Bartik, 2020).

cost of capital ρ is constant across cities, but each city applies a capital subsidy τ_j. Firms choose inputs to equate marginal revenue products to marginal costs:

$$w_j = \alpha \frac{Y_j}{L_j}; \qquad \rho(1-\tau_j) = (1-\alpha)\frac{Y_j}{K_j};$$

Combining these leads to the inverse local demand equation:

$$\ln w_j = M + \frac{1}{\alpha}\ln\ X_j - \frac{1-\alpha}{\alpha}\ln\ \rho(1-\tau_j) \tag{C1.3}$$

where $M \equiv \ln\alpha + \frac{(1-\alpha)}{\alpha}\ln(1-\alpha)$ is a constant term. Labor demand is flat in wage-employment space and wages in city j depend positively on local productivity and negatively on the local cost of capital.

Housing market. The marginal cost of producing an additional unit of housing is denoted by $r_j = r(L_j)$, with $r(\cdot)$ increasing in local population due to the fixed availability of land.

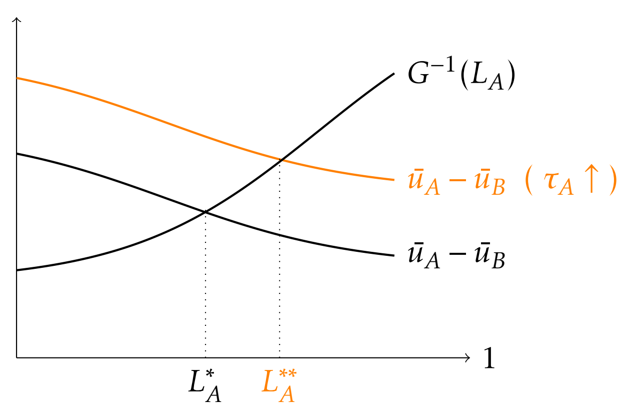
Equilibrium. Combining C1.2 with C1.3 and the housing supply equation leads to the equilibrium condition:

$$G^{-1}(L_A) = \frac{e^M}{\rho^{\frac{(1-\alpha)}{\alpha}}}\left[\frac{X_A^{\frac{1}{\alpha}}}{(1-\tau_A)^{\frac{(1-\alpha)}{\alpha}}} - \frac{X_B^{\frac{1}{\alpha}}}{(1-\tau_B)^{\frac{(1-\alpha)}{\alpha}}}\right] + A_A - A_B - (r(L_A) - r(1-L_A)) \tag{C1.4}$$

The left-hand side can be interpreted as a relative supply curve to city A, defining the taste of a marginal worker for city B relative to city A. As L_A rises, the relative taste for city B increases and the curve slopes up. The right-hand side is instead the relative demand curve (the difference in real wages across the two cities minus the difference in amenities). As L_A rises, real wages in city A decrease relative to city B and the willingness to pay to work in A goes down. At equilibrium, the marginal worker is indifferent between the two cities. Figure C1.1.1 shows the two curves in black and depicts the model's equilibrium city size L_A^*.

The effect of PBIP. Consider now a place-based subsidy that alters the relative cost of capital across the two cities by increasing the capital subsidy in city A. From C1.3, we obtain that an increase in τ_A raises the wage paid in city A by $dw_A/d\tau_A = w_A(1-\alpha)/\alpha(1-\tau_A)$. The policy pushes the relative demand curve up (the orange line in Figure C1.1.1) and leads to a larger equilibrium share of workers in city A, L_A^{**}. A similar conclusion obtains if the policy increases local productivity X_A, through for example investment in infrastructure.[49] Notably, the model imposes that any increase in employment in city A occurs through out-migration from city B. In the data, we test whether the policy had any effect on local labor market participation and unemployment. In the presence of agglomeration economies in production, $X_j = X(L_j)$, the relative demand curve might become upward sloping in some segments and multiple equilibria arise. In this setting, large enough government intervention might push the economy in a developed equilibrium in the long run (Kline, 2010).

Appendix Figure C1.1.1. The employment effects of PBIP

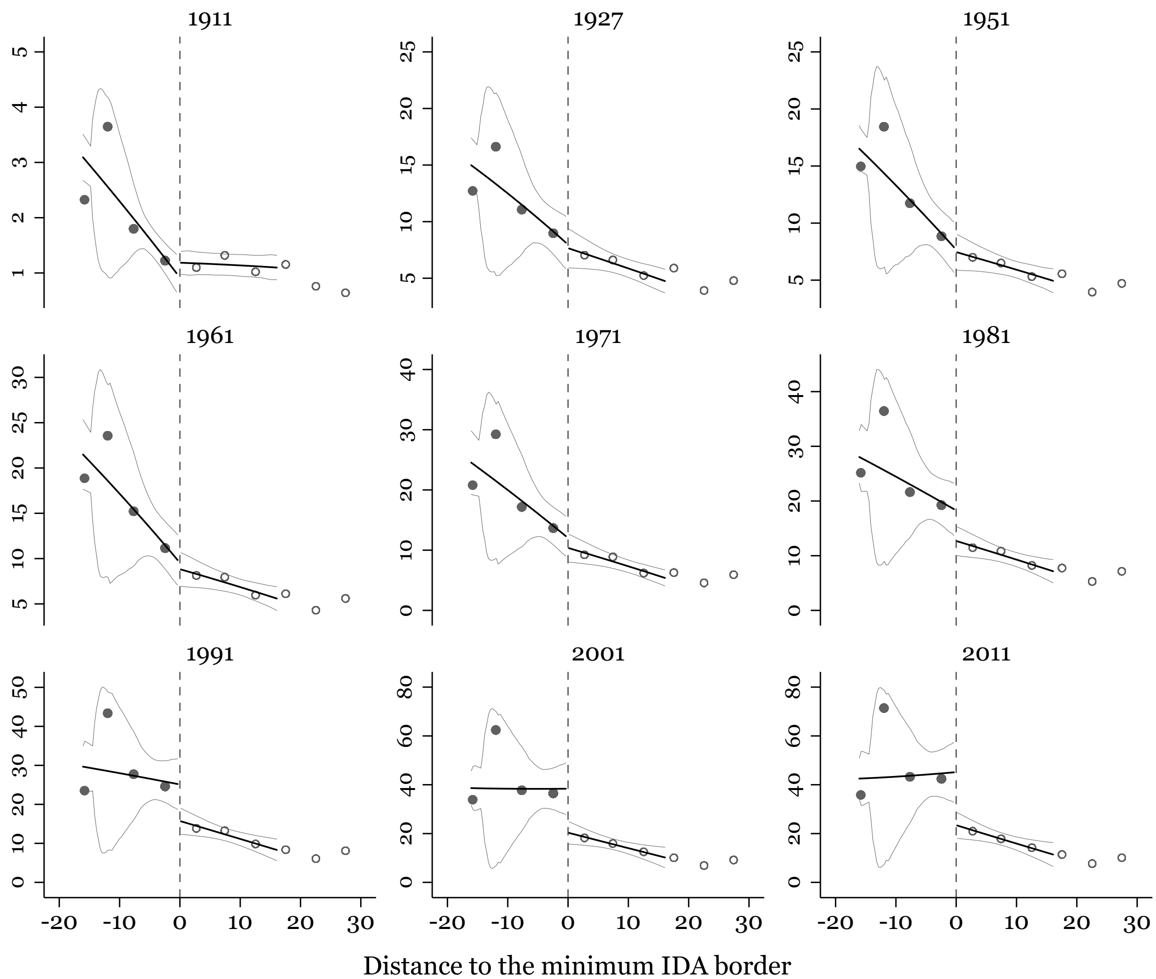


The graph shows the spatial equilibrium of the model described in Appendix 1.C.1. The black demand and supply curves denote the initial equilibrium. The orange demand curve is the one resulting from an increase in the capital subsidy in city A. See Kline (2010), Kline and Moretti (2014b) and text for details.

[49] Here we do not make any parametric assumption on the shape of the $G(\cdot)$ and $r(\cdot)$ functions and we do not explicitly derive the effects on employment. These would depend on workers' preferences for location, which determine worker mobility, and on the local elasticity of housing supply. See Kline and Moretti (2014b) for a more detailed analysis.

1.C.2 Appendix C2: Results

Appendix Figure C1.2.1. Establishment density



Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Table C1.2.1. Establishment density – Baseline

	Reduced form	2-SLS	
		IDA status	EIM subsidies
	(1)	(2)	(3)
	Contemporaneous effect (1991)		
RD Estimate	9.18	23.50	1.60
	(4.82)*	(11.01)**	(0.81)*
Mean around the border	15.08	15.08	14.82
Standard deviation	21.98	21.98	21.53
Observations	586	586	562
R^2	0.23		
KP F-stat		19.06	5.18
	Persistent effect (2011)		
RD Estimate	19.83	50.73	3.43
	(8.97)*	(20.58)**	(1.63)**
Mean around the border	23.10	23.10	22.63
Standard deviation	37.88	37.88	36.87
Observations	586	586	562
R^2	0.25		
KP F-stat		19.06	5.18

Column (1) shows the estimation output of Equation 1.1b. Column (2) reports the fuzzy RD estimates. Column (3) replaces IDA status with EIM subsidies as treatment variable. All regressions are estimated over a 16-km symmetric bandwidth around the minimum IDA border and control for a linear polynomial in the distance from the border and IDA region effects. Standard errors clustered by IDA region in parentheses. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table C1.2.2. Employment density – Robustness tests

	(1) 2^{nd} order	(2) 3^{rd} order	(3) Excl. centers	(4) Dist. to center	(5) No IDA reg. eff.
			Contemporaneous effect (1991)		
RD Estimate	82.35	92.91	81.44	111.98	107.72
	(38.96)**	(40.20)**	(41.01)*	(43.71)**	(40.82)**
Mean around the border	47.62	47.62	42.39	47.62	47.62
Standard deviation	79.68	79.68	66.86	79.68	79.68
Observations	586	586	574	586	586
KP F-stat	26.03	12.69	18.52	18.60	22.58
			Persistent effect (2011)		
RD Estimate	123.04	140.17	126.85	162.57	157.70
	(61.84)*	(67.47)**	(60.08)**	(63.91)**	(59.35)**
Mean around the border	62.97	62.97	56.39	62.97	62.97
Standard deviation	108.15	108.15	93.55	108.15	108.15
Observations	586	586	574	586	586
KP F-stat	26.03	12.69	18.52	18.60	22.58

Replication of Table 1.3, Column (2), robustness checks. Columns (1) and (2) specify $\varphi(\delta_m)$ as a quadratic and cubic polynomial, respectively. Column (3) excludes IDA centers from the estimation sample. Column (4) controls linearly for the distance to the IDA center. Column (5) excludes IDA region effects from the baseline specification. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table C1.2.3. Employment and establishment density – Conley standard errors

	Employment per km^2		Establishments per km^2	
	1991	2011	1991	2011
RD Estimate	43.31	62.99	9.18	19.83
	(12.00)***	(16.81)***	(3.25)***	(5.90)***
Mean around the border	47.62	62.97	15.08	23.10
Standard deviation	79.68	108.15	21.98	37.88
Observations	586	586	586	586

Replication of Table 1.3, Column (1). Standard errors allow for spatial correlation (Conley, 1999). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table C1.2.4. Employment and establishment density – Randomization inference

	Employment per km^2		Establishments per km^2	
	1991	2011	1991	2011
ITT	47.06	73.62	13.21	27.57
Finite sample P-value	0.00	0.00	0.01	0.01
Asymptotic P-value	0.01	0.01	0.01	0.01
Window	2.06	2.06	2.06	2.06

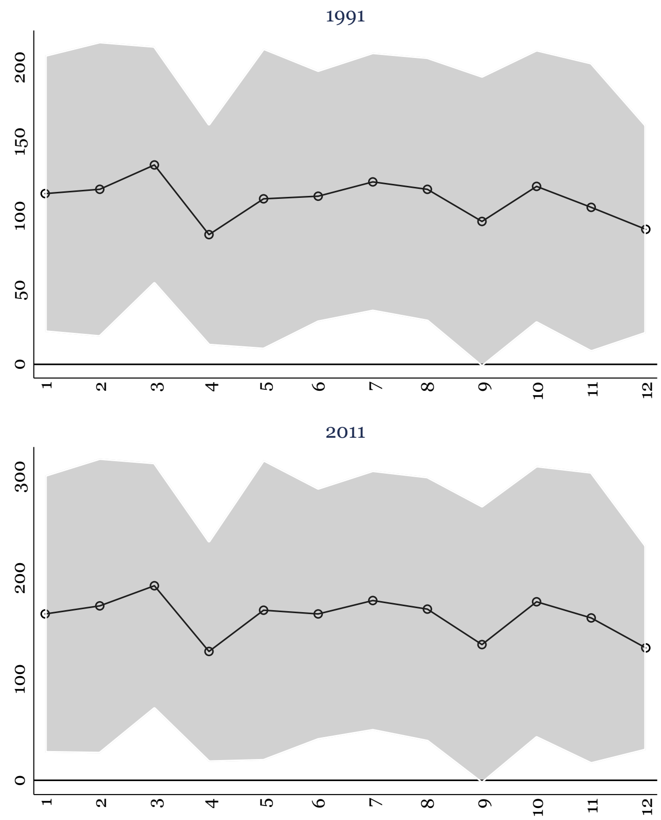
Estimation output for the fuzzy RD desing using local randomization inference as proposed in Cattaneo et al. (2016), with 1,000 replications, uniform kernel and without specifying a polynomial for the outcome transformation model – see the *rdrandinf* command in Cattaneo et al. (2016). The window-selection procedure is built on balance tests for RD under local randomization – see the *rdwinselect* command in Cattaneo et al. (2016). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table C1.2.5. Employment density – All IDAs

	Reduced form	2-SLS	
		IDA status	EIM subsidies
	(1)	(2)	(3)
	Contemporaneous effect (1991)		
RD Estimate	50.01	157.95	8.44
	(19.19)**	(68.70)**	(4.01)**
Mean around the border	70.49	70.49	69.78
Standard deviation	111.57	111.57	111.24
Observations	775	775	744
R^2	0.40		
KP F-stat		15.42	7.87
	Persistent effect (2011)		
RD Estimate	64.04	202.25	10.36
	(24.82)**	(83.97)**	(4.63)**
Mean around the border	96.25	96.25	94.95
Standard deviation	149.60	149.60	148.15
Observations	775	775	744
R^2	0.45		
KP F-stat		15.42	7.87

Replication of Table 1.3, including also the Napoli and Caserta IDAs (excluded from the baseline analysis because of the small distance between the two IDA centers). Standard errors clustered by IDA region in parentheses. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure C1.2.2. Employment density – Exclude individual IDAs



Estimates of the fuzzy RD coefficient and 95 percent confidence intervals excluding one IDA region at a time in 1991 (top panel) and 2011 (bottom panel). Each point on the horizontal axis denotes a specification where one of the IDA regions is removed from the sample.

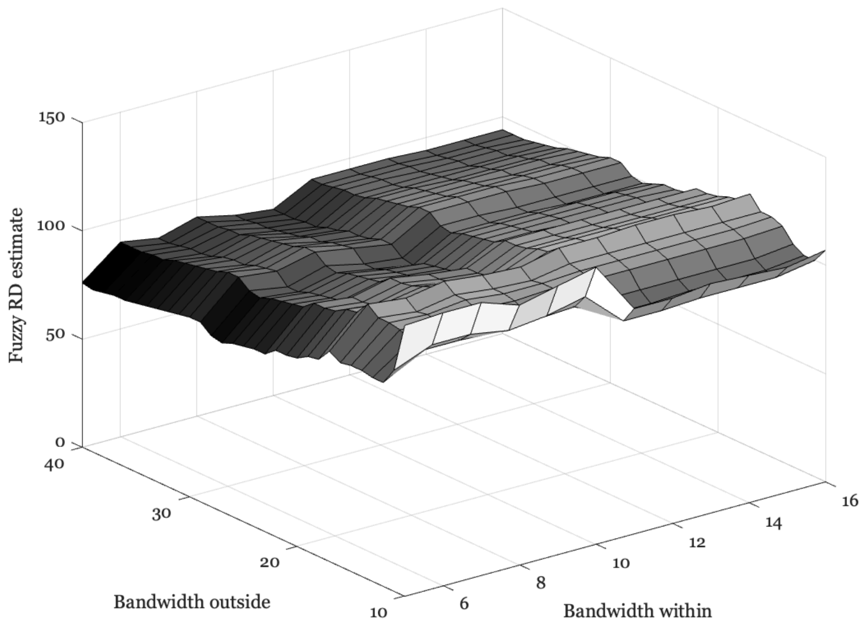
Appendix Table C1.2.6. Employment density – Non-parametric fuzzy RD estimates

	Contemporaneous effect (1991)		Persistent effect (2011)	
	Conventional	Robust	Conventional	Robust
RD Estimate	106.87	143.59	178.46	234.04
	(66.06)	(89.24)	(105.19)*	(139.36)*
Bandwidth within	5.94	5.94	6.42	6.42
Bandwidth outside	22.00	22.00	20.74	20.74
Mean around the border	40.84	40.84	54.36	54.36
Standard deviation	68.63	68.63	95.10	95.10
Observations	708	708	680	680

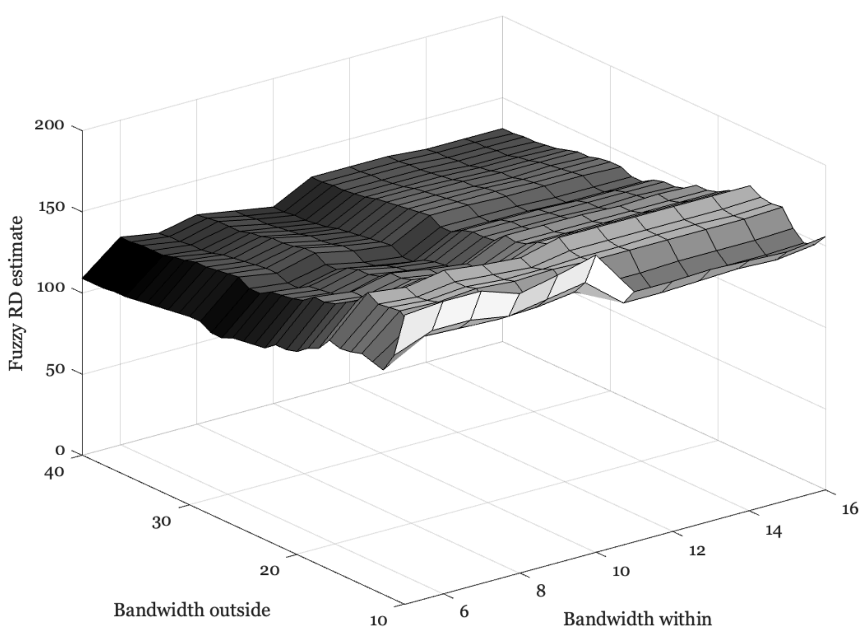
Fuzzy RD estimates obtained using the non-parametric estimation and robust bias-corrected inference method proposed by Calonico et al. (2014a). The optimal bandwidth is computed by minimizing the Mean Squared Error separately left and right of the cutoff. Observations are weighted using a triangular kernel. The specification controls for IDA region effects and standard errors are clustered by IDA region. See text for details. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Appendix Figure C1.2.3. Employment density – robustness to bandwidth choice

(a) 1991



(b) 2011



Estimates of the fuzzy RD coefficient using varying bandwidths around the RD cutoff in 1991 (top) and 2011 (bottom).

Appendix Table C1.2.7. Migration and relocation – Fuzzy RD estimates

	Net migration	Mobil.	Mobil. work
	Contemporaneous effect (1991)		
RD Estimate	0.02	5.35	69.44
	(0.09)	(2.96)*	(38.37)*
Mean around the border	-0.02	19.35	108.48
Standard deviation	0.31	8.48	92.48
Observations	587	587	587
	Persistent effect (2011)		
RD Estimate	-0.30	4.19	62.07
	(0.24)	(3.06)	(46.61)
Mean around the border	-0.04	25.75	155.80
Standard deviation	0.63	9.52	115.50
Observations	587	587	587

Replication of Table 1.3, Column (2). "Net migration" is the net inflow of immigrants into the municipality as a share of resident population. "Mobil." is the share of resident population who travel daily for work or study outside the municipality of residence to the resident population aged up to 64. "Mobil. work" is the share of resident population commuting daily for work outside the municipality of residence to resident population commuting daily for work within the municipality of residence. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table C1.2.8. (Log) Employment and population density estimates

	(Log) Employment density		(Log) Population density	
	Red. Form	2-SLS	Red. Form	2-SLS
	Contemporaneous effect (1991)			
RD Estimate	0.51	1.30	0.41	1.06
	(0.21)**	(0.49)**	(0.16)**	(0.37)***
Mean around the border	3.00	3.00	5.16	5.16
Standard deviation	1.30	1.30	1.13	1.13
Observations	586	586	587	587
	Persistent effect (2011)			
RD Estimate	0.55	1.41	0.39	1.00
	(0.22)**	(0.52)**	(0.16)**	(0.37)**
Mean around the border	3.16	3.16	5.20	5.20
Standard deviation	1.44	1.44	1.21	1.21
Observations	586	586	587	587

Replication of Table 1.3, Columns (1)-(2). Outcomes defined as the logarithm of the number of workers per km^2 and of the number of residents per km^2. Standard errors clustered by IDA region in parentheses. See text for details. * p<0.10, ** p<0.05, *** p<0.01

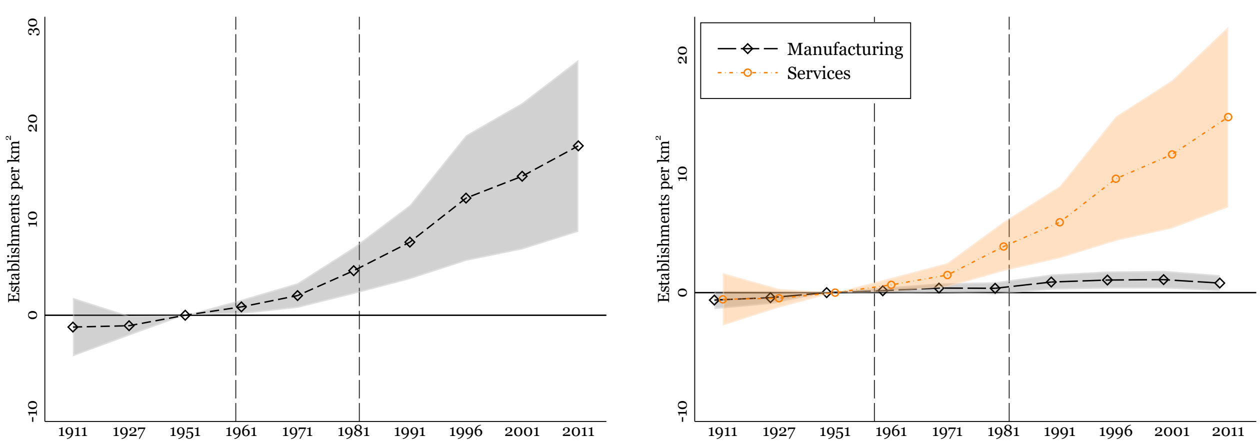
Appendix Table C1.2.9. Employment and participation rate – Fuzzy RD estimates

	1981	1991	2011
	Employment rate		
RD Estimate	4.75	3.97	1.90
	(1.60)***	(1.69)**	(1.31)
Mean around the border	36.23	33.88	38.33
Standard deviation	5.78	5.68	4.66
Observations	581	587	587
	Participation rate		
RD Estimate	3.45	3.40	3.09
	(1.26)**	(1.17)***	(1.32)**
Mean around the border	46.91	47.21	46.13
Standard deviation	5.99	4.51	4.50
Observations	581	587	587
	Unemployment rate		
RD Estimate	-4.65	-3.56	1.51
	(2.31)**	(2.17)	(1.75)
Mean around the border	22.75	28.33	16.97
Standard deviation	7.67	9.32	5.18
Observations	581	587	587

Replication of Table 1.3, Column (2). "Employment rate" is the ratio of employed people to total residents aged 15 years and older. "Participation rate" is the ratio of the resident working population to the resident population of the same age group. "Unemployment rate" is the ratio of the resident population 15 years and older seeking employment to resident population 15 years and older in employment. See text for details. * p<0.10, ** p<0.05, *** p<0.01

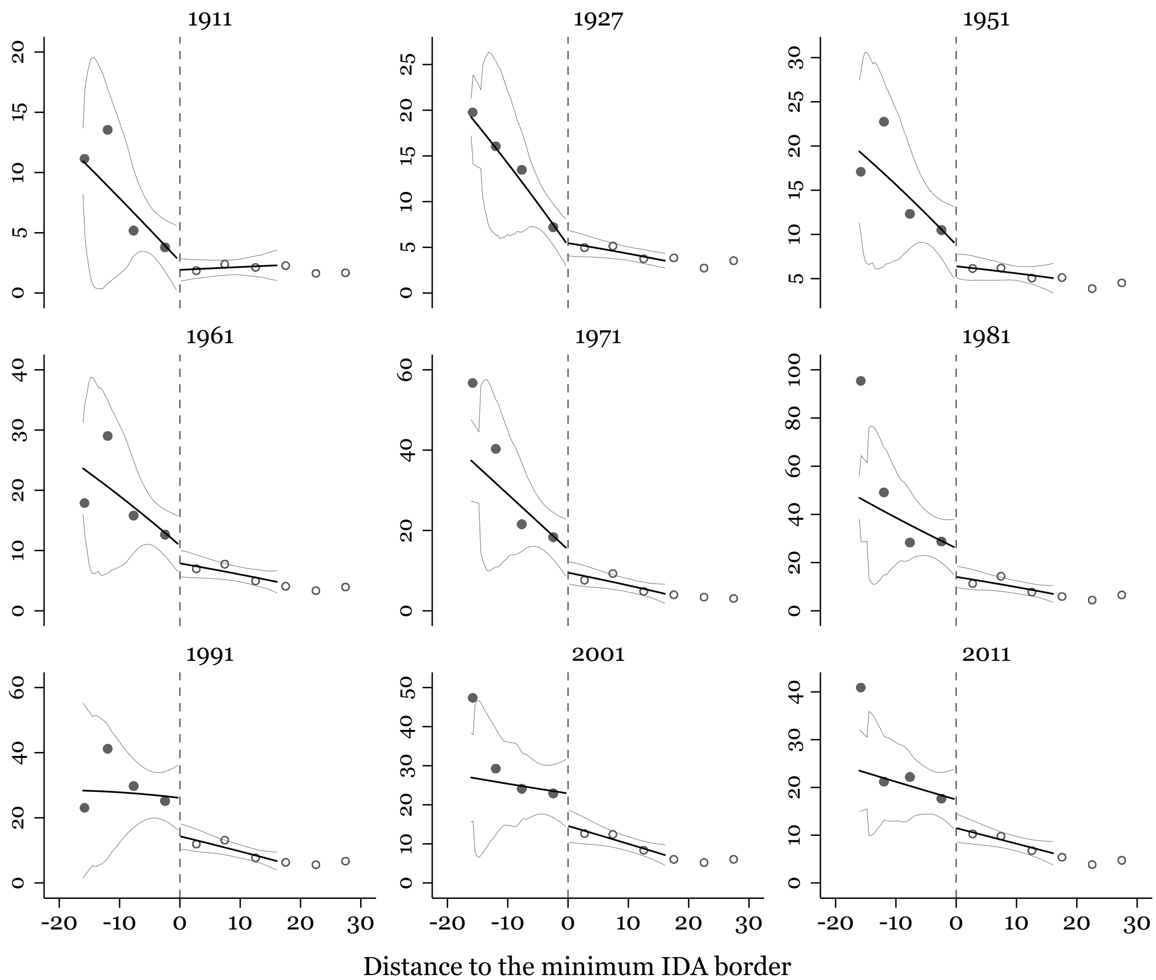
Appendix Figure C1.2.4. Establishment density – Diff-in-Disc

(a) Establishment density (b) Manufacturing versus services



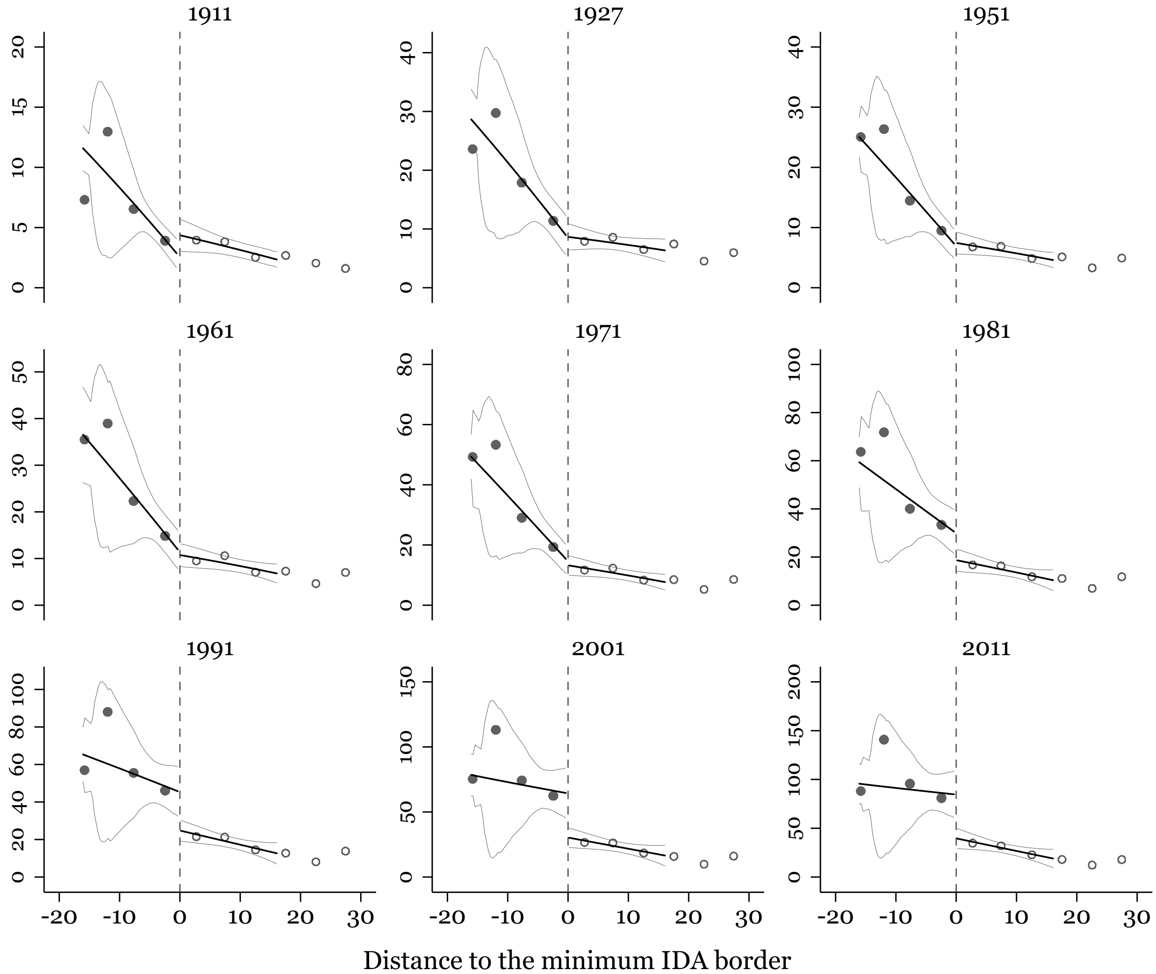
Coefficient estimates for Equation 1.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Figure C1.2.5. Manufacturing employment density



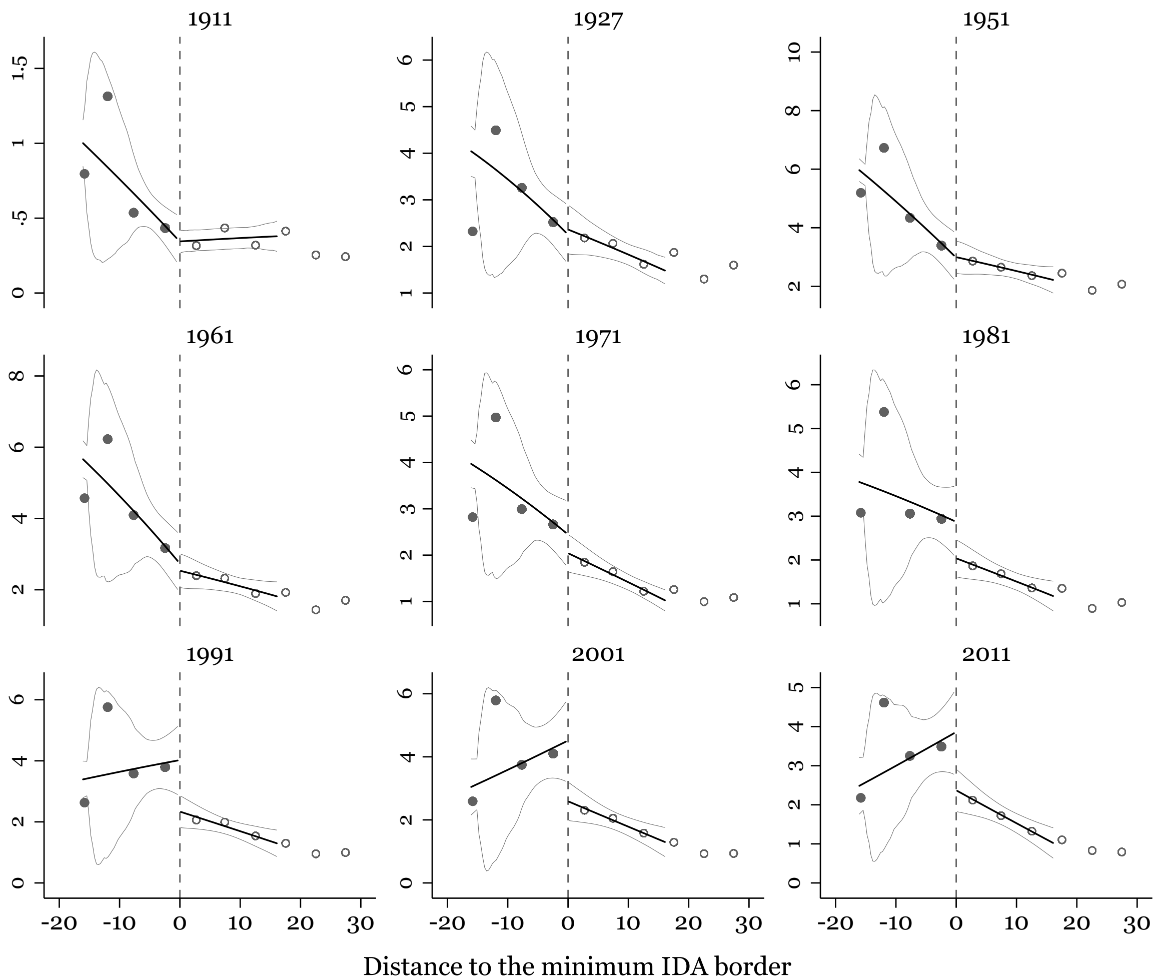
Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure C1.2.6. Services employment density



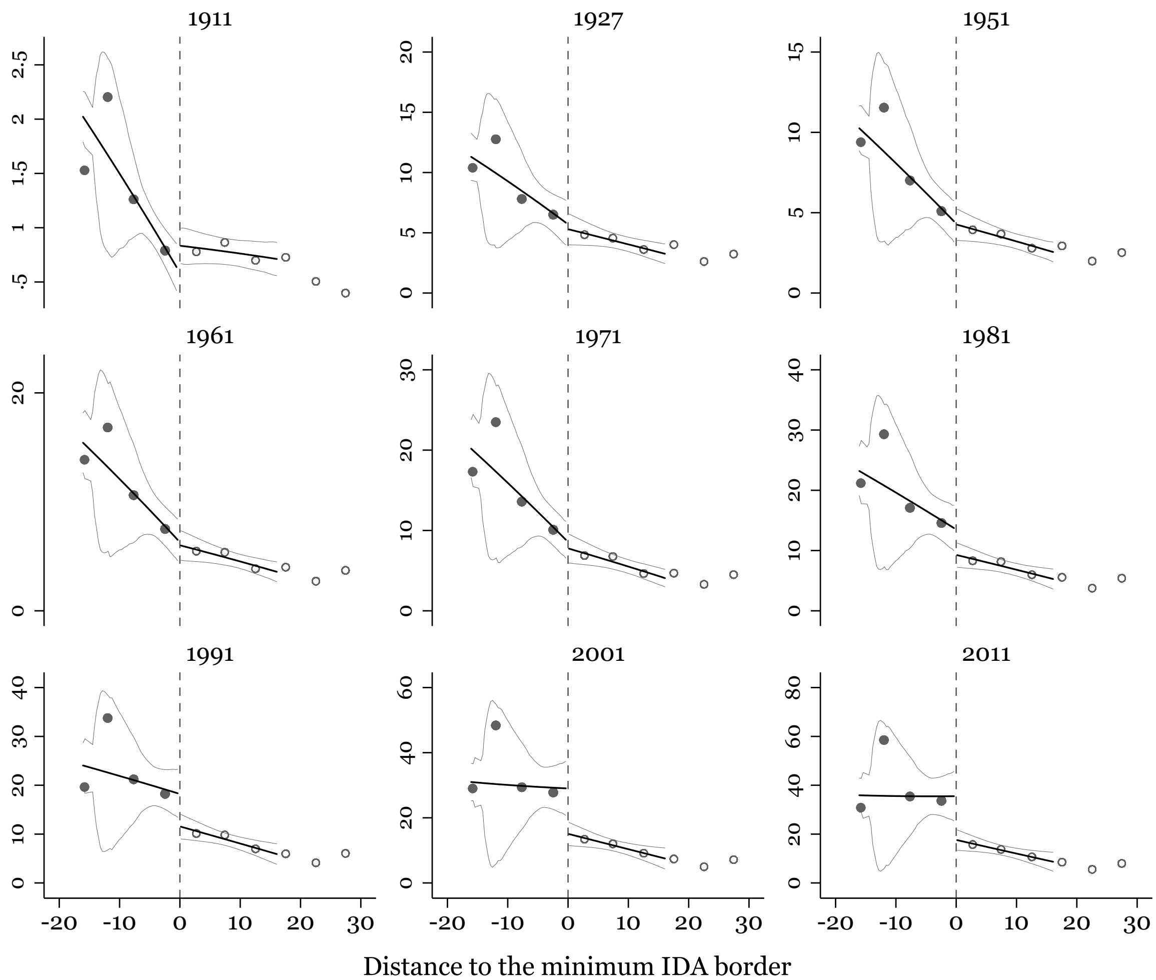
Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure C1.2.7. Manufacturing establishment density



Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure C1.2.8. Services establishment density



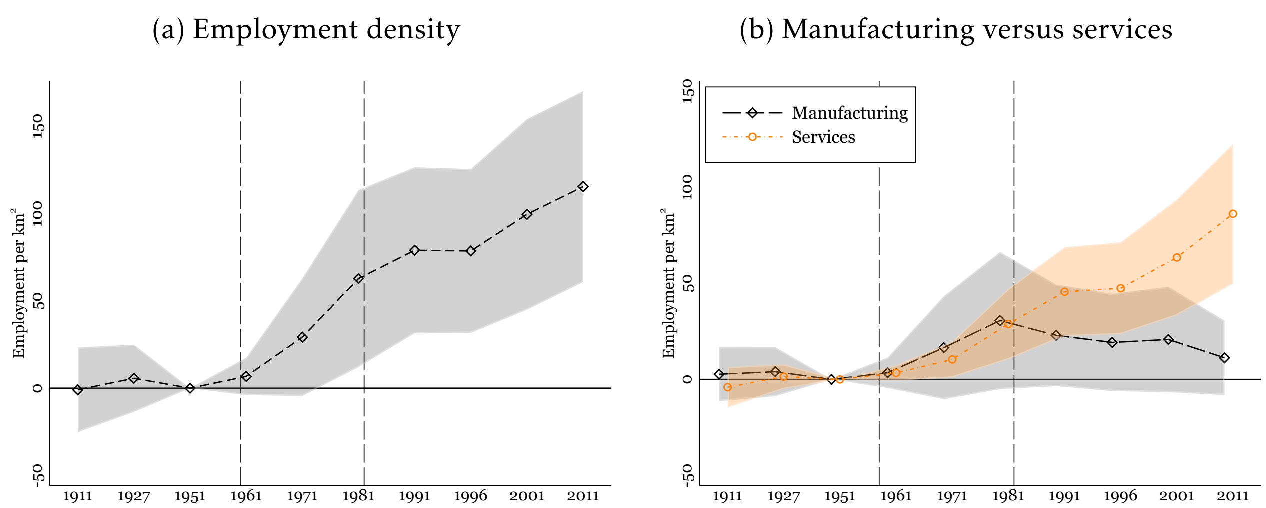
Negative distance denotes municipalities within the minimum IDA border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately at either side of the border using a symmetric 16-km bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Table C1.2.10. Manufacturing and services densities – Fuzzy RD estimates

	Employment density		Establishment density	
	Manufacturing	Services	Manufacturing	Services
	Contemporaneous effect (1991)			
RD Estimate	28.27	57.40	3.69	17.76
	(14.08)**	(23.17)**	(1.61)**	(8.32)**
Mean around the border	14.06	25.45	2.26	11.10
Standard deviation	26.80	43.14	3.30	16.90
Observations	586	586	586	586
	Persistent effect (2011)			
RD Estimate	14.99	112.61	2.75	43.22
	(9.68)	(45.43)**	(1.51)*	(17.35)**
Mean around the border	11.01	41.52	2.08	17.87
Standard deviation	18.74	75.44	3.08	30.85
Observations	586	586	586	586

Replication of Table 1.3, Column (2). See text for details. * p<0.10, ** p<0.05, *** p<0.01

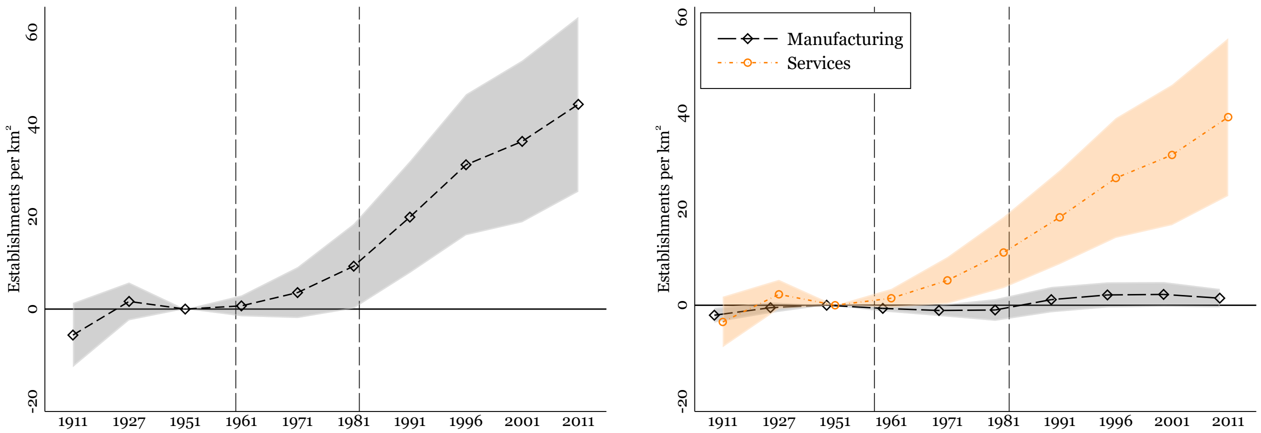
Appendix Figure C1.2.9. Event study using Center-North (within) – Empl. density



Coefficient estimates for Equation B3.1. Sample restricted to municipalities within the minimum IDA border excluding IDA centers (treatment group) and municipalities bordering placebo centers (control group). Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Figure C1.2.10. Event study using Center-North (within) – Est. density

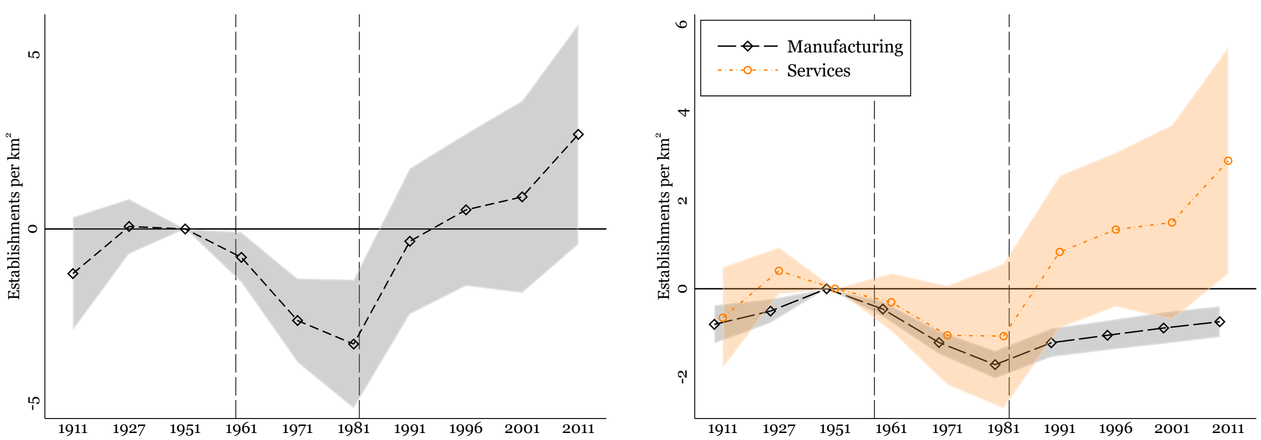
(a) Establishment density (b) Manufacturing versus services



Coefficient estimates for Equation B3.1. Sample restricted to municipalities within the minimum IDA border excluding IDA centers (treatment group) and municipalities bordering placebo centers (control group). Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Figure C1.2.11. Event study using Center-North (outside) – Est. density

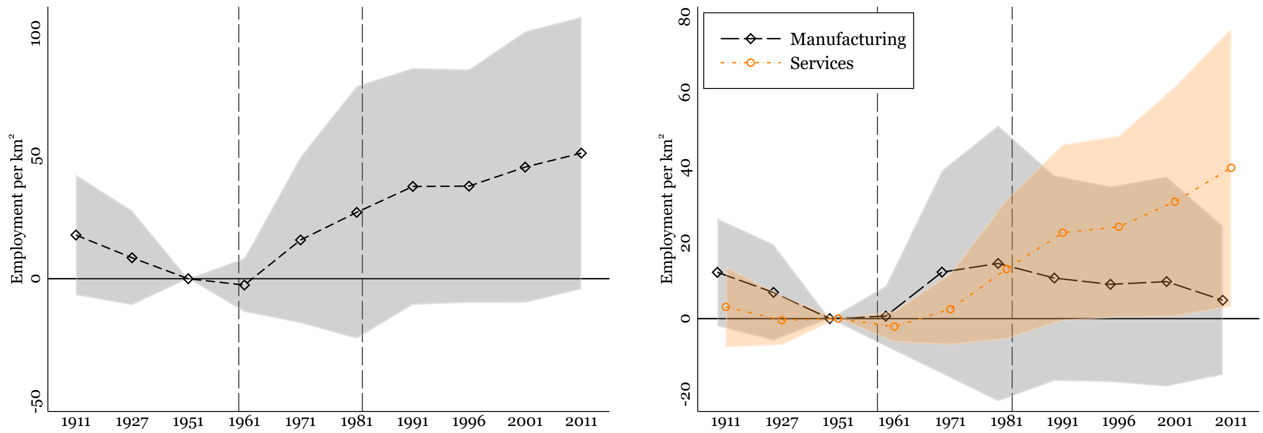
(a) Establishment density (b) Manufacturing versus services



Coefficient estimates for Equation B3.1. Sample restricted to municipalities up to 16 km outside of the minimum IDA border (treatment group) and municipalities up to 16 km outside of the placebo border traced by municipalities bordering placebo centers (control group). The treatment group excludes IDA municipalities. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Figure C1.2.12. Triple differences – Empl. density

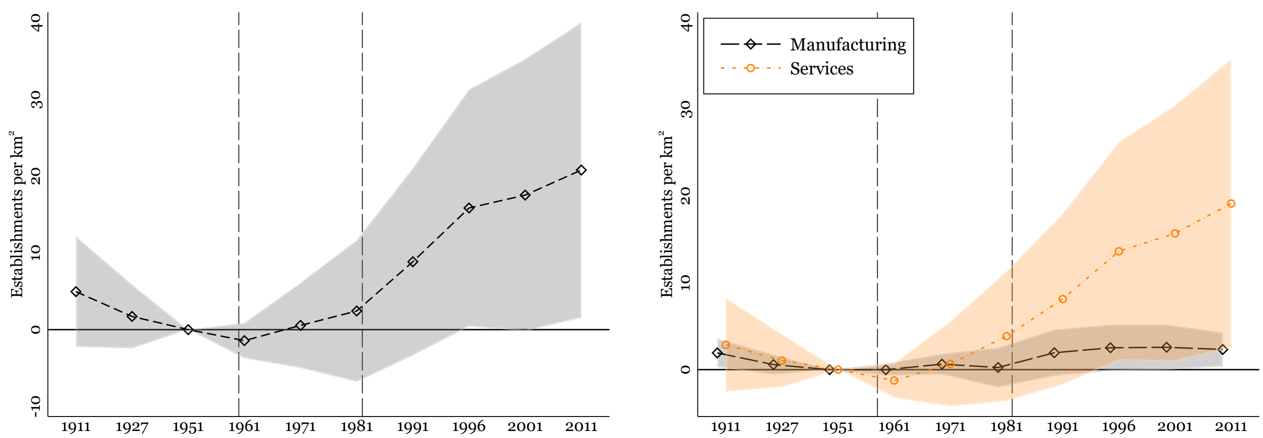
(a) Employment density | (b) Manufacturing versus services



Coefficient estimates for Equation B3.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Figure C1.2.13. Triple differences – Est. density

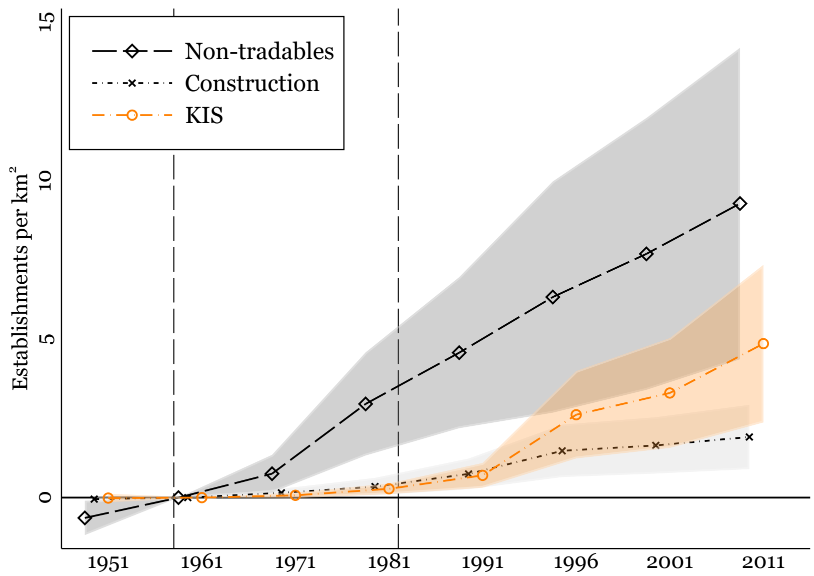
(a) Establishment density | (b) Manufacturing versus services



Coefficient estimates for Equation B3.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

1.D Appendix D

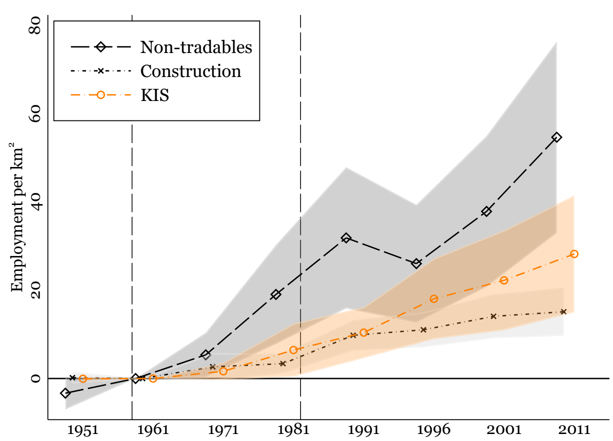
Appendix Figure D1.1. Establishment density – Services breakdown



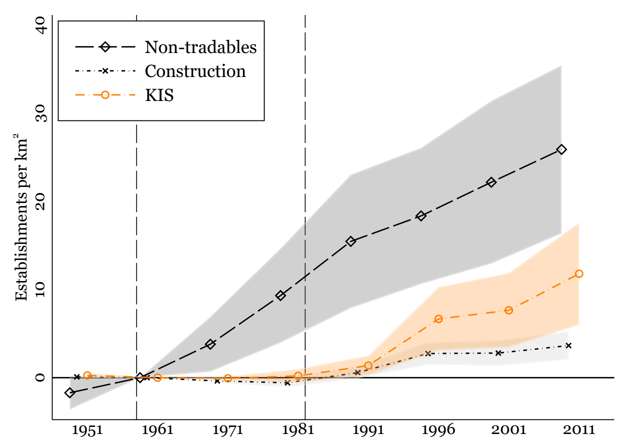
Coefficient estimates for Equation 1.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. "Non-tradables" include wholesale and retail trade, hotels and restaurants and other services (education, health, arts and entertainment, other). "KIS" (knowledge-intensive services) include communication, finance and insurance and services to firms. See text for details.

Appendix Figure D1.2. Event study using Center-North (within) – Services breakdown

(a) Employment density



(b) Establishment density

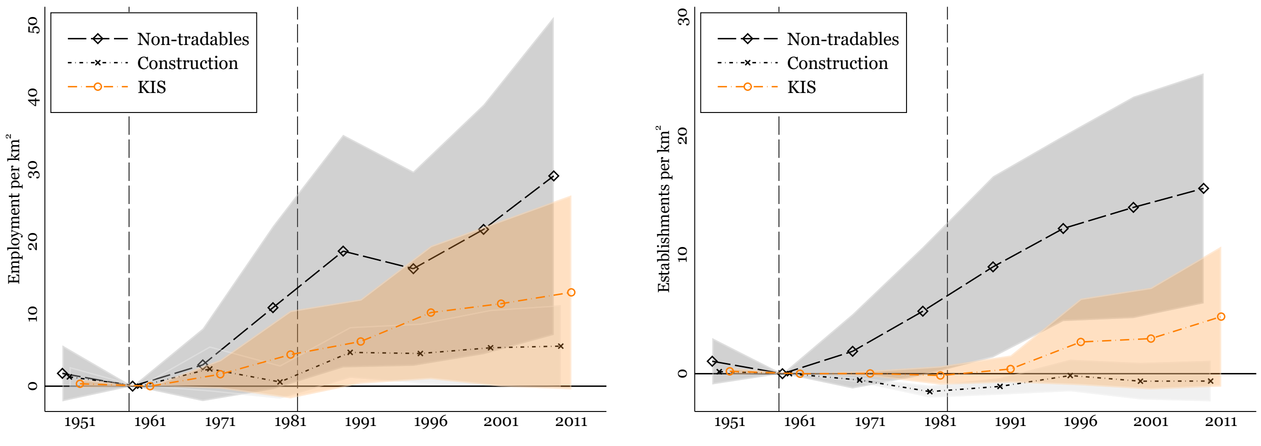


Coefficient estimates for Equation B3.1. Sample restricted to municipalities within the minimum IDA border excluding IDA centers (treatment group) and municipalities bordering placebo centers (control group). Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Figure D1.3. Triple differences – Services breakdown

(a) Employment density

(b) Establishment density



Coefficient estimates for Equation B3.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the IDAs. See text for details.

Appendix Table D1.1. Employment and firm shares in services – Fuzzy RD estimates

	Employment		Firms	
	KIS	Other serv.	KIS	Other serv.
	Contemporaneous effect (1991)			
RD Estimate	0.08	-0.08	0.06	-0.06
	(0.06)	(0.06)	(0.03)**	(0.03)**
Mean around the border	0.17	0.83	0.11	0.89
Standard deviation	0.19	0.19	0.10	0.10
Observations	570	570	570	570
	Persistent effect (2011)			
RD Estimate	0.08	-0.08	0.06	-0.06
	(0.04)**	(0.04)**	(0.02)***	(0.02)***
Mean around the border	0.10	0.90	0.10	0.90
Standard deviation	0.10	0.10	0.06	0.06
Observations	585	585	585	585

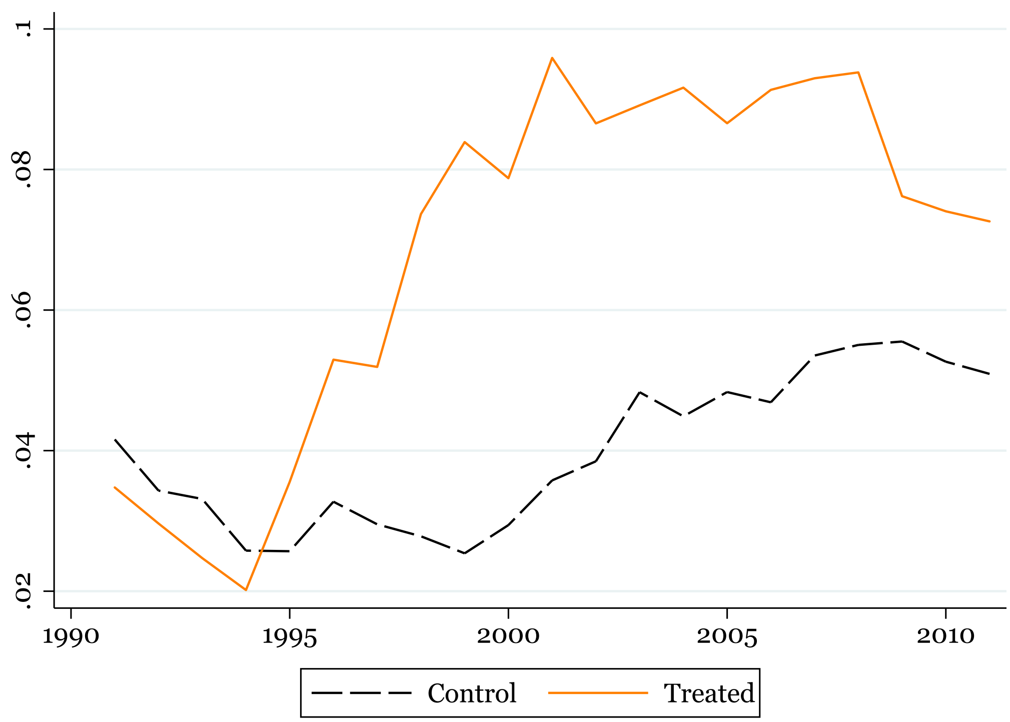
Replication of Table 1.3, Column (2). The outcomes are the share of employment and establishments in KIS and other services. The shares are obtained from social security data on the universe of Italian firms and the KIS classification is obtained from Eurostat/OECD. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table D1.2. Employment and firm shares in manufacturing – Fuzzy RD estimates

	Employment, 1991		Establishments, 1991	
	High-tech	Low-tech	High-tech	Low-tech
RD Estimate	0.27	-0.27	0.15	-0.15
	(0.09)***	(0.09)***	(0.05)***	(0.05)***
Mean around the border	0.16	0.84	0.14	0.86
Standard deviation	0.21	0.21	0.14	0.14
Observations	566	566	566	566

Replication of Table 1.3, Column (2). The outcomes are the share of employment across manufacturing sub-sectors, grouped by technological intensity. The shares are obtained from social security data on the universe of Italian firms and the technology classification is obtained from Eurostat/OECD. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure D1.4. Share of KIS new hires from high-technology manufacturing



The graph shows the cumulative share of job-to-job new hires in KIS coming from high-technology manufacturing, separately for treated and control municipalities, since 1991. "KIS" (knowledge-intensive services) include communication, finance and insurance and services to firms. The shares are computed for municipalities included in the baseline estimation sample. Treated municipalities are those bordering IDA centers. See text for details.

Appendix Table D1.3. Employment shares within 3-digit services – Fuzzy RD estimates

	RD Estimate	S.E.	Mean	S.D.
Other human resources provision	3.17	(1.76)*	0.31	3.82
Maintenance and repair of motor vehicles	2.49	(0.66)***	4.31	7.14
Computer programming, consultancy and related	1.60	(0.66)**	0.91	2.53
Other specialised wholesale	1.43	(0.84)*	1.93	3.48
Reinsurance	0.72	(0.41)*	0.39	1.55
Sports activities	0.69	(0.38)*	0.31	1.79
Management consultancy activities	0.49	(0.21)**	0.34	1.05
Legal activities	0.30	(0.16)*	0.45	0.80
Renting and operating of own or leased real estate	0.07	(0.04)*	0.05	0.24
Other telecommunications activities	0.07	(0.04)	0.03	0.18
Passenger air transport	0.03	(0.01)*	0.00	0.04
Fund management activities	0.01	(0.01)	0.00	0.03
Wholesale and retail trade and repair of motor vehicles	-0.01	(0.01)*	0.00	0.02
Retail sale in non-specialised stores	-0.13	(0.08)*	0.03	0.18
Wholesale of agricultural raw materials and live animals	-1.24	(0.77)	0.85	5.30
Retail sale of food, beverages and tobacco	-2.91	(1.06)***	3.28	4.82

Replication of Table 1.3, Column (2). Regressions run for employment shares within services using 3-digit sectors. We show estimates with p-value<0.11. Each outcome is in percentage units. Standard errors clustered by IDA region in parentheses. Descriptive statistics computed within the estimation sample. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table D1.4. Firm shares within 3-digit services – Fuzzy RD estimates

	RD Estimate	S.E.	Mean	S.D.
Reinsurance	0.79	(0.49)	0.66	1.80
Management consultancy activities	0.68	(0.30)**	0.44	1.01
Data processing, hosting and related activities; web portals	0.66	(0.41)	0.52	1.29
Sports activities	0.64	(0.36)*	0.39	1.61
Legal activities	0.55	(0.28)**	0.75	1.13
Other professional, scientific and technical activities n.e.c.	0.47	(0.19)**	0.33	0.99
Support activities for transportation	0.44	(0.17)***	0.73	1.47
Buying and selling of own real estate	0.41	(0.20)**	0.15	0.63
Retail trade not in stores, stalls or markets	0.26	(0.09)***	0.16	0.52
Other postal and courier activities	0.14	(0.08)*	0.06	0.24
Wholesale of information and communication equipment	0.11	(0.06)**	0.12	0.39
Market research and public opinion polling	0.11	(0.06)*	0.04	0.21
Fund management activities	0.03	(0.01)*	0.01	0.06
Translation and interpretation activities	0.01	(0.00)*	0.00	0.01
Wholesale and retail trade and repair of motor vehicles	-0.04	(0.02)**	0.01	0.05
Retail sale in non-specialised stores	-0.21	(0.11)*	0.05	0.26
Beverage serving activities	-3.16	(1.83)*	9.77	7.36
Retail sale of food, beverages and tobacco	-4.15	(1.19)***	5.38	4.57

Replication of Table 1.3, Column (2). Regressions run for firm shares within services using 3-digit sectors. We show estimates with p-value<0.11. Each outcome is in percentage units. Standard errors clustered by IDA region in parentheses. Descriptive statistics computed within the estimation sample. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table D1.5. Worker AKM effects – Fuzzy RD estimates (2011)

	Total	By sector		Within services	
		Manufacturing	Services	KIS	Other serv.
RD Estimate	0.07	0.03	0.14	0.22	0.13
	(0.02)***	(0.05)	(0.05)**	(0.11)**	(0.05)**
Mean around the border	-0.17	-0.17	-0.22	-0.19	-0.22
Standard deviation	0.11	0.12	0.18	0.21	0.19
Observations	576	506	548	327	544

Replication of Table 1.3, Column (2). The outcomes are the worker fixed effects from an AKM model of the (log) wage (Abowd et al., 1999) estimated between 1991 and 2011. The worker effects are then averaged at the municipality level. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table D1.6. Firm size and wage distribution – Fuzzy RD estimates

	Firm size			Firm wage		
	T1	T2	T3	T1	T2	T3
	Contemporaneous effect (1991)					
RD Estimate	-0.02	-0.04	0.06	-0.10	0.04	0.06
	(0.03)	(0.03)	(0.04)	(0.03)***	(0.02)**	(0.04)
Mean around the border	0.42	0.32	0.26	0.39	0.31	0.30
Standard deviation	0.13	0.10	0.11	0.14	0.10	0.12
Observations	582	582	582	582	582	582
	Persistent effect (2011)					
RD Estimate	-0.05	-0.02	0.07	-0.04	-0.01	0.05
	(0.03)*	(0.02)	(0.03)**	(0.02)**	(0.01)	(0.02)**
Mean around the border	0.43	0.33	0.24	0.35	0.33	0.32
Standard deviation	0.09	0.07	0.09	0.10	0.07	0.10
Observations	586	586	586	586	586	586

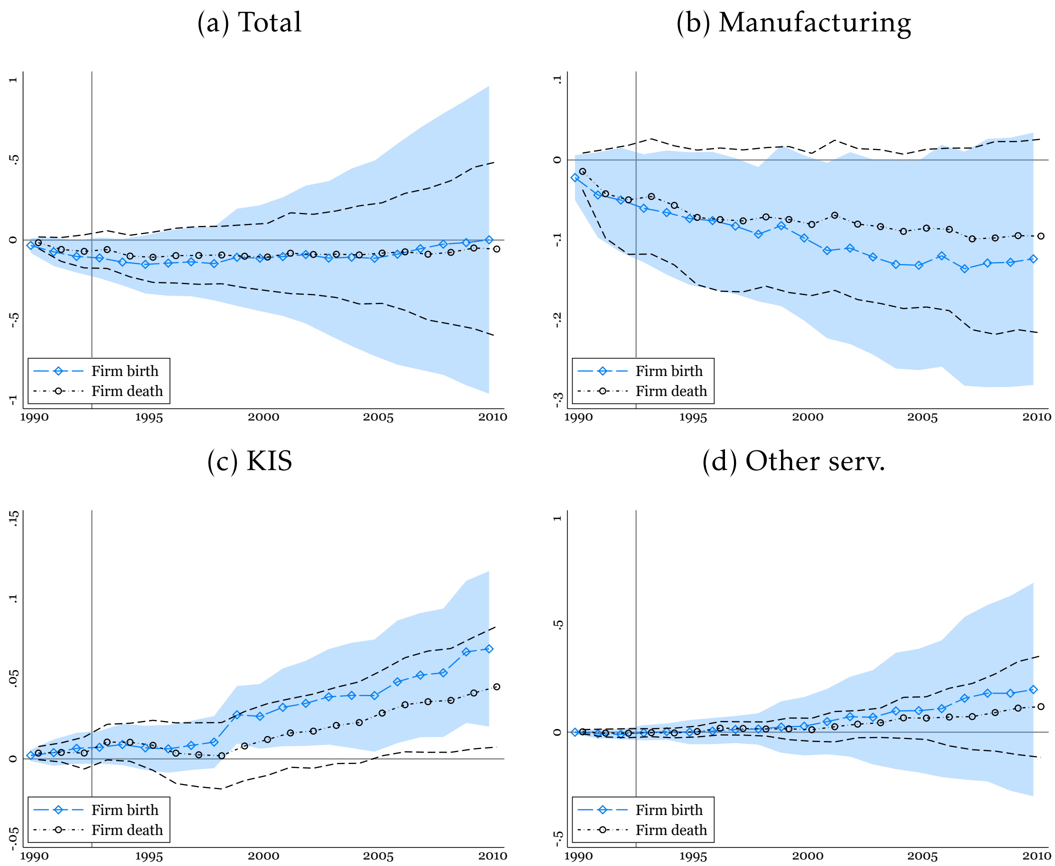
Replication of Table 1.3, Column (2). Outcomes are computed as the share of firms in each tertile of the distribution of firm size and wage paid. Tertiles are derived on the universe of the Italian firms each year. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table D1.7. Balance sheet outcomes, 2011 – Fuzzy RD estimates

	Total	By sector		Within services	
		Manufacturing	Services	KIS	Other serv.
			Value added		
RD Estimate	0.52	1.54	0.04	1.43	-0.16
	(0.31)*	(0.53)***	(0.31)	(0.64)**	(0.33)
Mean around the border	4.49	4.31	4.24	4.00	4.23
Standard deviation	0.88	1.07	0.90	1.12	0.91
Observations	577	507	545	369	543
			Investment		
RD Estimate	0.31	1.02	0.48	1.98	0.34
	(0.25)	(0.43)**	(0.35)	(0.99)**	(0.36)
Mean around the border	2.87	2.68	2.60	2.04	2.59
Standard deviation	1.14	1.41	1.25	1.56	1.27
Observations	582	516	553	369	552
			Sales		
RD Estimate	0.42	1.35	0.04	1.40	-0.05
	(0.35)	(0.55)**	(0.38)	(0.72)*	(0.42)
Mean around the border	6.07	5.78	6.00	5.00	6.04
Standard deviation	0.92	1.20	0.99	1.19	1.00
Observations	582	519	558	378	556
			Profits		
RD Estimate	1.04	2.23	0.82	-0.66	0.84
	(0.49)**	(0.82)***	(0.62)	(1.02)	(0.68)
Mean around the border	2.21	2.26	2.01	2.07	2.03
Standard deviation	1.42	1.63	1.49	1.69	1.47
Observations	361	285	316	240	307

Replication of Table 1.3, Column (2). All outcomes are as of 2011 and expressed in natural logarithm, scaled by total firm workforce. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure D1.5. Firm dynamics – Fuzzy RD estimates



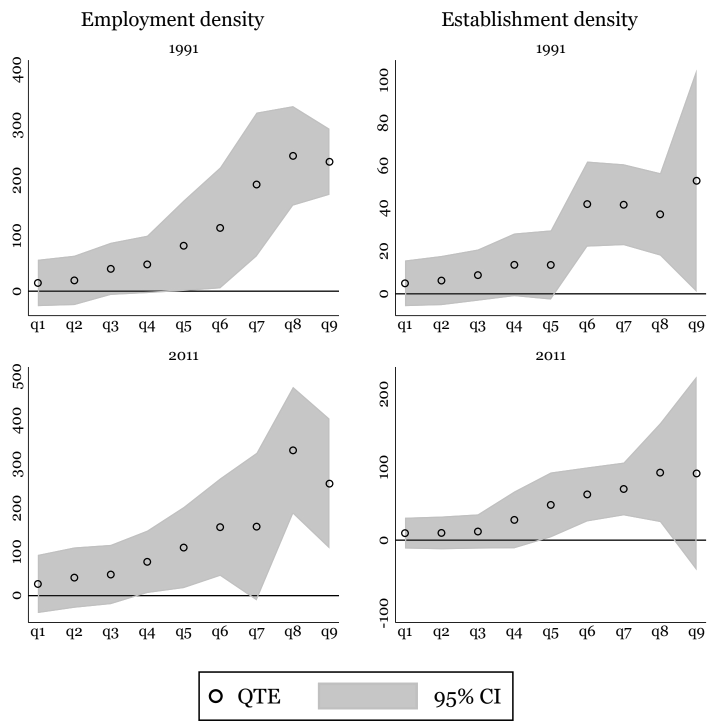
Coefficient estimates for the fuzzy RD model of Equations 1.1a and 1.1b. The shaded areas denote 95 percent confidence intervals. The vertical line marks the end of the EIM. Firm birth and death rates computed as the cumulative number of firm births and deaths every year since 1990, as a share of the total number of firms in the municipality in 1990. See text for details.

Appendix Table D1.8. Other outcomes – Fuzzy RD estimates

	Housing value	Rents	Tax income	Gini coeff.	Krugman Index
RD Estimate	543.97	2.01	0.33	0.03	-0.20
	(214.44)**	(0.88)**	(0.09)***	(0.01)***	(0.10)**
Mean around the border	1087.09	3.94	8.95	0.38	0.97
Standard deviation	580.83	1.97	0.23	0.03	0.32
Observations	574	537	587	587	586

Replication of Table 1.3, Column (2). “Housing value” and “Rents” are residential real estate prices and rents as of Q1-2011, measured in € / squared meter. "Tax income" denote (log) tax income in € / capita in 2010. "Gini coeff." is the Gini coefficient as of 2011. "Krugman Index" is the Krugman Specialization Index for manufacturing in 2011 (see Appendix 1.A.2). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure D1.6. Quantile treatment effects



Quantile treatment effects for the baseline fuzzy RD estimate. The estimators are described in Frandsen et al. (2012). The propensity score is calculated using a gaussian kernel and running 100 distribution regressions. See text for details.

Appendix Table D1.9. Municipal expenditure – Fuzzy RD estimates

a)	Total	Admin.	Educ.	Viabil.	Territ.
RD Estimate	-0.10	-0.06	-0.25	-0.11	-0.02
	(0.12)	(0.14)	(0.14)*	(0.21)	(0.16)
Mean around the border	9.43	8.18	6.84	7.21	8.09
Standard deviation	0.41	0.39	0.43	0.65	0.58
Observations	587	587	587	587	587
b)	Social	Just. & pol.	Cult. & sport	L. 488/1992	EU Funds
RD Estimate	0.11	0.21	-0.19	0.91	0.15
	(0.16)	(0.20)	(0.22)	(1.24)	(0.30)
Mean around the border	6.90	6.15	6.37	4.45	6.46
Standard deviation	0.54	0.41	0.75	4.34	1.24
Observations	587	587	587	587	544

Replication of Table 1.3, Column (2). Outcomes in Panel a) and the first three columns of Panel b) are cumulative municipality expenditures between 2000 and 2011, sourced from municipality balance sheets. All items include both current and capital expenditure. "L. 488/1992" measures the total funds obtained through Law 488/1992. "EU Funds" are total funds received through the EU Structural Funds program between 2007 and 2013. All variables are expressed in natural logarithm of the per capita amount in € (using the 2001 population). See text for details. * p<0.10, ** p<0.05, *** p<0.01

1.E Appendix E

This Appendix provides more details on the calculations performed in Section 1.7.

Cost per job. To obtain a first measure of cost per job, we consider the estimates of Table 1.3 Column (3). For 2011, we estimate that an increase in EIM funding of €1000 (2011 prices) per 1951 resident leads to 10.3 more workers per km^2. For municipalities in the estimation sample, the average 1951 population is 11,328.91 inhabitants and the average extension is 60.88 km^2. These numbers imply that, for the average municipality, total EIM funding of €11,328,910 leads to 630 more jobs – an estimated cost per job of €17,989, or \$25,048 using an exchange rate of 1.3924 (2011 average). The estimate rises to \$37,571 assuming a deadweight loss of taxation of 50 percent.

As alternative, we use the (arguably more robust) Diff-in-Disc estimates to inform our calculations of the cost per job. We do so by taking the last point estimate from the event study regressions in *i*) the baseline Diff-in-Disc specification (Figure 1.6 Panel (a): 53.64 workers per km^2), *ii*) the design using municipalities bordering provincial capitals in the Center-North as controls (Figure C1.2.9 Panel (a): 115.44 workers per km^2) and *iii*) the triple differences (Figure C1.2.12 Panel (a): 51.20 workers per km^2). For each of the three designs, we take the average extension of municipalities in the estimation sample (57.43, 67.33 and 53.16 km^2, respectively) and obtain the total number of jobs created in the average municipality by multiplying the coefficients by the average area: 3080 for design *i*), 7772 for design *ii*) and 2722 for design *iii*).

To compute the costs, designs *i*) and *iii*) require an estimate of the jump in EIM funding at the minimum IDA border, which is provided in Table 1.2 Column (2). To retain consistency with the Diff-in-Disc designs, we re-estimate the discontinuity in EIM funding on a sample that excludes IDA centers. This yields an effect of €5,797 per 1951 resident, which is very similar to the €5,720 jump reported in Table 1.2 Column (2) for the full sample. For design *ii*), which compares municipalities bordering IDA centers to those bordering provincial capitals in the Center-North, we simply take the average EIM funding for the former group (€11,520 per 1951 resident). We then multiply these average cost measures by the average 1951 population in the estimation sample (8287.16, 9900.70 and 7650.64) to obtain total EIM funding in the average municipality: €48,040,678 for design *i*), €114,058,387 for design *ii*) and €44,350,743 for design *iii*). Putting everything together, we estimate a cost per job of €15,596 ($21,716) for design *i*), €14,675 ($20,433) for design *ii*) and €16,294 ($22,687) for design *iii*). Assuming a 50 percent deadweight loss, the final estimates of the cost per jobs are similar to the baseline ones: $32,575 for design *i*), $30,650 for design *ii*) and $34,031 for design *iii*).

Cost-benefit analysis. We now describe the cost-benefit analysis based on our reduced-form estimates, which builds on the study of US Empowerment Zones in Busso et al. (2013a).[50] The goal is to estimate the gains entailed by IDAs and to compare them with the total costs of the policy to assess its cost-effectiveness. In our exercise, we focus exclusively on the benefits generated by the policy *after* its termination, and assess whether any persistent effect we estimate is enough to cover the (very large) costs. We break down total surplus into three components: wage gains for workers, corporate profits for firms and rental gains for landlords.[51] For each of these components, we compute the flow each year between 1991 and 2011. Specifically:

1. Wage bill: we use firm-level information on average monthly wages, available for the universe of Italian firms in the Bank of Italy - INPS social security archives. These are multiplied by twelve to obtain annual values and then by the firm's total employment each year to compute the total wage bill.

[50] Other applications are Chaurey (2017) for India, Lu et al. (2019) for China and Lapoint and Sakabe (2022) for Japan.

[51] None of these variables are available during the policy years, which leads us to concentrate on long-run effects. We are also unable to distinguish between benefits for IDA residents and non-resident commuters, as done in Busso et al. (2013a). That said, our focus on long-term gains makes this distinction less meaningful as we have documented no migration and commuting patterns after the end of IDAs.

2. Corporate profits: income statements sourced from Cerved are available only for incorporated firms. In addition, the Cerved data start in 1995 and coverage is not very large until the 2000s. For these reasons, we impute firm profits for all incorporated firms using the fitted value of a regression of firm profits on total wages and employment, controlling for year and province dummies. This procedure automatically sets to zero profits of all non-incorporated firms, thus underestimating total profits in a municipality.[52]

3. Housing rents: estimating rental gains for landowners is challenging as we have data on house prices and rents only for 2004 and 2011. We use information of rental prices in € /squared meter in a municipality, which we then multiply by the total building area in the municipality to obtain the flow.[53] We compute annual flows in 2004 and 2011, which we then linearly interpolate for the other years.

We then compute the effect of the policy on each of these outcomes in the post-IDA years ($\hat{\pi}_j$). For the wage bill and firm profits, we run a cross-sectional specification of Equation 1.1b at the minimum IDA border on the pooled sample of years between 1991 and 2011, controlling for year effects. This regression produces a unique (reduced-form) estimate of the effect of IDAs after their termination. Estimating the coefficient year by year and then averaging the effect across years delivers almost identical results. For housing rents, we estimate Equation 1.1b separately for 2004 and 2011 and then compute the simple average of the two coefficients. Table E1.1 shows the estimation output.

These estimates are used to calculate the counterfactual flow for each outcome j and year y as $counterfactual_{jy} = observed_{jy}/(1 + \hat{\pi}_j)$. In turn, the net benefit is the difference between the observed and counterfactual amount. These net benefits

[52]Firms in the Cerved data cover just about 30 percent of the total number of firms in Italy. These are however the largest firms and likely account for the lion's share of aggregate profits.

[53]We approximate the building area of a municipality as 1.3 percent of the total area. This estimate is produced by the Italian Tax Office, which calculates a total gross floor area of dwellings of roughly four billion squared meters (1.3 percent of Italy's surface). This share is most likely larger in our setting as we focus on urban centers, meaning that the rental gains we estimate are a lower bound of the true value.

are then aggregated over time using a discount rate of 10 percent to obtain the present discounted value of IDA gains. This rate, chosen to roughly mirror the one-year rate on Italian treasury bonds in the early 1990s, is admittedly high. The estimated net benefits would increase with smaller discount rates of, say, 3 percent (Lu et al., 2019) or 5-7 percent (Lapoint and Sakabe, 2022). Table E1.2 shows the final calculations. The benefits generated by IDAs between 1991 and 2011 are estimated at €196 billion, 60 percent of which in the form of higher wage bill. The share of firm profits is smaller at 38 percent, and that of housing rents is almost negligible. The present discounted value of the total IDA benefits hovers just below €86 billion. Compared with total funding in IDA municipalities of €88 billion, this implies that the gains generated in the two decades after the end of transfers are enough to cover the total costs of the policy.

This analysis comes with some caveats. On the one hand, the total costs of the IDA policy are likely larger than €88 billion as they also include expenses directly borne by the consortium, which are not reported in the ASET data. On the other hand, however, our estimates of the gains are quite conservative. As noted, the true effects on firm profits and housing rents are underestimated since *i*) we only consider profits of incorporated firms and *ii*) we make very conservative assumptions on the building area of a municipality. In addition, we do not account for the gains in housing valuations, which are another important effect of the policy as showed in Table D1.8. In log terms, we estimate a positive effect of 18 percent on house prices in 2011. This results in further €10 billion accruing to landlords, which do not feature for in our baseline calculations. All considered, our conclusion that the gains of IDAs in the two decades after their end at least compensate for the total cost of the policy seems fairly robust. In turn, this suggests that the program entailed a net surplus assuming that it generated gains while it was in place.

Appendix Table E1.1. Coefficient estimates ($\hat{\pi}_j$) for the cost-benefit analysis

	(Log) Wage bill	(Log) Firm profits	(Log) Rents	
			2004	2011
RD Estimate	0.70	0.97	0.18	0.19
	(0.33)**	(0.37)***	(0.05)***	(0.06)***
Observations	12,282	8,573	535	537

For wage bill and firm profits, we estimate Equation 1.1b on the pooled sample of years 1991-2011 and control for year effects. For rents, we run Equation 1.1b separately for 2004 and 2011. Standard errors clustered by IDA region in parentheses. See text for details. * p<0.10, ** p<0.05, *** p<0.01

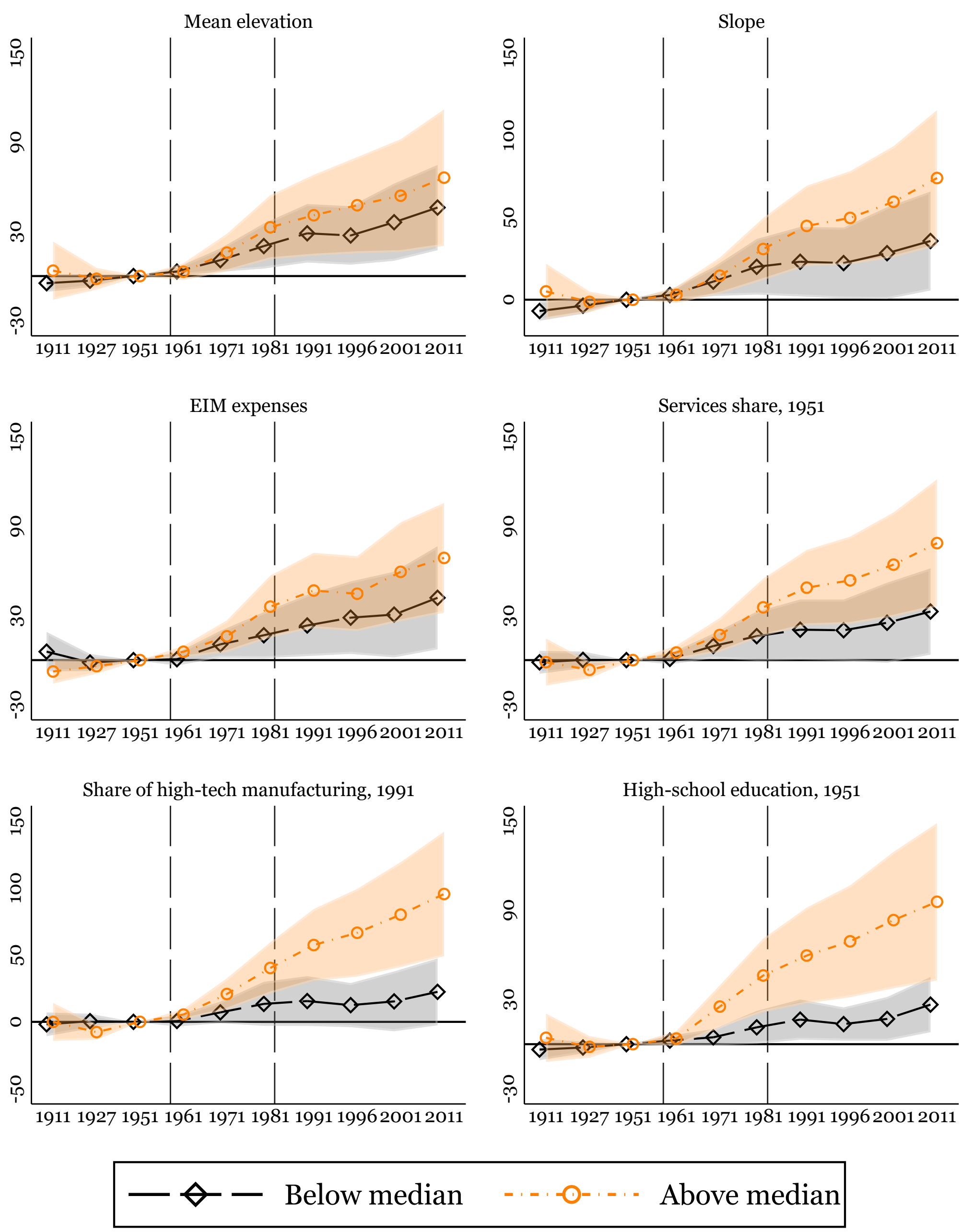
Appendix Table E1.2. Benefits of the IDA policy

	Observed (€bn)	$\hat{\pi}_j$	Counterf. (€bn)	Benefit (€bn)	PDV benefits (€bn)
Wage bill	237.16	0.70	118.07	119.09	52.06
Firm profits	118.68	0.97	44.80	73.88	32.66
Housing rents	20.63	0.19	17.12	3.50	1.21
Total	376.46		179.99	196.47	85.93

All amounts are cumulated between 1991 and 2011 and measured in billion € (2011 prices). The counterfactual amount is obtained as $counterfactual_j = observed_j/(1+\hat{\pi}_j)$. We transform the coefficient using $(e^{\hat{\pi}_j}-1)$. The presented discounted value is calculated using a 10% discount rate. The effect of the policy $\hat{\pi}_j$ is estimated using the reduced-form specification in Equation 1.1b. For firm profits, the actual flows refer only to incorporated firms in the Cerved data. See text for details.

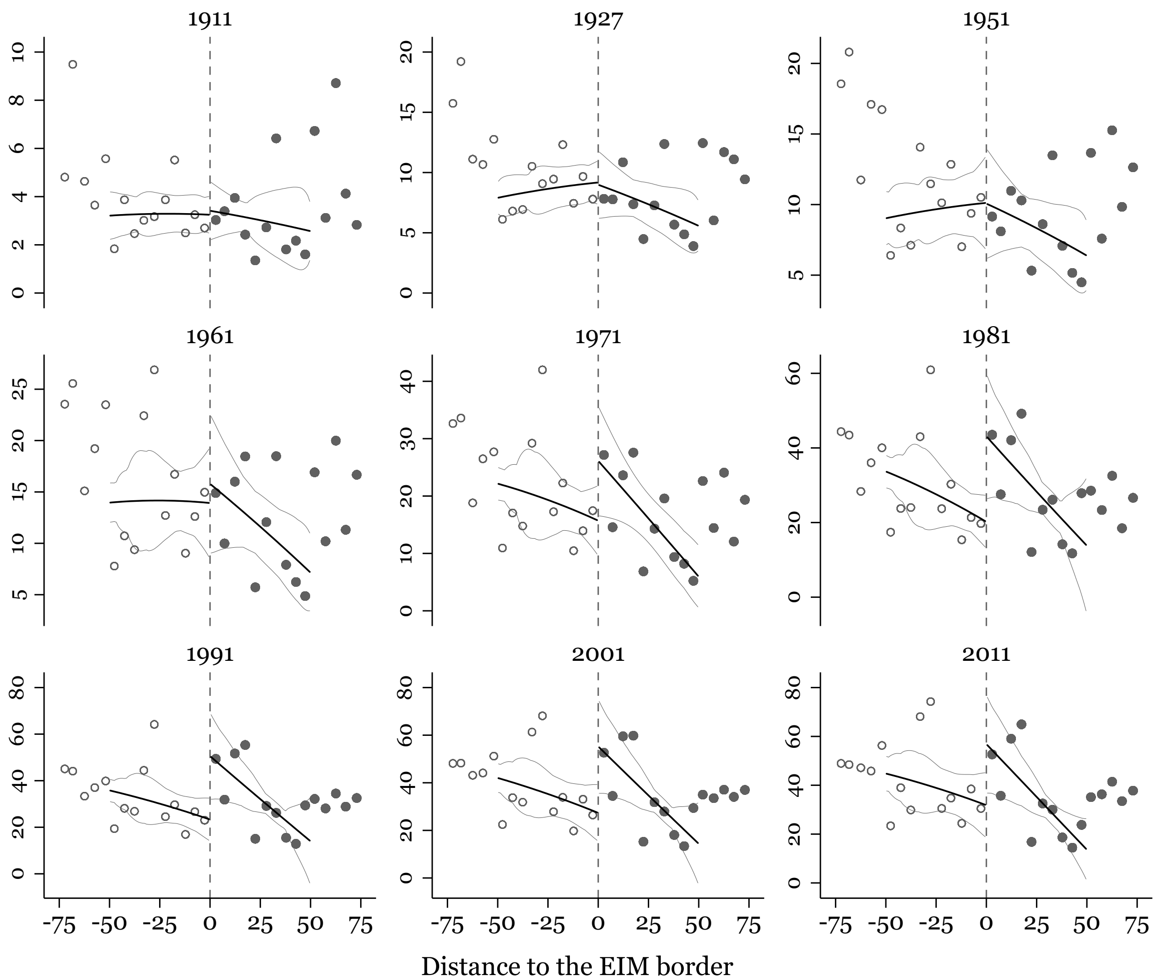
1.F Appendix F

Appendix Figure F1.1. Employment density – Heterogeneity



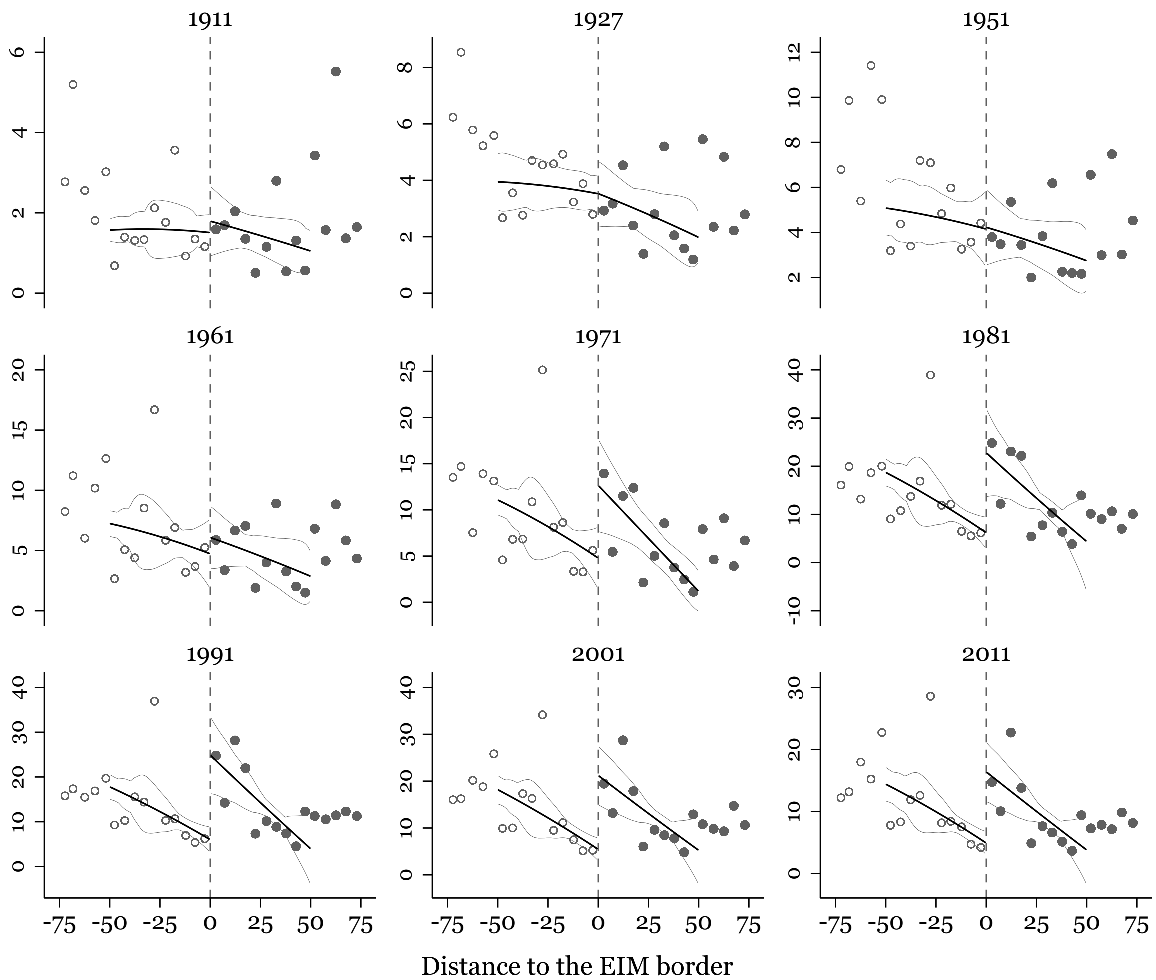
Coefficient estimates for Equation 1.2. EIM expenses measured in euros (2011 prices) per 1951 inhabitant, cumulated between 1950 and 1992. For each of the six variables, we compute the mean within each IDA region using only municipalities bordering the IDA center. Share of high-technology manufacturing computed according to the Eurostat/OECD classification, using administrative data on the universe of firms. For each variable we compute the median across IDA regions. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. See text for details.

Appendix Figure F1.2. Employment density



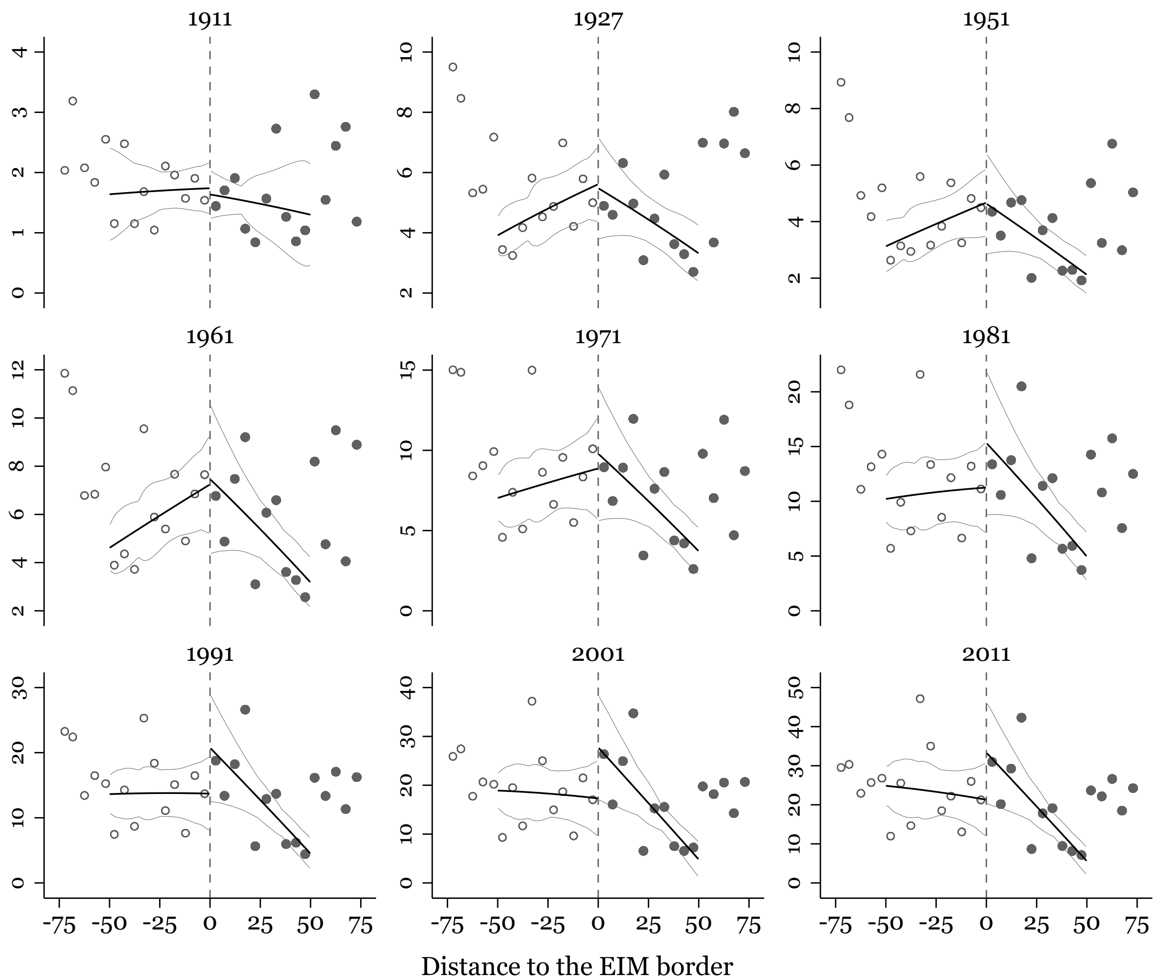
Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure F1.3. Manufacturing employment density



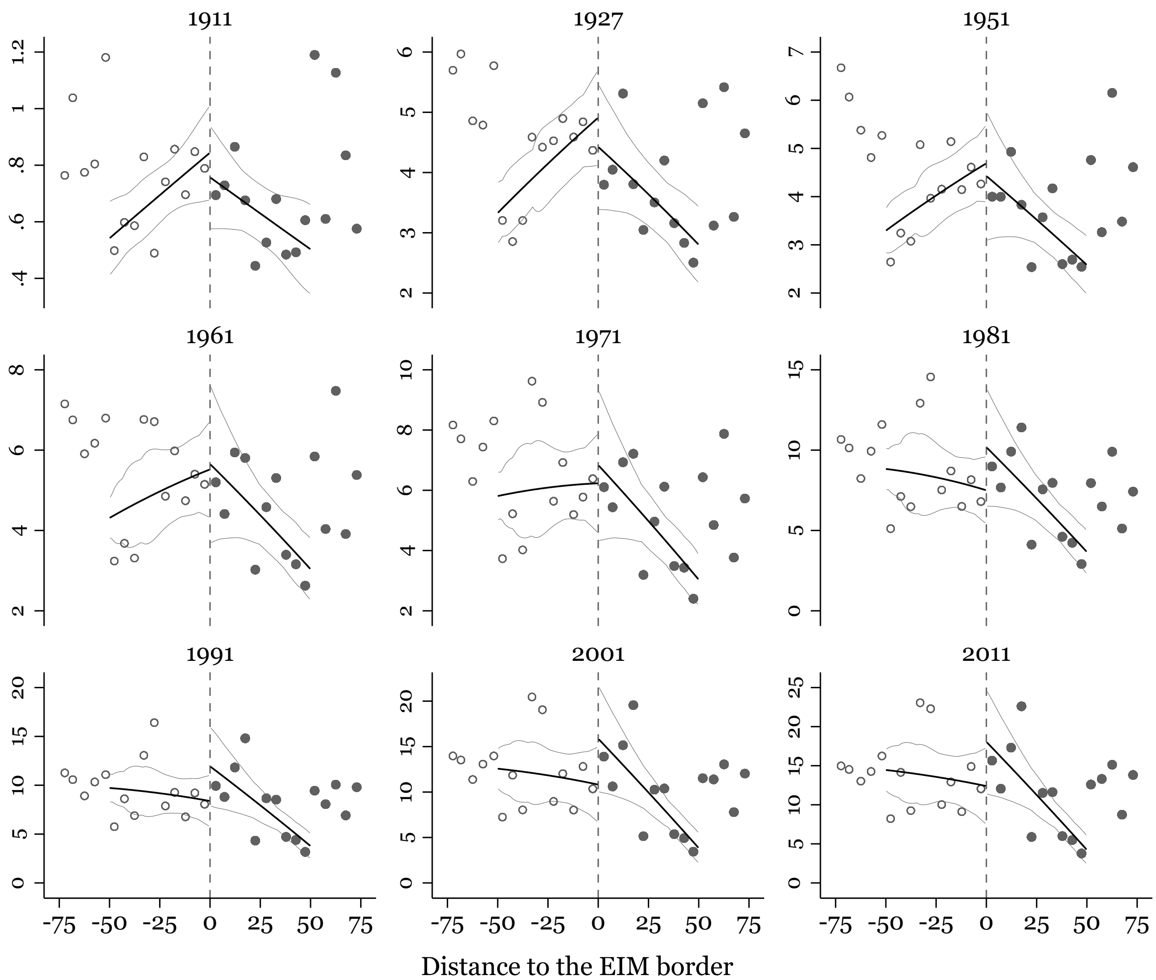
Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure F1.4. Services employment density



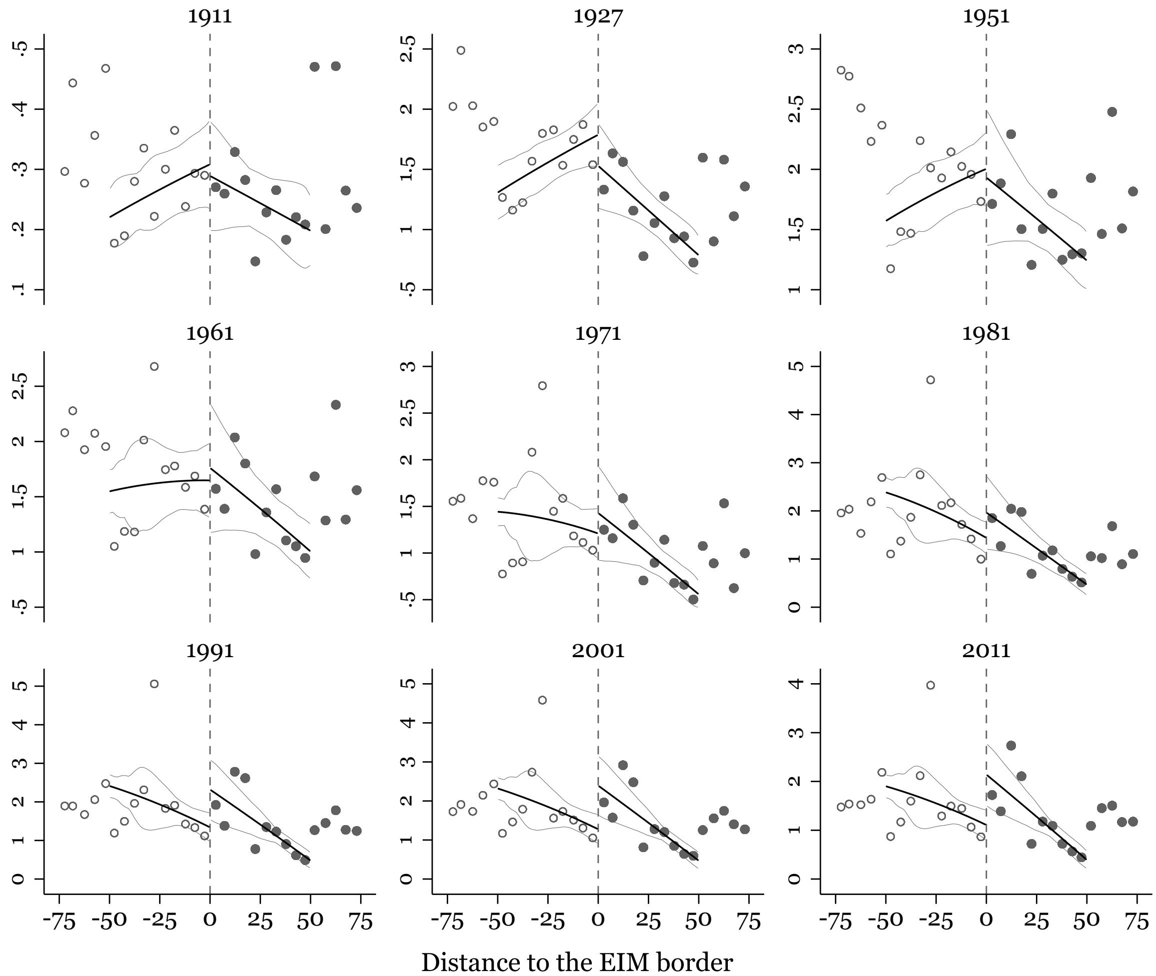
Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure F1.5. Establishment density



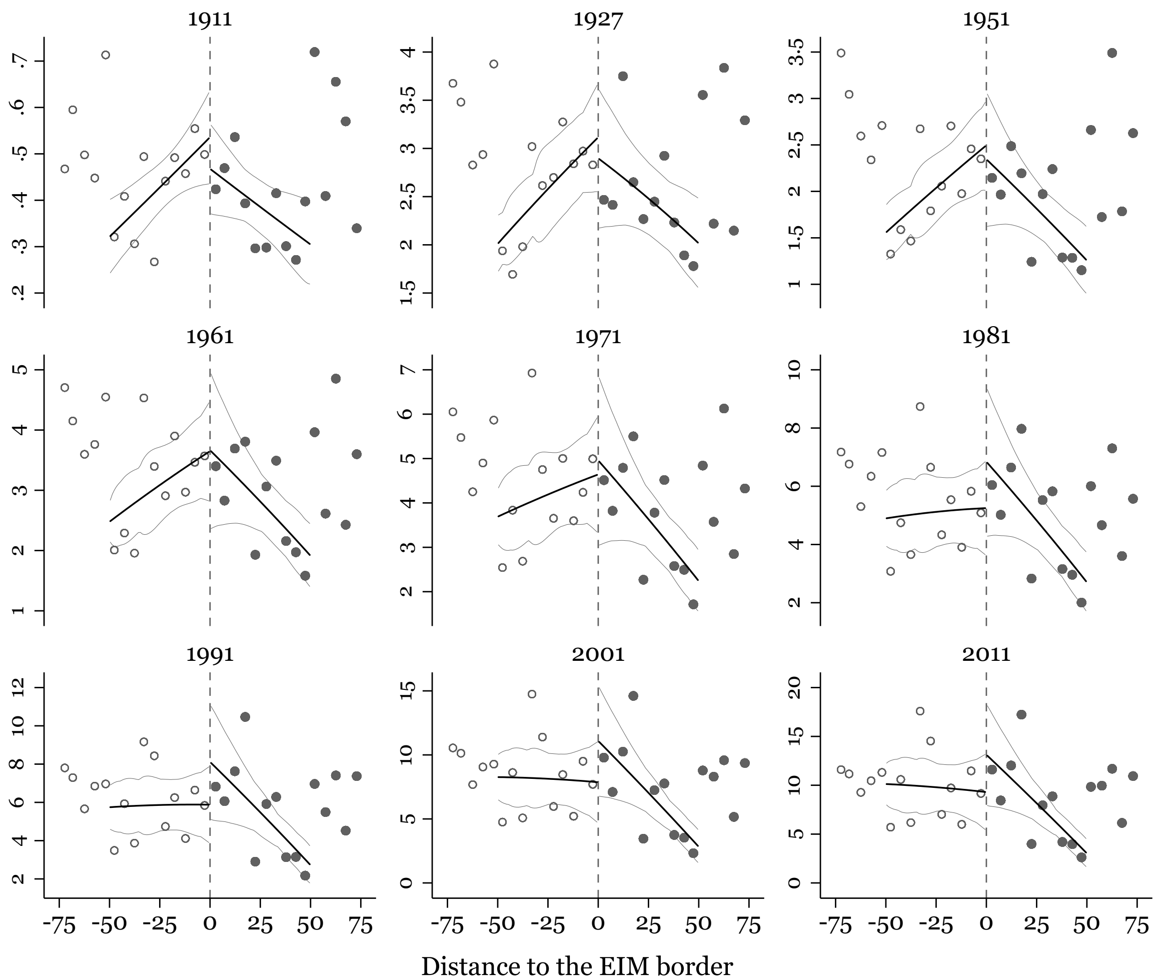
Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure F1.6. Manufacturing establishment density



Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Figure F1.7. Services establishment density



Negative distance denotes municipalities north of the EIM border. The dots are binned means of the outcome computed within disjoint, evenly-spaced 5-km bins of the running variable. The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border using a 50-km symmetric bandwidth. The gray lines are 95 percent confidence intervals. See text for details.

Appendix Table F1.1. RD estimates – EIM border

	Empl., 1991	Empl., 2011	Est., 1991	Est., 2011
RD Estimate	18.59	14.95	1.94	2.77
	(9.93)*	(11.72)	(2.40)	(4.09)
Mean around the border	30.78	37.09	8.64	12.59
Standard deviation	61.14	71.38	14.74	24.01
Observations	587	587	587	587
R^2	0.29	0.30	0.34	0.29

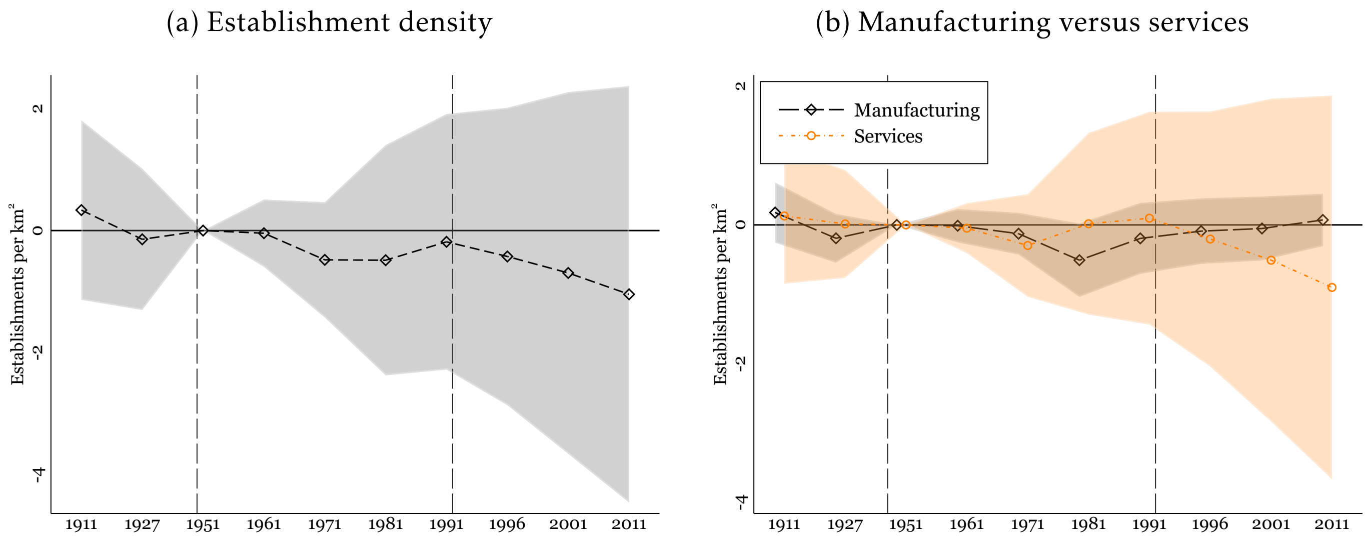
Coefficient estimates from Equation B4.1 separately for employment density and establishment density. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table F1.2. Manufacturing and services densities – EIM border

	Employment density		Establishment density	
	Manufacturing	Services	Manufacturing	Services
	Contemporaneous effect (1991)			
RD Estimate	15.36	3.44	0.71	1.03
	(4.02)***	(5.01)	(0.42)	(1.81)
Mean around the border	12.77	13.53	1.66	5.76
Standard deviation	28.13	28.45	3.22	10.48
Observations	587	587	587	587
	Persistent effect (2011)			
RD Estimate	9.26	6.04	0.77	1.56
	(2.61)***	(7.86)	(0.35)**	(3.25)
Mean around the border	9.61	21.79	1.40	9.14
Standard deviation	19.60	46.82	2.61	18.81
Observations	587	587	587	587

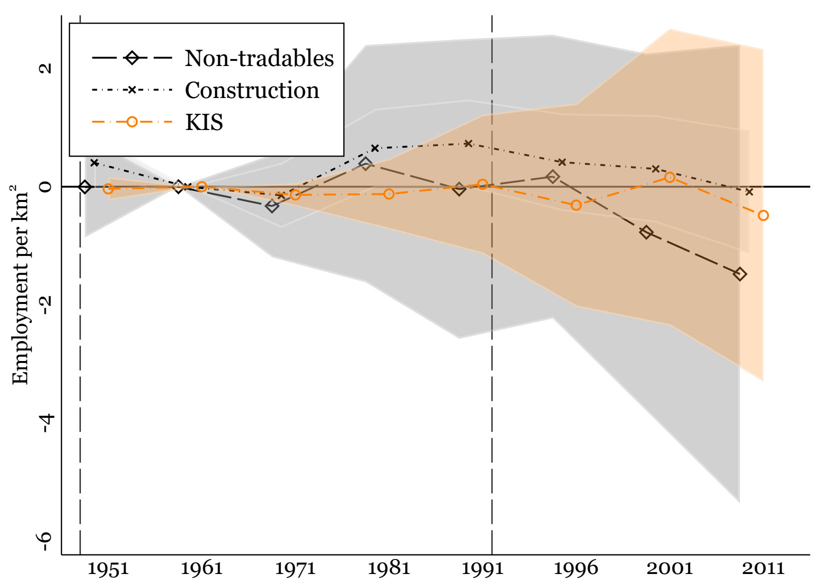
Coefficient estimates from Equation B4.1 separately for employment density and establishment density. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure F1.8. The EIM border – Difference-in-discontinuities



Coefficient estimates for Equation B4.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the EIM. See text for details.

Appendix Figure F1.9. The EIM border – Employment density, sectoral breakdown



Coefficient estimates for Equation B4.2. Standard errors clustered at the municipality level. The shaded areas denote 95 percent confidence intervals. The dashed vertical lines mark the beginning and the end of the EIM. "Non-tradables" include wholesale and retail trade, hotels and restaurants and other. KIS include communication, finance and insurance and services to firms. See text for details.

Appendix Table F1.3. Employment and firm shares in services – EIM border

	Employment		Establishments	
	KIS	Other serv.	KIS	Other serv.
	Contemporaneous effect (1991)			
RD Estimate	-0.02	0.02	-0.01	0.01
	(0.03)	(0.03)	(0.02)	(0.02)
Mean around the border	0.13	0.87	0.11	0.89
Standard deviation	0.20	0.20	0.14	0.14
Observations	526	526	526	526
	Persistent effect (2011)			
RD Estimate	0.00	-0.00	0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)
Mean around the border	0.09	0.91	0.09	0.91
Standard deviation	0.13	0.13	0.09	0.09
Observations	570	570	570	570

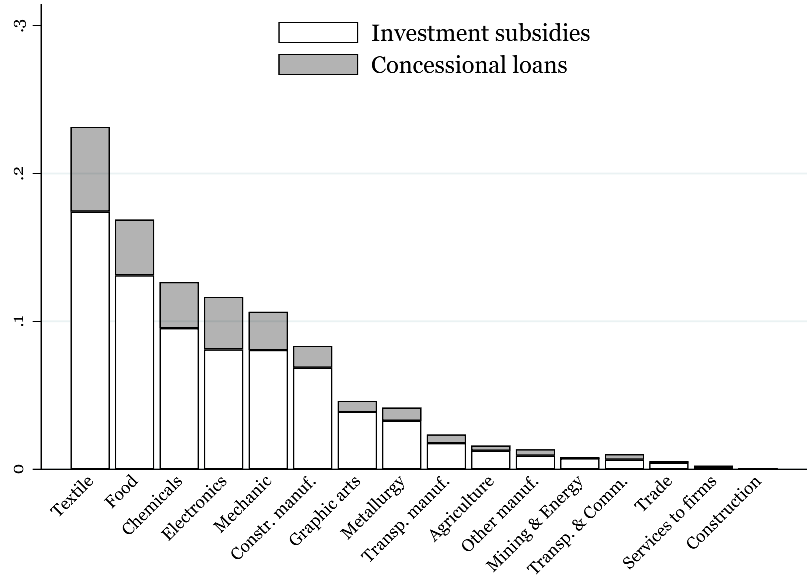
Coefficient estimates from Equation B4.1. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). The outcomes are the share of employment and establishments in KIS and other services. The shares are obtained from social security data on the universe of Italian firms and the KIS classification is obtained from Eurostat/OECD. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table F1.4. Employment and firm shares in manufacturing – EIM border

	Employment, 1991		Establishments, 1991	
	High-tech	Low-tech	High-tech	Low-tech
RD Estimate	0.02	-0.02	-0.00	0.00
	(0.03)	(0.03)	(0.03)	(0.03)
Mean around the border	0.14	0.86	0.13	0.87
Standard deviation	0.21	0.21	0.15	0.15
Observations	509	509	509	509

Coefficient estimates from Equation B4.1. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). The outcomes are the share of employment across manufacturing sub-sectors, grouped by technological intensity. The shares are obtained from social security data on the universe of Italian firms and the technology classification is obtained from Eurostat/OECD. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure F1.10. The EIM border – Subdsidies to firms, breakdown



Sector breakdown of firm investment subsidies and concessional loans. Sample includes municipalities up to 50 km south of the EIM border.

Appendix Table F1.5. (Log) wages – EIM border

	Total	By sector		Within services	
		Manufacturing	Services	KIS	Other serv.
		Contemporaneous effect (1991)			
RD Estimate	0.15	0.19	0.16	0.08	0.15
	(0.02)***	(0.04)***	(0.04)***	(0.10)	(0.04)***
Mean around the border	7.11	7.12	7.09	7.08	7.10
Standard deviation	0.17	0.25	0.29	0.47	0.24
Observations	580	509	526	331	519
		Persistent effect (2011)			
RD Estimate	0.04	0.04	0.06	0.09	0.06
	(0.03)	(0.05)	(0.04)	(0.09)	(0.04)
Mean around the border	7.08	7.12	6.93	7.05	6.91
Standard deviation	0.18	0.26	0.28	0.52	0.28
Observations	584	514	570	387	569

Coefficient estimates from Equation B4.1. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). Outcome computed as the natural logarithm of the average monthly wage paid by the firm, then averaged across firms in a municipality. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table F1.6. Education and occupations – EIM border

	High school educ.	Univ. degree	Low-skill	High-skill
	Contemporaneous effect (1991)			
RD Estimate	-0.18	-0.28	-0.39	-1.55
	(0.74)	(0.51)	(0.62)	(0.83)*
Mean around the border	16.87	5.65	10.96	17.32
Standard deviation	5.18	3.73	4.72	5.91
Observations	585	585	585	585
	Persistent effect (2011)			
RD Estimate	-0.34	0.01	0.71	-1.66
	(0.86)	(1.01)	(0.75)	(0.81)**
Mean around the border	38.19	20.65	18.83	24.74
Standard deviation	6.20	7.51	4.92	5.55
Observations	587	587	587	587

Coefficient estimates from Equation B4.1. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). "High school educ." is the share of people aged at least 6 with high school education or more. "Univ. degree" is the ratio of the resident population aged 30-34 years old with a university degree to the resident population aged 30-34 years old. "Low-skill" is the employment share of those in low-skill jobs (unskilled occupations - Isco08 code 8). "High-skill" is the employment share of those in high-skill jobs (Legislators, Entrepreneurs, High Executives, Scientific and Highly Specialized Intellectual Professions, Technical Professions - Isco08 codes 1, 2 and 3). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table F1.7. Firm size and wage distribution – EIM border

	Firm size			Firm wage		
	T1	T2	T3	T1	T2	T3
	Contemporaneous effect (1991)					
RD Estimate	-0.11	0.01	0.10	-0.19	0.07	0.11
	(0.03)***	(0.02)	(0.02)***	(0.03)***	(0.02)***	(0.03)***
Mean around the border	0.42	0.33	0.25	0.36	0.32	0.32
Standard deviation	0.18	0.17	0.15	0.20	0.15	0.18
Observations	580	580	580	580	580	580
	Persistent effect (2011)					
RD Estimate	-0.07	0.02	0.05	-0.03	0.01	0.03
	(0.02)***	(0.02)	(0.02)***	(0.02)	(0.02)	(0.02)
Mean around the border	0.42	0.32	0.25	0.36	0.30	0.34
Standard deviation	0.16	0.13	0.13	0.15	0.13	0.14
Observations	584	584	584	584	584	584

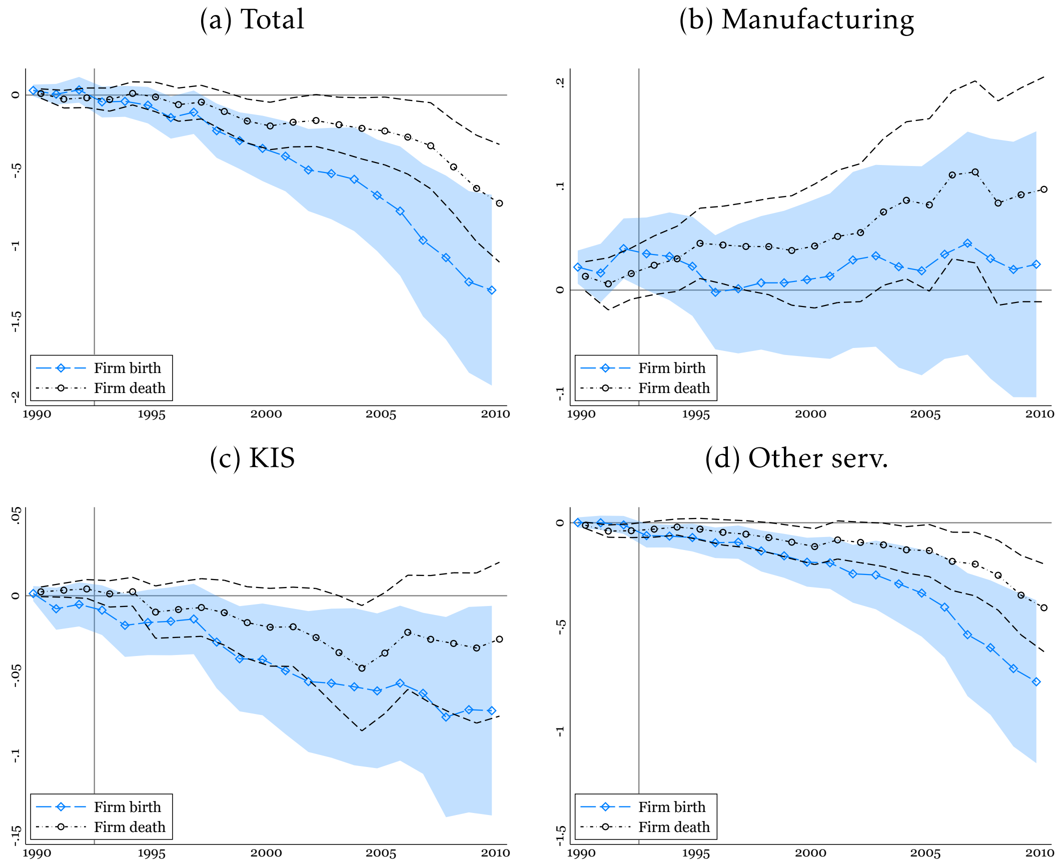
Coefficient estimates from Equation B4.1. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). Outcomes are computed as the share of firms in each tertile of the distribution of firm size and wage paid. Tertiles are derived on the universe of the Italian firms each year. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table F1.8. Balance sheet outcomes, 2011 – EIM border

	Total	By sector		Within services	
		Manufacturing	Services	KIS	Other serv.
			Value added		
RD Estimate	0.50	0.39	0.27	0.21	0.31
	(0.15)***	(0.19)**	(0.19)	(0.25)	(0.20)
Mean around the border	4.38	4.28	4.11	3.94	4.13
Standard deviation	1.00	1.10	1.19	0.99	1.23
Observations	542	417	497	278	484
			Investment		
RD Estimate	0.85	0.50	0.79	0.47	0.81
	(0.21)***	(0.25)*	(0.25)***	(0.38)	(0.25)***
Mean around the border	2.66	2.48	2.41	2.00	2.41
Standard deviation	1.35	1.48	1.51	1.58	1.53
Observations	542	418	496	270	487
			Sales		
RD Estimate	0.74	0.35	0.49	0.37	0.48
	(0.17)***	(0.21)*	(0.20)**	(0.29)	(0.21)**
Mean around the border	5.89	5.71	5.79	5.01	5.86
Standard deviation	1.11	1.19	1.28	1.23	1.30
Observations	548	425	507	287	496
			Profits		
RD Estimate	0.93	0.28	0.09	-0.02	0.21
	(0.31)***	(0.39)	(0.36)	(0.42)	(0.37)
Mean around the border	2.21	2.27	2.18	1.80	2.21
Standard deviation	1.65	1.79	1.68	1.45	1.73
Observations	334	247	275	173	271

Coefficient estimates from Equation B4.1. All regressions are estimated over a 50-km symmetric bandwidth around the EIM border and control for a linear polynomial in the distance to the border and border segment effects. Standard errors allow for spatial correlation (Conley, 1999). All outcomes are as of 2011 and expressed in natural logarithm, scaled by total firm workforce. See Appendix 1.A.3 and text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Figure F1.11. Firm dynamics – EIM border



Coefficient estimates of Equation B4.1 using a symmetric 50-km bandwidth a controlling for a linear polynomial in distance to the EIM border and for border segment fixed effects. Standard errors allow for arbitrary spatial correlation (Conley, 1999). The shaded areas denote 95 percent confidence intervals. The vertical line marks the end of the EIM. Firm birth and death rates computed as the cumulative number of firm births and deaths every year since 1990, as a share of the total number of firms in the municipality in 1990. See text for details.

Appendix Table F1.9. Other outcomes – EIM border

	Housing value	Rents	Tax income	Gini coeff.	KSI
RD Estimate	-153.68	-0.57	-0.02	0.01	0.02
	(67.86)**	(0.26)**	(0.02)	(0.00)*	(0.06)
Mean around the border	1106.11	4.14	9.18	0.37	1.06
Standard deviation	511.06	2.01	0.15	0.04	0.43
Observations	584	522	586	587	586

Coefficient estimates of Equation B4.1 using a symmetric 50-km bandwidth a controlling for a linear polynomial in distance to the EIM border and for border segment fixed effects. Standard errors allow for arbitrary spatial correlation (Conley, 1999). "Housing value" and "Rents" are residential real estate prices and rents as of Q1-2011, measured in euros per squared meter. "Tax income" denote (log) tax income in euros per capita in 2010. "Gini coeff." is the Gini coefficient as of 2011. "KSI" is the Krugman Specialization Index for manufacturing in 2011 (see Appendix 1.A.2). See text for details. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table F1.10. The IDAs versus the EIM border – descriptive statistics

	IDAs	EIM border
Firm subsidies	4.99	4.53
	(10.51)	(8.21)
Infrastructure spending	2.62	3.10
	(5.18)	(4.76)
Employment density (1951)	19.01	7.47
	(23.09)	(14.31)
Establishment density (1951)	8.33	3.43
	(8.55)	(5.11)
Manuf. employment density (1951)	9.47	3.10
	(13.76)	(6.19)
Manuf. establishment density (1951)	3.44	1.64
	(3.64)	(2.25)
Share of high-tech manuf. (%, 1951)	5.11	5.21
	(5.50)	(2.94)
Population density (1951)	307.76	111.81
	(318.29)	(104.39)
Agriculture share (%, 1951)	31.28	34.49
	(13.53)	(12.00)
High school education (%, 1951)	2.17	1.84
	(1.20)	(0.88)
Mean elevation	188.38	728.24
	(153.53)	(440.26)
Slope	417.26	947.85
	(460.47)	(572.53)
Seismicity	2.80	1.66
	(0.91)	(0.72)
Number of municipalities	95	168

Column (1) restricts the sample to municipalities bordering IDA centers and Column (2) to municipalities 50 km south of the EIM border. The sample excludes municipalities 50 km south of the EIM border that belong to IDAs. Firm subsidies and infrastructure spending measured in thousand 2011 euros per 1951 resident, winsorized at 1 and 99 percent. Employment and establishments (total and manufacturing) are sourced from the 1951 industrial census. "Share of high-tech manuf." is the share of manufacturing workers employed in chemical and mechanics in 1951. "Agriculture share" computed as the number of agriculture workers per 100 residents aged at least 15. “High school education” denotes the share of people aged at least 6 with high school education or more. "Mean elevation" and "Slope" measured in meters. "Seismicity" is a categorical variable ranging from 1 "High seismicity" to 4 "Very low seismicity". Standard deviations in parentheses.

1.G Appendix G

A common drawback of RD designs is that external validity is limited to units close to the cutoff. This issue is exacerbated further in fuzzy RD, as the LATE estimate refers to compliers only. A series of papers have emerged assessing the external validity of RD estimates for units far from the cutoff (Angrist and Rokkanen, 2015) and, specifically for fuzzy RD, other compliance groups (Bertanha and Imbens, 2020). We briefly analyze both cases in this Appendix.

Extrapolation away from the cutoff. Do the positive effects of PBIP still apply away from the IDA centers? Angrist and Rokkanen (2015) devise a method to extrapolate RD treatment effects to inframarginal units, leveraging the availability of additional predictors of the outcome other than the running variable. Conditional on a vector of these covariates (henceforth, "CIA covariates"), there is mean independence between the outcome and the running variable – a Conditional Independence Assumption (CIA). To obtain the CIA covariates, we exploit the data-driven algorithm in Palomba (2023).[54] Specifically, we feed the following list of potential baseline predictors of the outcome (employment density in 2011): geographical characteristics (slope, mean elevation, coastal location, seismicity), employment and population density in 1951, manufacturing and agriculture shares in 1951 and high-school education in 1951. The algorithm selects as CIA covariates slope, mean elevation, seismicity and population density in 1951. Conditional on these, the correlation between employment density and distance to the cutoff breaks, as showed in Columns (1) and (2) of Table G1.[55] These covariates are then used to identify counterfactual values of the outcome away from the cutoff, and in turn extrapolate the RD effects. We show in Column (3) that replacing the running variable with the CIA covariates produces treatment effects at varying bandwidths away from the cutoff that are very similar to the baseline reduced-form RD esti-

[54] We use the *ciasearch* Stata command included in the *getaway* package (Palomba, 2023).

[55] This approach additionally rests on a common support assumption that assumes variation in treatment status within cells based on the selected CIA covariates (Angrist and Rokkanen, 2015).

mate of 60 workers per km^2 in 2011.

Other compliance groups. An added limitation to external validity in fuzzy RD designs is that the estimated LATE refers to complier units (in our case, municipalities that are included in an IDA if and only if they are contiguous to an IDA center). What about the effects for always-takers and never-takers? To this end, Bertanha and Imbens (2020) define external validity as "independence between potential outcomes and compliance types". If this holds, then the LATE for compliers equals that for always-takers and never-takers. They show that this condition implies exogeneity of treatment participation, which can be falsified using a joint test of restrictions. Namely, one should test equality of average outcome between always-takers and treated compliers, and never-takers and control compliers. Bertanha and Imbens (2020) propose a joint formal test of these restrictions, which we perform within the baseline 16-km bandwidth using employment density in 2011 as outcome.[56] The test delivers an F-stat of 0.226, meaning that we fail to reject equality of average outcomes across compliance types, lending support to external validity. We do not place much emphasis on this result as we lack statistical power due to the small sample size. Most importantly, testing equality between never-takers and control compliers is not feasible in our set-up due to the very low number of never-takers (there are only ten municipalities bordering IDA centers and not part of an IDA). If anything, our results at the EIM border suggest that never-taker municipalities are unlikely to benefit from PBIP in the long run – see the discussion in Section 1.8.

[56] We use the *rdexo* Stata command introduced in Bertanha and Imbens (2020).

Appendix Table G1. IDAs – External validity

	CIA		External validity
	Distance to the minimum IDA border		Employment density, 2011
Bandwidth	(1)	(2)	(3)
20 km	-1.86	-1.07	58.15
	(0.53)***	(0.28)***	(26.44)*
30 km	-1.32	-0.21	57.04
	(0.27)***	(0.15)	(26.09)*
40 km	-1.03	-0.24	59.78
	(0.16)***	(0.09)**	(27.83)*
50 km	-0.72	-0.08	59.93
	(0.11)***	(0.06)	(27.72)*
60 km	-0.55	-0.03	59.15
	(0.09)***	(0.05)	(27.20)*
70 km	-0.49	-0.04	59.05
	(0.07)***	(0.04)	(26.98)*
80 km	-0.46	-0.04	59.00
	(0.06)***	(0.04)	(27.08)*

External validity analysis based on Angrist and Rokkanen (2015). Columns (1) and (2) show the coefficient for the running variable (distance to the minimum IDA border) in a regression of the outcome (employment density in 2011) on the running variable outside of the minimum border, within the bandwidth indicated on the left. Column (2) additionally controls for slope, mean elevation, seismicity and population density in 1951. These controls, which break the correlation between the outcome and the running variable, are obtained through the *ciasearch* algorithm in Palomba (2023). Column (3) estimates Equation 1.1b within the bandwidth indicated on the left, but replaces distance to the border with the above covariates. See text for details. * p<0.10, ** p<0.05, *** p<0.01

Chapter 2

Government Transfers and Votes for State Intervention

Giuseppe Albanese, Guido de Blasio, Lorenzo Incoronato[1]

2.1 Introduction

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,[2] we study a large place-based policy, the *extraordinary*

[1]This article is forthcoming in the *American Economic Journal: Economic Policy*. Albanese: Bank of Italy; De Blasio: Bank of Italy; Incoronato: UCL.

[2]To ease exposition, throughout the text we will use the expression "voters' support for state

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 regulation", "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

intervention" as a shorthand for "voters' support for parties promoting state intervention in the economy".

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).[3] 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 re-

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

sults 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.[4] Our investigation also contributes to the stream of literature analyzing 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

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

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., 2013b; Kline and Moretti, 2014a; 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.[5] 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.

[5]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 paper is organized as follows. Section 2.2 provides a brief historical overview of the *extraordinary intervention* in the South of Italy. Section 2.3 illustrates how we construct the main outcome. Section 2.4 discusses identification and Section 2.5 presents the empirical analysis. Section 2.6 discusses the potential mechanisms behind our findings. The last section concludes.

2.2 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).[6]

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.[7] 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.

[6] 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).

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

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.[8] The cost of the program jumped from a total of around 49 billion euros (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.[9]

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

2.3.1 Measuring voters' support for 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

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

[9]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.

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"*.[10] 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 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.[11]

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 welfare 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

[10] 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.

[11] 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.

the party's performance 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.[12] 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 voters' 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 ideology and party preferences. Specifically, for each party and election year, the score associated 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", "Nationalisation" and "Welfare state expansion" (described in Table 2.1). 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 parties differentiate from each other on their pro-state positions - a proxy for the salience of state intervention topics in the political de-

[12] 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.

bate. Figure 2.1 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*.[13] 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:

$$stateint_m^t = \sum_j share_{j,m}^t \cdot manifesto_j^t \tag{2.1}$$

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.[14] 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.

2.3.2 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

[13] 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 2.1 - the only exceptions are "Nationalism", "Culture" and "Equality" - see Figure A2.1 in the Online Appendix. Figure A2.2 in the Online Appendix reproduces the salience pattern for the composite state intervention score of Figure 2.1 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.

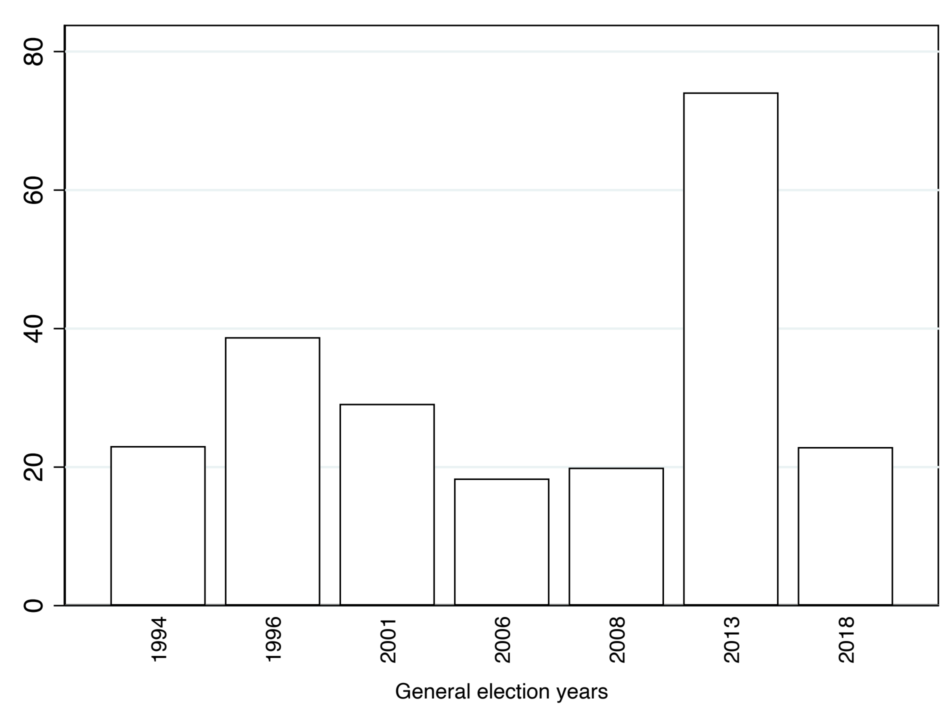
[14] We will only use the variable computed in Equation 2.1 as our outcome throughout the analysis, since single components could be affected by random errors and statistical noise in their coding (Benoit et al., 2009; Volkens et al., 2013). Online Appendix Table A2.1 reports the raw (non-standardized) value of the composite score for each party at the 2013 election. Appendix Table A2.2 shows descriptive statistics for the outcome computed in Equation 2.1 in the CasMez area in 2013.

Table 2.1. 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"

Notes: Description of the Manifesto scores used to compute the index of voters' support for state intervention in Equation 2.1. More details available in the Manifesto Project Dataset – Codebook (Version 2019b).

Figure 2.1. Salience of state intervention across election years (1994-2018)



Notes: 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 2.1. See text for details.

(firm transfer or infrastructure project), the year of approval and the total financial resources allocated.[15] 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.[16] Data on voting at all general elections between 1946 and 2018 is sourced from the Italian Ministry of Interior.[17] 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.

2.4 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.

[15]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*.

[16]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 2.4). 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 2.D.

[17]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).

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 2.2, 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 2.2, Panel (a)). This area was defined in 1950 (a time when the program was supposed to last for ten years only) and remained unchanged until the termination of the policy in 1992.[18] Panel (b) of Figure 2.2 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.[19]

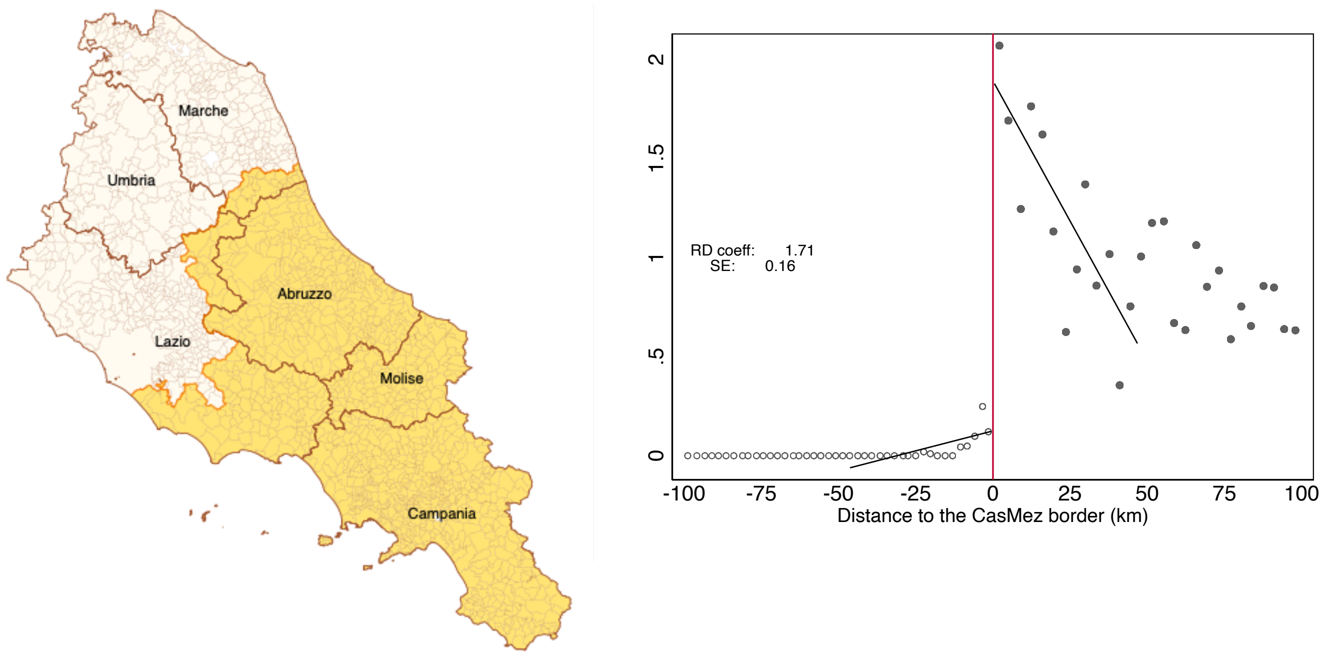
[18]Online Appendix Figure B2.1 shows the full jurisdiction of the program.

[19]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.

Figure 2.2. The CasMez border

(a) The border

(b) (Log) CasMez transfers at the border



Notes: 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. (2014b) 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 2.5 for details).

This jump in government transfers gives rise to a spatial sharp regression discontinuity (RD) design where the CasMez border $\mathscr{B}$ constitutes a two-dimensional discontinuity in latitude-longitude space that separates the treated area $\mathscr{A}^t$ from the control area $\mathscr{A}^c$. 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: $T_m = [\ell_m \in \mathscr{A}^t]$. Differently from standard RD designs, the running variable in geographic RD is two-dimensional. We collapse it to a one-dimensional metric $\delta_m \equiv d(\ell_m, \mathscr{B})$, 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 $T_m = [\ell_m \in \mathscr{A}^t] = [\delta_m \geq 0]$. 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).[20]

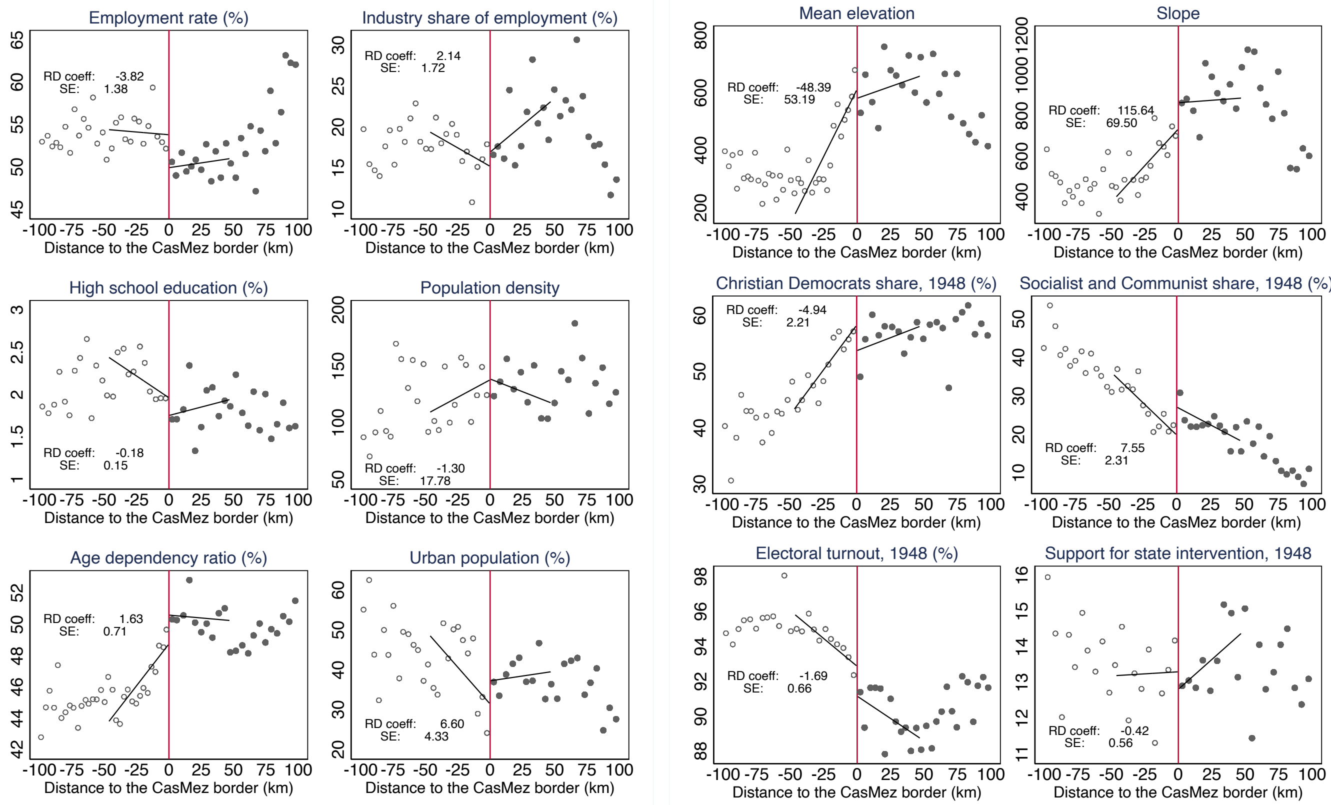
To provide a first test of the continuity assumption, we look for imbalances at the cutoff by plotting relevant observable characteristics in its vicinity. Panel (a) of Figure 2.3 shows little discontinuities in labor market and demographic outcomes at the onset of the policy in 1951. The industry share 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 2.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.[21]

[20]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 B2.2). The policy might have induced sorting of individuals at the border, which is discussed below as one of the possible mechanisms driving the results.

[21]We show RD plots for additional variables in Online Appendix Figure B2.3.

Figure 2.3. CasMez border - balancing

(a) Labor markets and demography, 1951 (b) Geography and electoral outcomes



Notes: 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. (2014b) 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 2.5 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 2.1 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.[22] Im-

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

portantly, at the time the border was set, the *extraordinary intervention* had a well-defined lifespan of only ten 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.[23] 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 2.2) 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).

two parts 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).

[23]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 2.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.

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 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.

2.5 Estimation and results

2.5.1 Main 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) (Model 2.2) or separately for each election, with a focus on the 2013 vote (Model 2.3):

$$stateint^{t}_{m,p,b} = \alpha + \beta \cdot T_{m,p,b} + \varphi(\delta_{m,p,b}) + \gamma_p + \theta_b + \tau_t + \epsilon_{m,p,b,t} \qquad (2.2)$$

$$stateint^{2013}_{m,p,b} = \alpha + \beta \cdot T_{m,p,b} + \varphi(\delta_{m,p,b}) + \gamma_p + \theta_b + \epsilon_{m,p,b} \qquad (2.3)$$

Where $stateint^{t}_{m,p,b}$ captures voters' support for state intervention in election year t as measured in Equation 2.1 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.[24] Equation 2.2 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. (2014b) to Equation 2.2, estimated on the pooled sample of all general elections between 1946 and 2018.[25] 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.[26]

Figure 2.4 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.[27]

We quantify these discontinuities in Table 2.2. We begin by showing regression estimates of the β coefficient for the pooled sample of elections after the end

[24]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.

[25]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.

[26]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 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.

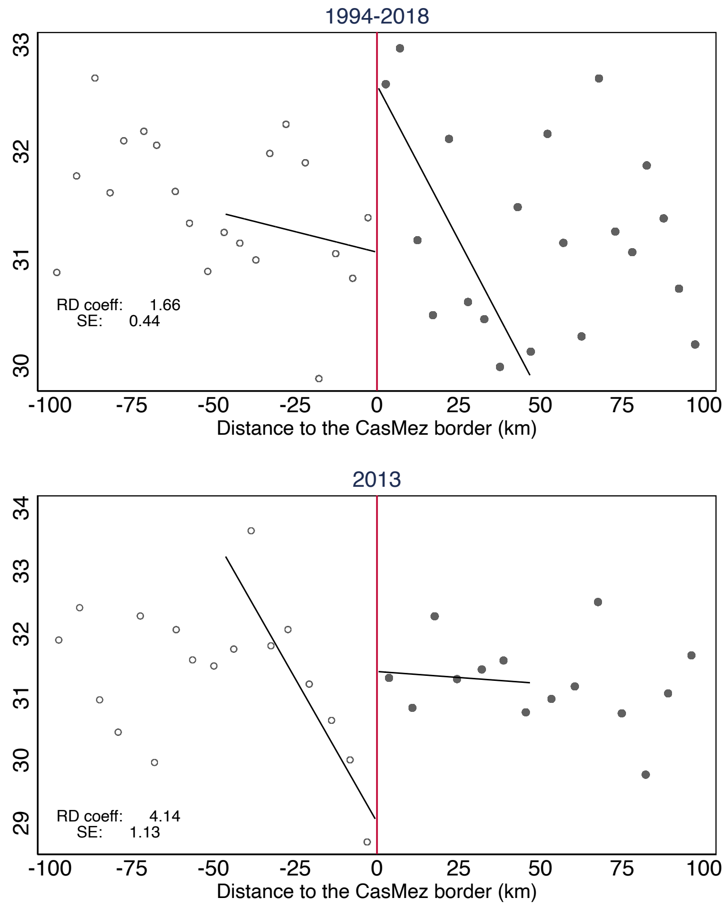
[27]This discontinuity might encompass a negative spillover to municipalities north of the border. This implies, as in von Ehrlich and Seidel (2018), that we are unable to estimate the net effect of the policy, but rather a gross effect that includes this spillover.

of the *extraordinary intervention* (1994-2018) as estimated by Equation 2.2 (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 2.3 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.[28] In Panel (b) of Table 2.2 we implement the non-parametric estimation method proposed in Calonico et al. (2014b), 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.[29]

[28]We also estimate Equation 2.3 separately for each general election after the end of the policy and plot the resulting estimates for the β coefficient in Appendix Figure B2.4. 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 2.1).

[29]The baseline estimates of Table 2.2 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 B2.1 shows that results are very similar when focusing on the entire CasMez border.

Figure 2.4. Support for state intervention at the CasMez border



Notes: Support for state intervention measured as described in Equation 2.1. 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 2.3 and text for details.

Table 2.2. Baseline RD estimates

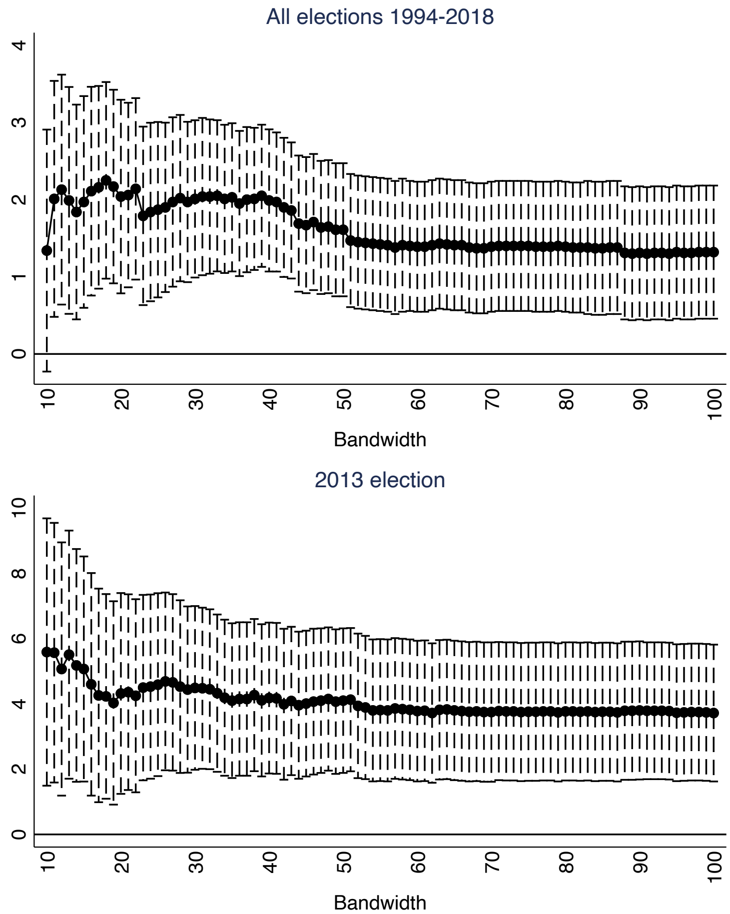
Outcome variable: *Support for state intervention*	(1) All elections 1994-2018	(2) 2013 election
	Panel (a): Parametric (linear) estimates	
RD estimate	1.66	4.14
	(0.44)	(1.13)
	Panel (b): Non-parametric estimates	
RD estimate	2.21	4.38
	(0.57)	(1.35)
Bandwidth (km)	46.96	46.96
Observations	2470	360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

Notes: 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 2.1. 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 2.2 for Column (1) and Equation 2.3 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. (2014b). 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.

2.5.2 Robustness exercises

We now focus on the baseline estimates of Panel (a) in Table 2.2 and perform a battery of robustness tests. We first show the sensitivity to the estimation bandwidth in Figure 2.5, where we plot the RD coefficient and confidence intervals as each specification (Equation 2.2 for the pooled sample of post-CasMez elections, Equation 2.3 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 2.3 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 between 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 2.5. RD estimate, robustness to bandwidth choice



Notes: The top panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2.2 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 2.3 run for the 2013 election. See text for details.

Table 2.3. 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

Notes: Replication of Panel (a) in Table 2.2, 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 2.5 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 2.2 and text for details.

As argued in Section 2.3, 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 2.4, 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 nationalist attitudes. We describe all the indicators we collect from the Manifesto database in Appendix Table B2.2. These scores are again mapped at the municipality level using local party shares as described in Equation 2.1. Figure 2.6 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.[30] 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 2.4. 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.[31] 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 2.4 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

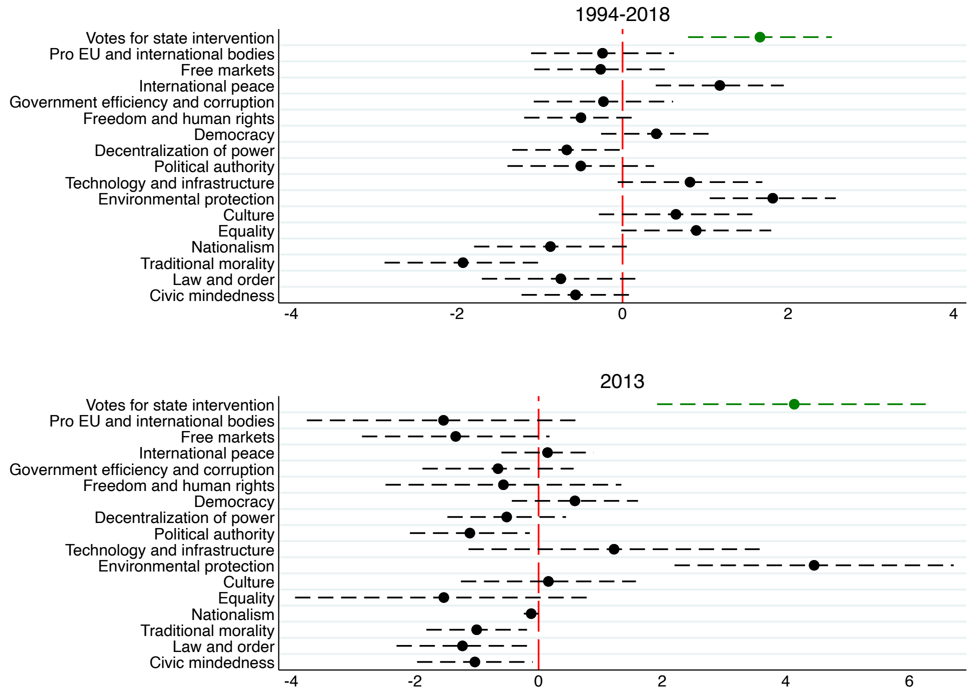
[30] Appendix Tables B2.3 and B2.4 report the RD estimates used in Figure 2.6. Figure B2.5 shows the RD plots for 2013. The largest and most significant discontinuity we estimate is in the "Environmental protection" index - another relevant theme in the 5-Star Movement's manifesto.

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

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.[32] 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 border in 2014. Indeed, this is confirmed in Column (3) of Table 2.4. 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.

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

Figure 2.6. Other views in the electorate



Notes: Each index is computed by weighing the Manifesto Project score with party vote shares at each national election using Equation 2.1. The top panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2.2 run for the pooled sample of post-CasMez elections. The bottom panel shows the estimated β coefficient and robust 95% confidence interval from Equation 2.3 run for the 2013 election. Appendix Table B2.2 describes each score. See Table 2.2 and text for details.

Table 2.4. Populism

	(1) 5-Star Mov. votes share (%), 2013	(2) Forza Italia votes share (%), 1994	(3) 5-Star Mov. 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

Notes: Estimation output of Equation 2.3 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 2.2 and text for details.

In a last exercise, we estimate Equation 2.2 on two different samples - the pooled sample of elections before (1946-1950) and during (1950-1992) the *extraordinary intervention*. We present results in Table 2.5. 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 2.2), which is of similar magnitude but more precisely estimated than that for the policy period.[33] 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 2.3. 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 2.5 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.

[33]Online Appendix Figure B2.6 reports coefficient estimates separately for each election. The estimated discontinuity during the policy years is almost always null or positive, with the 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 A2.2), suggesting that the discontinuity in 1983 might not reflect different support for state intervention but other factors.

Table 2.5. 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

Notes: Estimation output of Equation 2.2 using the optimal bandwidth. Robust standard errors in parentheses. Descriptive statistics are always computed within the estimation sample. See Table 2.2 and text for details.

2.5.3 Economic outcomes

We now examine whether the *extraordinary intervention* had any impact on the economies of the subsidized areas while it was in place and after its termination. Figure 2.7 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 B2.5), 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 (2014a) 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 2.8 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.[34]

Last, we investigate whether the policy induced differential population trends at the border by plotting population growth relative to 1951 over time in Figure 2.9. 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.[35] 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 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 B2.8).[36] 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 out-

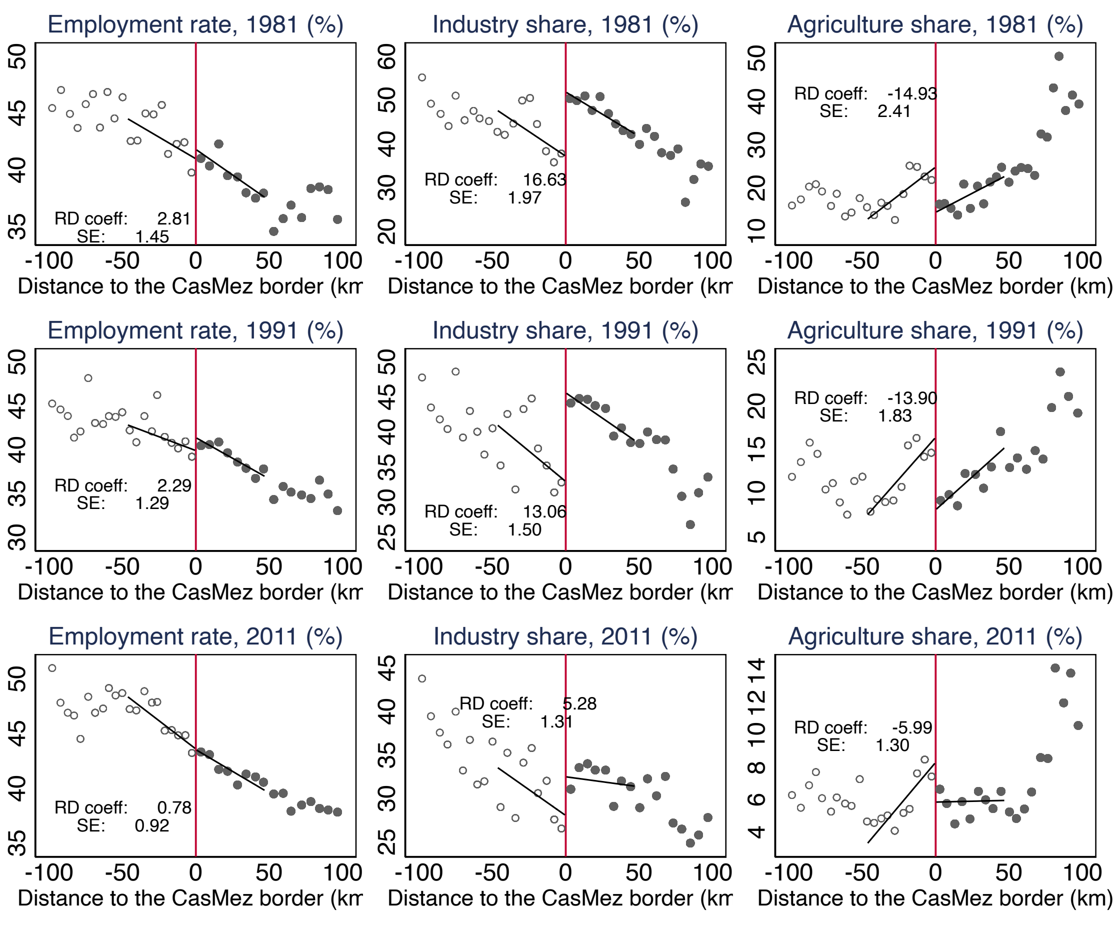
[34]The last row of Table B2.5 shows the corresponding estimates. These findings are by and large confirmed by the several robustness checks, presented in Appendix Tables B2.6 and B2.7, that test their stability under various specifications.

[35]Appendix Table B2.8 shows the corresponding coefficient estimates.

[36]Table B2.8 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.

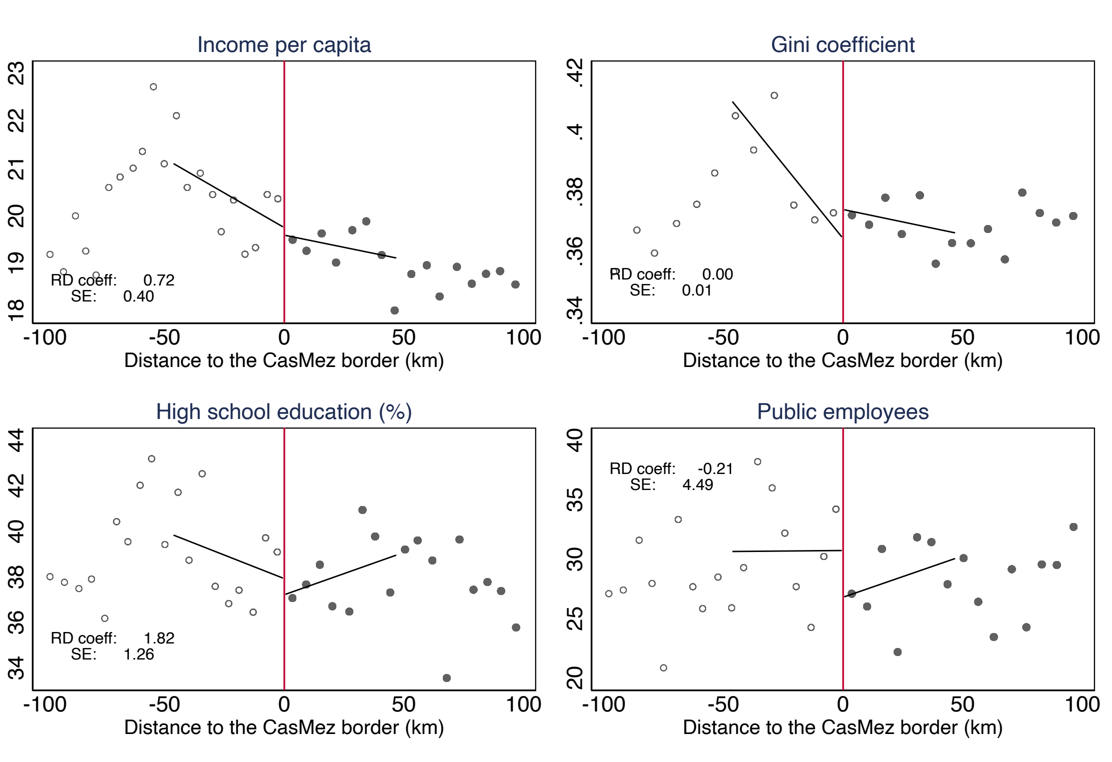
comes likely differed between treated and control areas during the policy years – most likely in terms of lower emigration rates south of the CasMez border.

Figure 2.7. Impact on labor markets



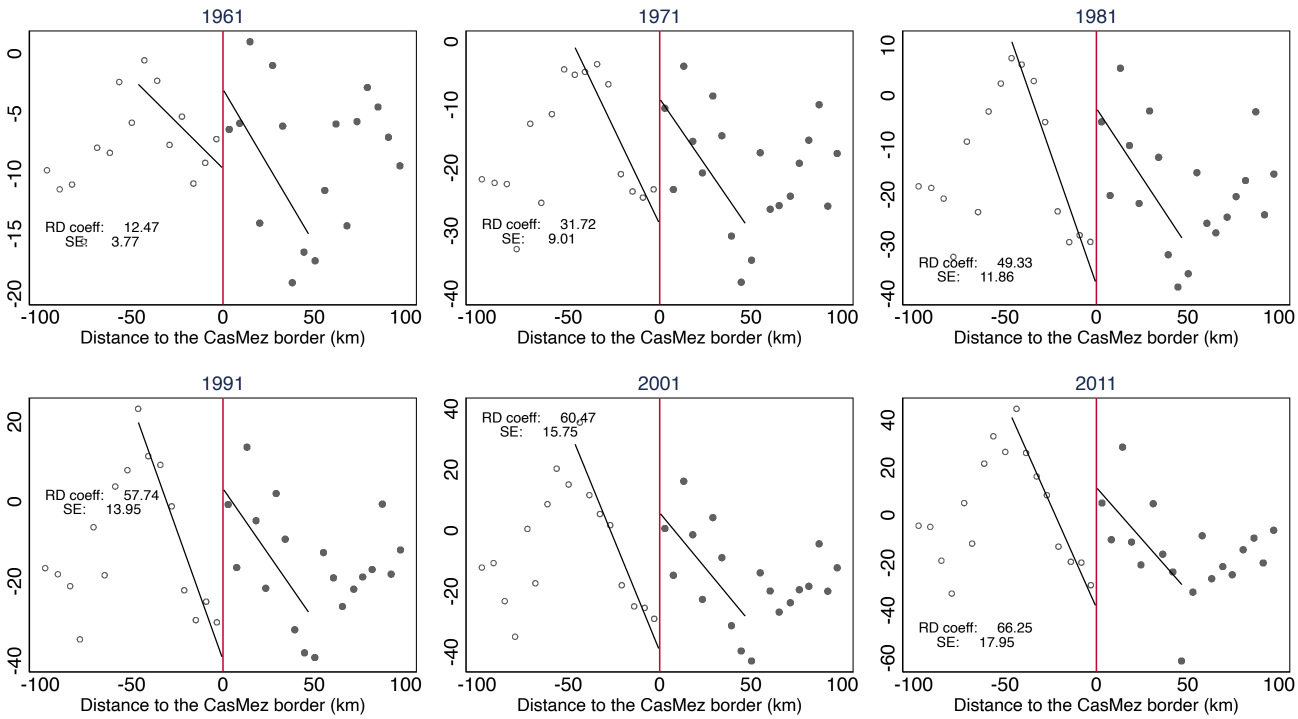
Notes: The solid black line is a linear polynomial of the outcome on the running variable, fit separately north and south of the border. See Figure 2.3 and text for details.

Figure 2.8. Economic outcomes, 2011



Notes: "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 2.3 and text for details.

Figure 2.9. Population growth relative to 1951



Notes: 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 2.3 and text for details.

2.5.4 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 2.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 2.D, are qualitatively in line with the RD estimates.

2.6 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.[37] 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 possibilities 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 condi-

[37]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).

tions 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 2.5). 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., 2023) 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.[38] 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.[39] 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

[38]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).

[39]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.

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.[40] We unfortunately lack sufficient data to perform a credible investigation of this channel. We show in Appendix 2.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.

2.7 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

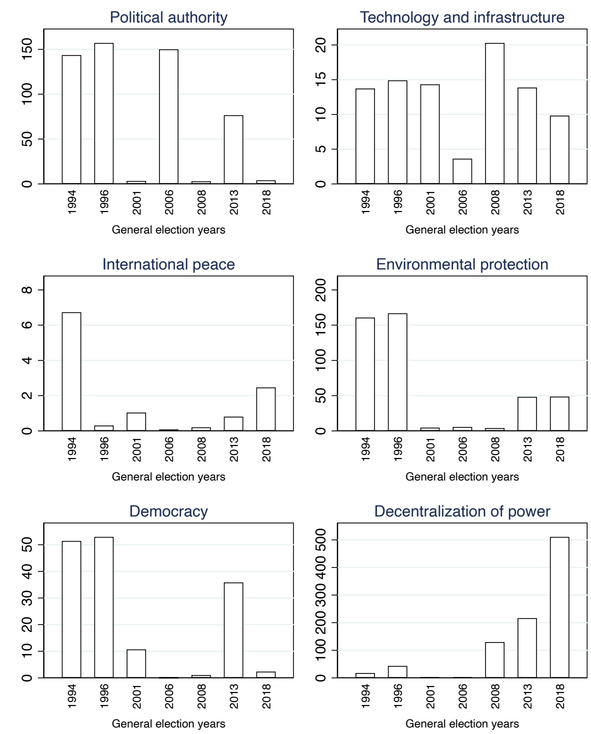
[40]If a society believes that effort has only a little role in determining wealth, it will further reinvigorate public intervention in the economy, possibly making these beliefs self-sustained (Alesina and Angeletos, 2005; Benabou, 2008).

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.

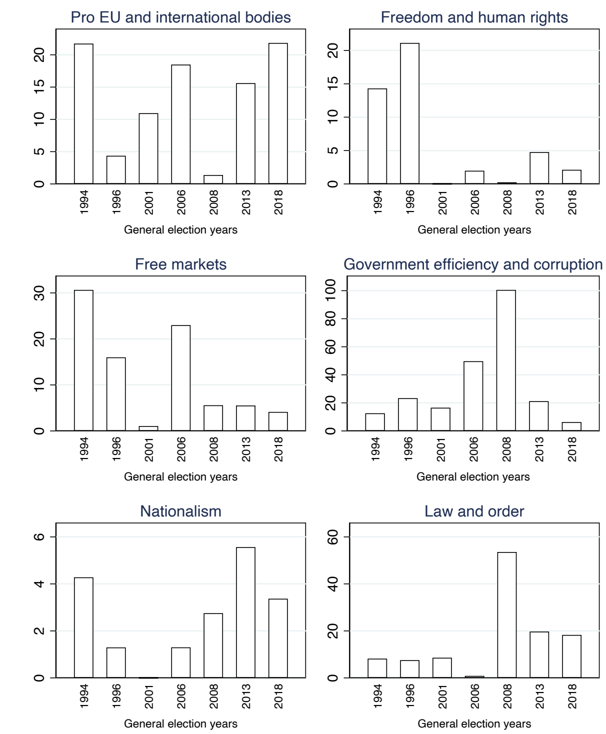
2.A Appendix A

Appendix Figure A2.1. Salience of other topics across election years (1994-2018)

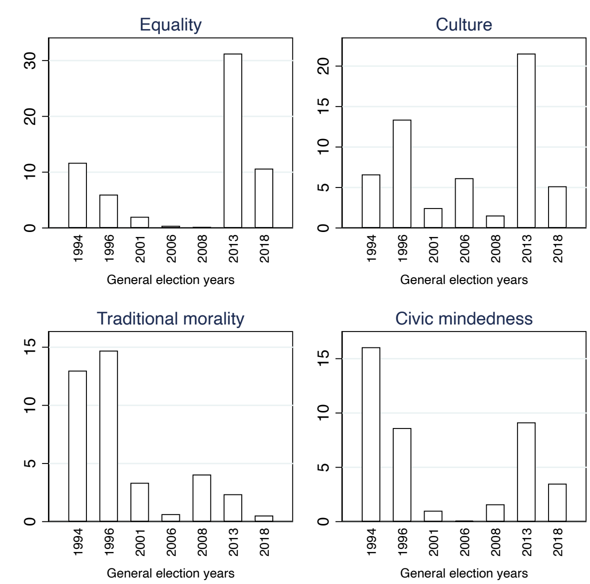
(a)



(b)

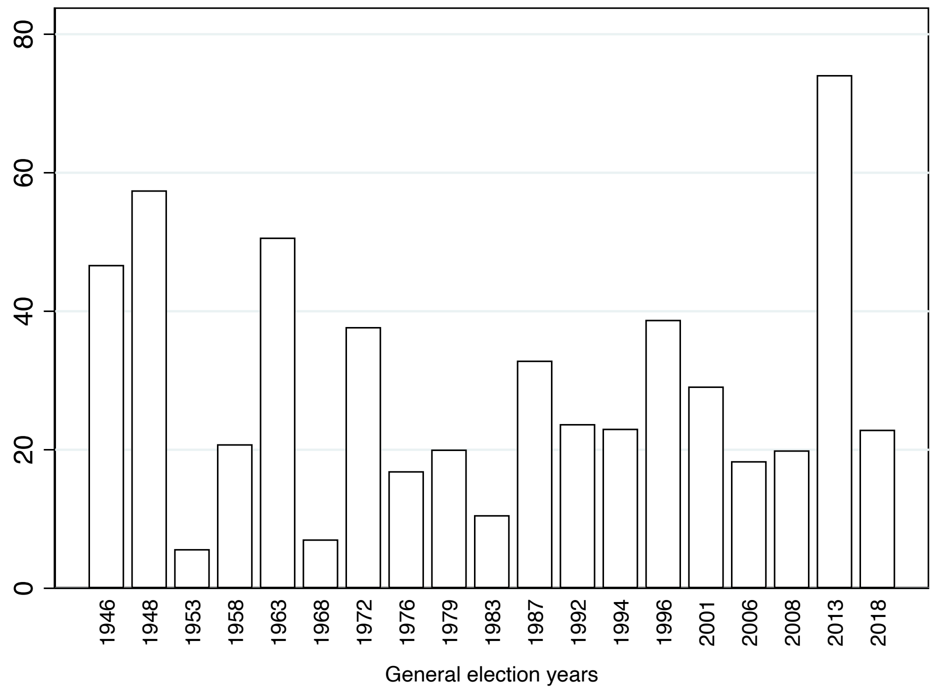


(c)



Notes: Each bar measures the variance of the Manifesto scores across parties for each election year since 1994. Each score is described in Table B2.2. See text for details.

Appendix Figure A2.2. Salience of state intervention across election years (1946-2018)



Notes: 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 2.1. See text for details.

Appendix Table A2.1. 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

Notes: 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 2.1 in the party's publicly available manifesto. See text for details.

Appendix Table A2.2. 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

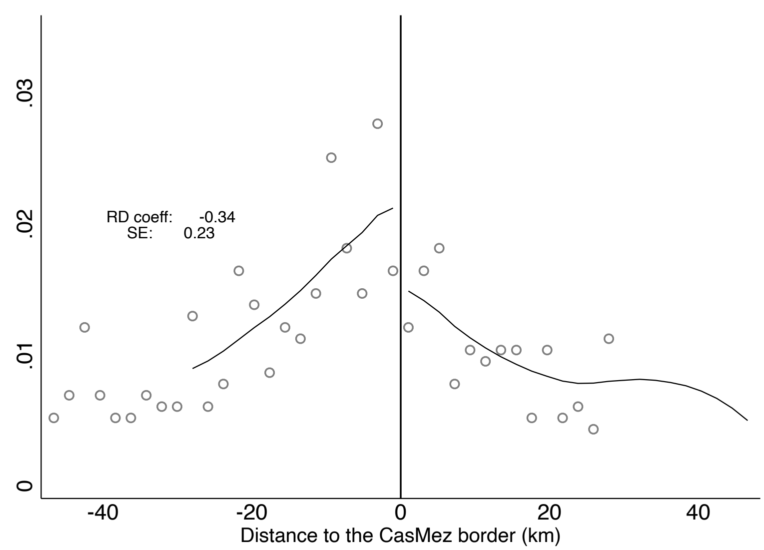
Notes: Descriptive statistics in the CasMez area. The index is computed by combining the party-specific composite Manifesto score with party vote shares at the 2013 general election (see Equation 2.1).

2.B Appendix B

Appendix Figure B2.1. CasMez jurisdiction

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

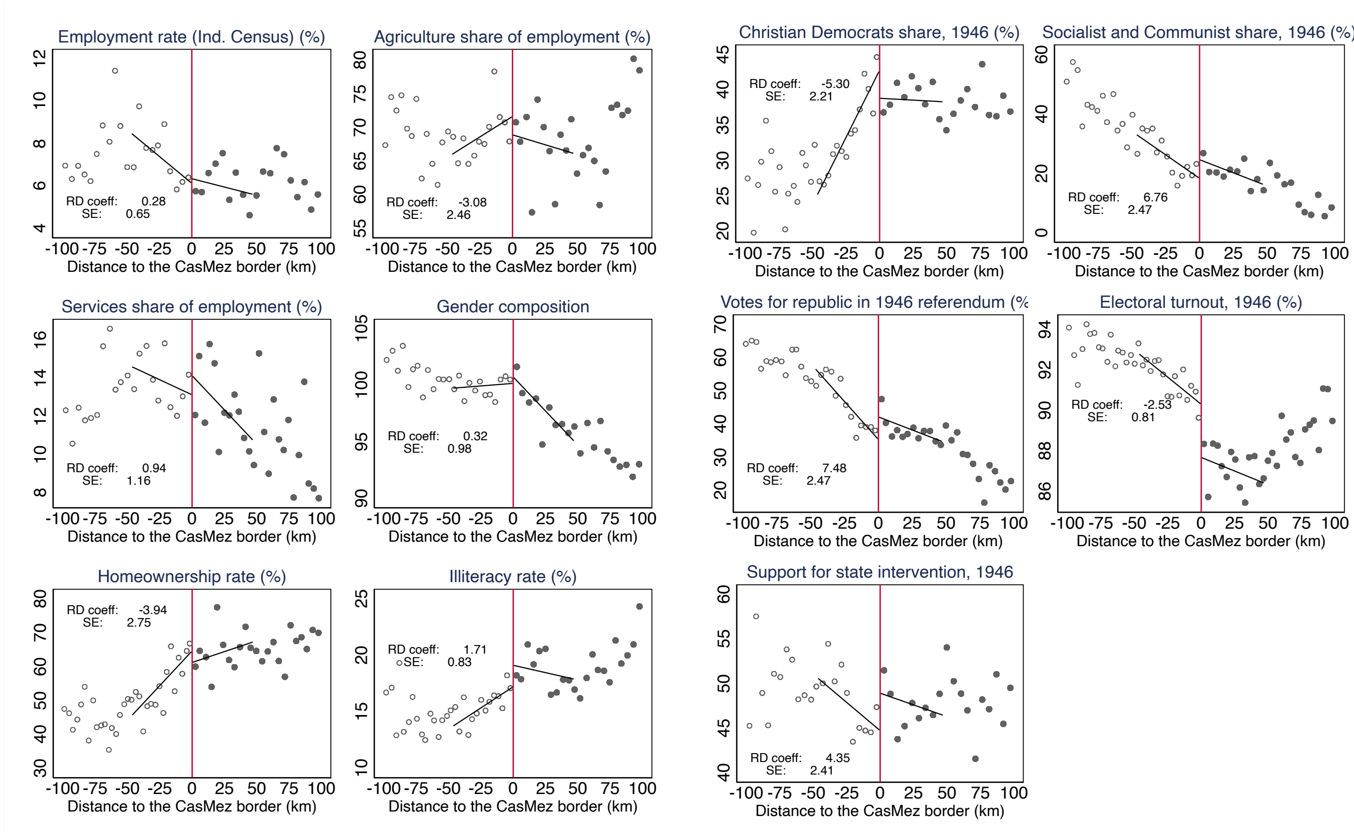
Appendix Figure B2.2. McCrary (2008) test



Notes: Output of a McCrary (2008) test of continuity in the density of the running variable.

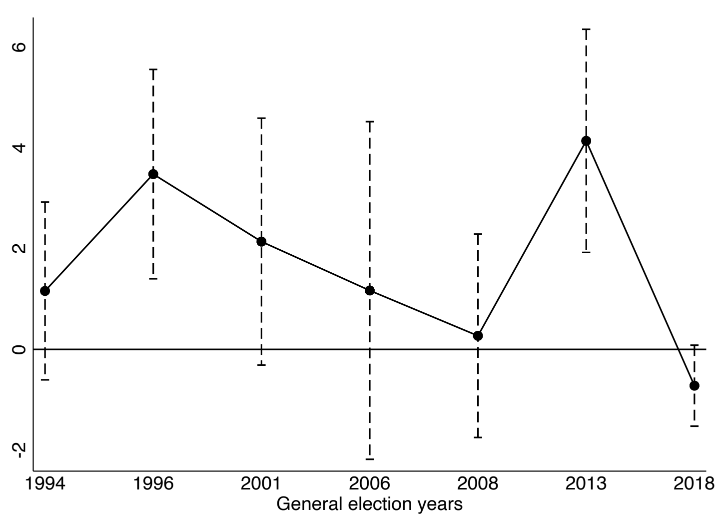
Appendix Figure B2.3. CasMez border - balancing (continued)

(a) Labor markets and demography, 1951 (b) Electoral outcomes, 1946



Notes: 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 2.1 for the 1946 election, accounting for province and border segment effects. See Figure 2.3 and text for details.

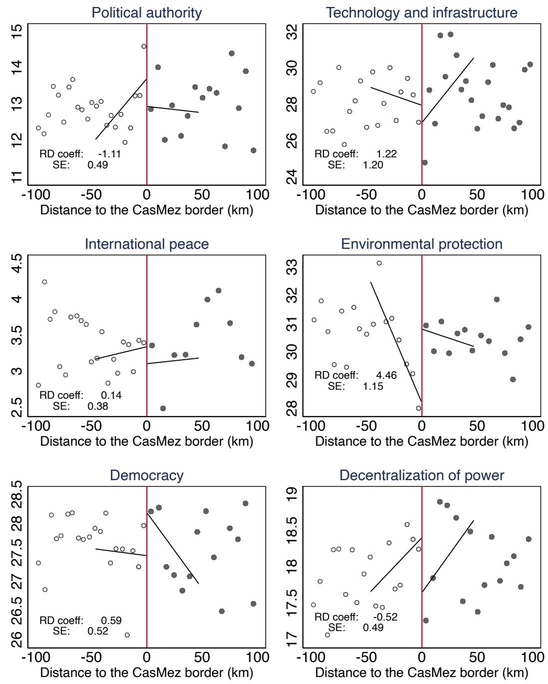
Appendix Figure B2.4. RD estimate across election years



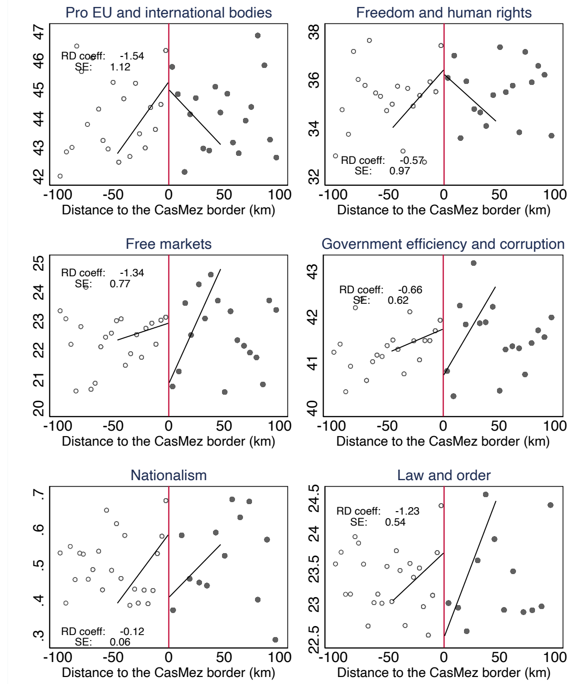
Notes: Regression estimates of the β coefficient and 95% robust confidence intervals resulting from the estimation of Equation 2.3 separately across election years post-CasMez, using the optimal bandwidth. See text for details.

Appendix Figure B2.5. Other views in the electorate, 2013

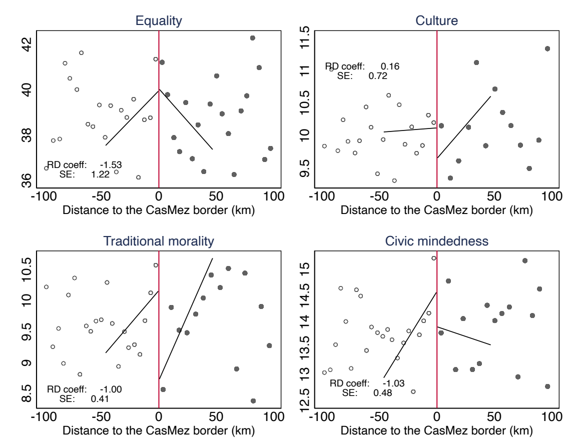
(a)



(b)

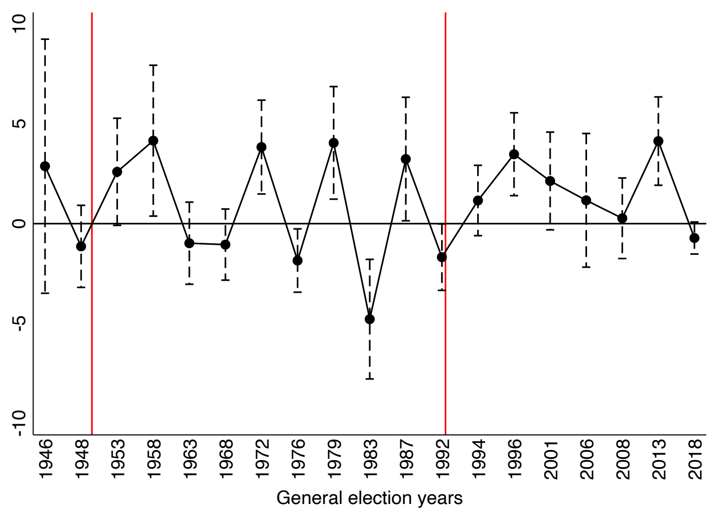


(c)



Notes: Each index is computed by weighing the Manifesto Project score with party vote shares at the 2013 election using Equation 2.1. 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 B2.2 describes each of the above scores. See Figure 2.3 and text for details.

Appendix Figure B2.6. RD estimate across election years



Notes: Regression estimates of the β coefficient and 95% robust confidence intervals resulting from the estimation of Equation 2.3 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 B2.1. RD estimates - full sample

Outcome variable: *Support for state intervention*	(1) All elections 1994-2018	(2) 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

Notes: Replication of Table 2.2, 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 2.4. See text for details.

Appendix Table B2.2. 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"

Notes: Description of the Manifesto scores used to compute the indices showed in Figure 2.6 and Figure B2.5. More details available in the Manifesto Project Dataset – Codebook (Version 2019b).

Appendix Table B2.3. 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

Notes: RD estimates of coefficient β in Equation 2.3 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 B2.2. Robust standard errors in parentheses. See text for details.

Appendix Table B2.4. 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

Notes: RD estimates of coefficient β in Equation 2.2 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 B2.2. Robust standard errors in parentheses. See text for details.

Appendix Table B2.5. 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	

Notes: Estimation output of Equation 2.3 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 2.2 and text for details.

Appendix Table B2.6. 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 B2.7. 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

Notes: Replication of Table B2.5, 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 B2.5 and text for details.

Appendix Table B2.8. 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

Notes: Estimation output of Equation 2.3 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 2.2 and text for details.

2.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 2.3 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.[41] Figure C2.1 replicates the RD plots of Figure 2.3 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 C2.2 shows that our results are virtually un-

[41] These are municipalities where the lead of Christian Democrats in 1948 was very small, or negative. Table C2.1 details the distribution of this variable in municipalities 10 km south of the CasMez border.

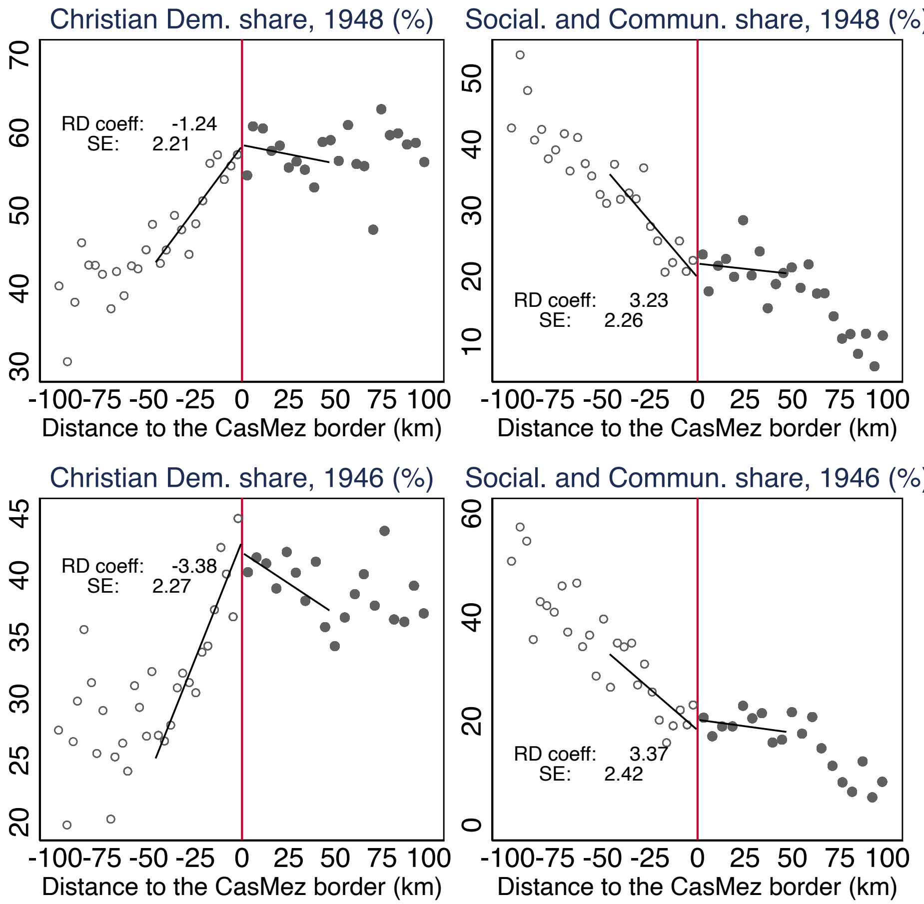
changed when excluding these potentially problematic municipalities, as we still estimate a positive effect on voters' support for state intervention long after the end of the policy (the point estimate is almost identical to that for the baseline sample).

Appendix Table C2.1. 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

Notes: 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 C2.1. Balancing, pre-CasMez elections



Notes: Replication of Figure 2.3 on sample excluding municipalities south of the border with strong support for opposition parties. See text for details.

Appendix Table C2.2. RD estimates - Low lead of Christian Democrats in 1948

Outcome variable: *Support for state intervention, 2013*	(1) Baseline estimates	(2) 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

Notes: RD estimates of coefficient β in Equation 2.3 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 25^{th} percentile of the distribution up to 10 km south of the border. Robust standard errors in parentheses. See text for details.

2.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 D2.1, which shows the cumulative amount of CasMez transfers received by each municipality between 1950 and 1992. To address the endogeneity concerns raised in Section 2.4 and provide more reliable estimates, we exploit here a source of institutional variation in transfers. As described in Section 2.2, 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.[42] The ASET historical archives provide a list of the Industrial Zones, together with the 400 included municipalities, which we digitize and plot in Figure D2.1, 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 D2.1, 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

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

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 D2.2. The form listed a range of demographic, geographic and economic characteristics aimed at assessing 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:

$$e_{m,p} \equiv Pr(IZ_{m,p} = 1 | W_{m,p}, \gamma_p, \epsilon_{m,p}) = \Phi(\alpha + \gamma_p + W_{m,p}^{'} \cdot \beta + \epsilon_{m,p}) \tag{2.4}$$

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 $\hat{e_{m,p}}$.[43] 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 D2.3. Descriptive statistics are reported in Table D2.2 and confirm the overall balancing of the sample. A stark gap in CasMez transfers 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.[44] 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

[43]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).

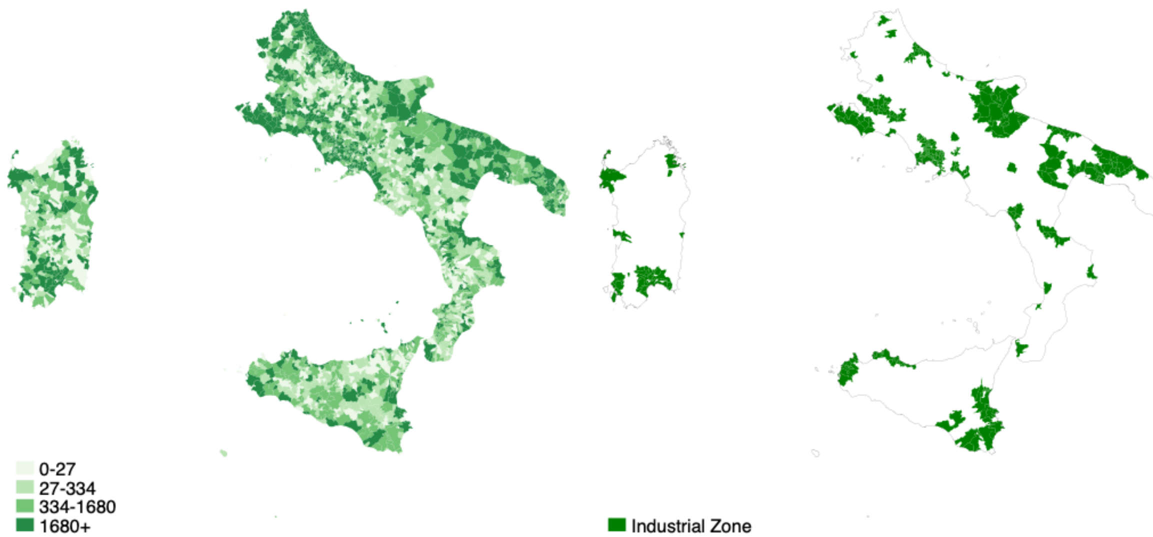
[44]In other words, two municipalities sharing similar characteristics but with different Zone status can be safely compared as the missed inclusion in a Zone is driven by factors exogenous to electoral support for state intervention in 2013. This ensures that the reduced form effect of $IZ_{m,p}$ on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

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 D2.3. 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.

Appendix Figure D2.1. The Industrial Zones

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



Notes: 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 D2.2. 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	»	

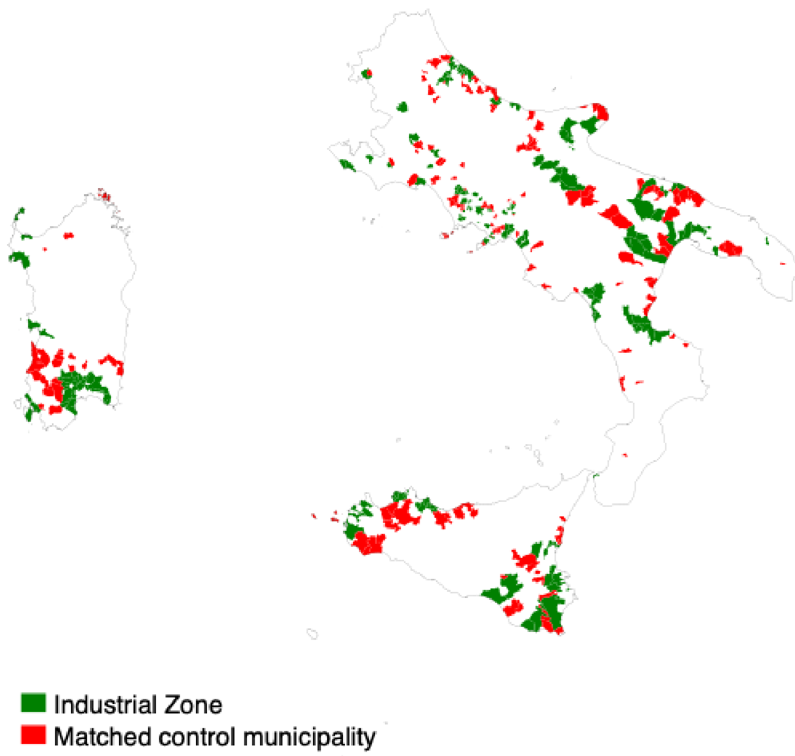
(1) Addetti alle industrie in totale (3.1.1.) meno addetti all'artigianato secondo le indicazioni del *Censimento Industriale del 1951.*

(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.

Notes: 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 D2.3. Industrial Zones - matched sample



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

Appendix Table D2.1. 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

Notes: 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 1^{st} and 99^{th} 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 D2.2. 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

Notes: 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 1^{st} and 99^{th} 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 D2.3. Baseline 2-SLS estimates

	(1) First stage	(2) 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

Notes: 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 1^{st} and 99^{th} 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 2.1. The regression includes province-level fixed effects. Robust standard errors in parentheses. See text for details.

2.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 (Corneo and Gruner, 2002, Alesina and Giuliano, 2011) which might, in turn, reinforce preferences for state intervention (Alesina and Angeletos, 2005; Benabou, 2008). We exploit the 5$^{\text{th}}$ Wave (2005-2009) of the World Values Survey (WVS), which contains a small set of questions on individual preferences for state intervention for Italy.[45] 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"*).[46] 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 5$^{\text{th}}$ wave of the WVS features responses from 1,012 individuals scattered

[45] At the time of writing, Italy features only in the 5$^{\text{th}}$ wave of the WVS (2005-2009).

[46] 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"*).

around the country. Of these, only 17 live 47 km (the baseline RD bandwidth) south of the CasMez 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.

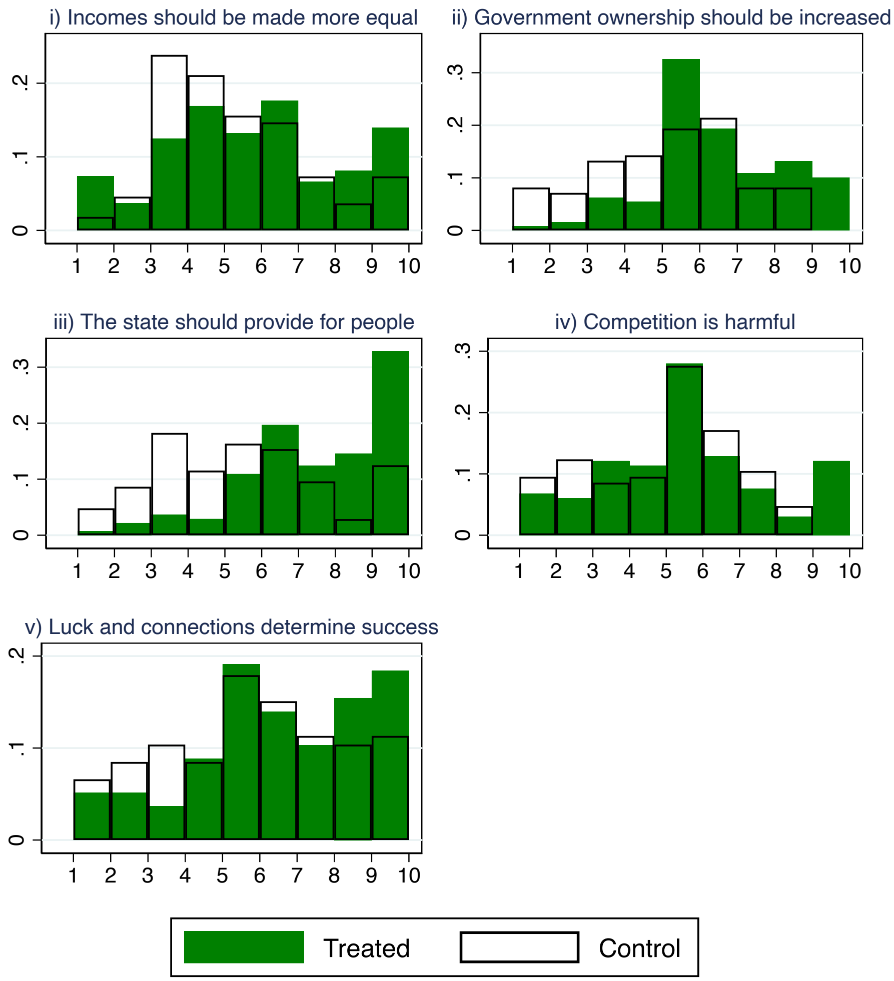
Figure E2.1 plots the empirical distribution of responses to the five above questions separately for respondents living south (green bars) and north (white bars) of the border. A glance at the histograms suggests that the distribution of responses in treated municipalities is more skewed towards agreement 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 E2.2 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 E2.1 of a positive discontinuity at the CasMez border, although the very small sample size and limited variation make these results quite uncertain.

Table E2.1 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.[47]

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.

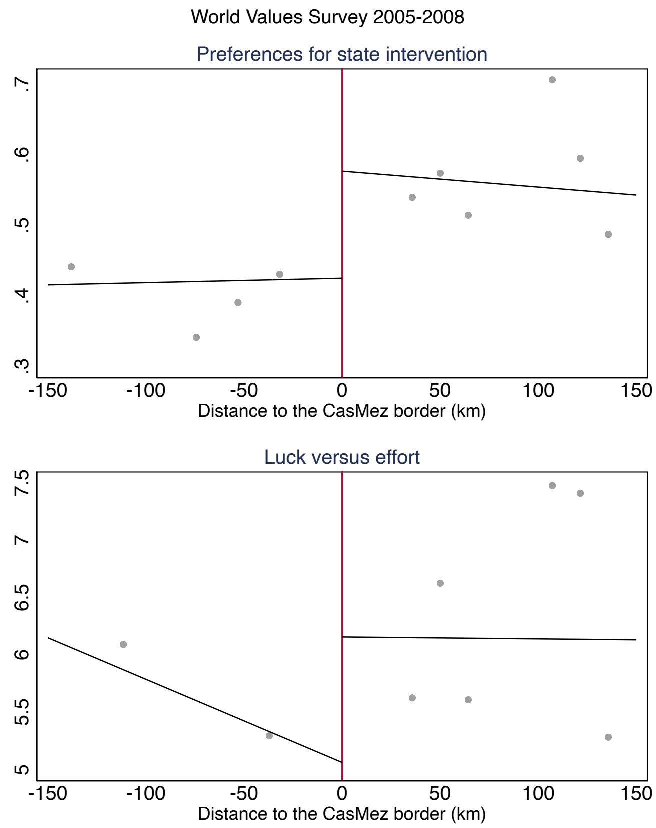
[47] The coefficients in Columns (1)-(4) and Column (6) are expressed in log-odds units and do not have a meaningful interpretation. Table E2.2 shows the implied predicted probabilities (only for the estimates in Panel a)).

Appendix Figure E2.1. WVS responses, distribution south and north of CasMez border



Notes: 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 E2.2. WVS responses, RD plots



Notes: "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 2.3 and text for details.

Appendix Table E2.1. 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

Notes: 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 E2.2 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 E2.2. 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%

Notes: 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 E2.1, 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 E2.1 and text for details.

Chapter 3

Opting Out of Centralized Collective Bargaining: Evidence from Italy

Christian Dustmann, Chiara Giannetto, Lorenzo Incoronato, Chiara Lacava, Vincenzo Pezone, Raffaele Saggio, Benjamin Schoefer[1]

3.1 Introduction

A key policy concern in many European countries are the trade-offs arising from centralized, high-coverage collective bargaining regimes (Visser, 2013). While these institutions establish minimum labor standards for all workers and firms, many observers point to the distortions from this rigidity. For instance, these standards may prevent firms from optimally adjusting in response to negative shocks, and may depress employment in unproductive areas (Boeri et al., 2021). To balance these trade-offs, the OECD has proposed "coordinated decentralization" as a suitable institutional set-up (OECD, 2019). In such a regime, centralized (e.g., national sector-level) agreements define a broad negotiation framework, but individual firms can opt out to negotiate wages and other labor provisions more directly with their workforce. Such decentralization would raise competitiveness and employment stability but may entail wage reductions. While this "competi-

[1] This article is the outcome of the merger of two previously separated papers, and of the respective author teams. Dustmann: UCL; Giannetto: UCL; Incoronato: UCL; Lacava: Goethe University Frankfurt; Pezone: Tilburg University; Saggio: UBC; Schoefer: UC Berkeley.

tiveness channel" is a crucial ingredient in models of optimal decentralization of bargaining regimes (Calmfors and Driffill, 1988; Jimeno and Thomas, 2013), there is little empirical evidence on it and, more generally, on the benefits and costs of such decentralization to firms and workers. The key challenges are to precisely identify in micro-data the firms and workers subject to opt-outs, and to obtain suitable quasi-experiments in opt-out events.

This paper examines the recent decentralization of industrial relations in Italy – an ideal laboratory, as the Italian economy features rigid collective bargaining institutions that are heavily centralized and impose national wage floor schedules for each sector that frequently bind, particularly in low-productivity firms and regions (Boeri et al., 2021). Italy's persistent challenges following the Great Recession have increased pressure on labor market institutions (Boeri and Garibaldi, 2019), leading to both reforms but also increased attempts by employers to evade rigid regulations and gain flexibility within the existing system. Our paper focuses on the emerging attempts of firms to opt out of centralized collective bargaining.

To investigate how firm opt-outs affect firm and worker outcomes, we combine information on collective bargaining agreements (CBAs) with detailed matched employer-employee data provided by the Italian Social Security Institute (INPS) for the universe of private-sector workers and firms in Italy from 2005 to 2019. A key feature of the Italian matched employer-employee data is that it permits to observe the CBA applied to a particular job, and hence precisely identify opt-outs of firms from their national CBA.

The first opt-out variation is in the form of idiosyncratic firm-level ones: firms evading their original CBAs into so-called "pirate" CBAs that offered flexible working conditions and lower wage floors. These opt-out events stemmed from a regulatory loophole that gained traction during the recession of the early 2010s. By 2019, pirate CBAs constituted around two thirds of the total number of collective contracts in Italy and covered half a million workers (3 percent of total private-sector employment). Our matched contract-firm-worker data let us directly observe such transitions from a standard to a pirate CBA.

Second, we study a prominent secession of a group of large employers operating in the mass-retail sector (e.g., Ikea, Zara, Carrefour). In 2011, these employers abandoned their original employer organization, which represented *all* employers in retail and whose provisions on various margins, such as opening hours, tended to favor small businesses. This opt-out, in the form of a secession, permitted them to bargain separately with unions. In our data, we can identify precisely the firms (and therefore workers) that are part of the retail opt-out.

To study the effects of these opt-out events, we employ matched difference-in-differences designs comparing incumbent workers subject to the opt-out to suitable control groups. To mitigate selection problems, we assign each treated observation to a similar control peer, employed elsewhere, based on pre-opt-out characteristics. Our comparison of results from two separate sources of variation in opt-out (and distinct potential selection) fosters the causal interpretation. Moreover, because opt-out decisions are made by firm managers and "imposed" on workers, selection issues are likely not very pronounced in our worker-level analysis, where we track the cohort of workers employed at the firm right before the event (as in, e.g., Goldschmidt and Schmieder, 2017).

We find evidence broadly consistent with the competitiveness channel of opt-outs, both for the pirate agreements and for the retail secession. Our main results are that opt-outs significantly lower workers' wages – implying that firms used the increased flexibility to cut labor costs – but at the same time raise employment stability. On net, the positive employment effect and the negative wage effect roughly compensate each other, such that earnings are unaffected. Quantitatively, wages fall by 3-4 percent for either opt-out event, while the probability of staying employed increases by 2-4 percentage points, partly driven by higher retention with the original employer.[2]

In additional checks, we uncover suggestive evidence for complex heterogene-

[2]We also study firm-level effects of opting out, but caveat that even after matching, dynamic selection is likely a much larger concern than for workers. For the pirate agreements, we find an about 3 percent reduction in labor costs, but positive effects on firm survival of about 4 percentage points in the 2-3 years immediately after the opt-out.

ity. Small firms exhibit lower wage cuts and employment gains, with the competitiveness channel more evident for large firms – perhaps as they face stricter employment protection. Also, firms in Southern Italy cut wages by more when adopting pirate agreements, consistent with national nominal wage floors being more distortionary there (Boeri et al., 2021). But we also find smaller employment effects in the South, possibly because of lower labor market competition.

Our work presents novel evidence of the effects of decentralizing collective bargaining on workers and firms. Despite a global decline over the past decades, collective bargaining remains highly relevant in many labor markets, especially in Europe, where CBAs cover between 60 and 100 percent of the workforce (OECD, 2019). Dustmann et al. (2014) contrast the inflexibility of CBAs typically found in Southern European economies[3] with the autonomous industrial relations in Germany, where opening clauses allowed re-negotiations of union contracts at the firm level in times of economic hardship. Card et al. (2013) present evidence suggesting that firms opting out of sectoral bargaining agreements helps understand recent trends in wage inequality in Germany. Rigorous and direct micro-evidence on the effects of firms changing to more flexible CBAs remains, however, scarce, as it is often hard to precisely identify firms and workers that are affected by transitions, especially in administrative data. This paper fills this gap by leveraging Italian social security data to identify opt-out events, thanks to information on the collective bargaining agreement applied to every job.

Our paper is closely related to Lucifora and Vigani (2021), Dahl et al. (2013) and Gürtzgen (2016). Lucifora and Vigani (2021) also examine the rise of pirate agreements in Italy, which is one of our two sources of variation, and find substantial wage penalties. Our paper extends Lucifora and Vigani (2021) in several dimensions. First, we leverage the universe of Italian private-sector workers (rather than a 1/90 sample) and hence study the universe of pirate agreements in Italy. Second, we study the effects of pirate agreements on a richer set of outcomes, in-

[3]Many Southern European countries, like Italy and Spain, impose rigid sector-specific wage schedules at the national level that apply to all workers, irrespective of their union status (Adamopoulou and Villanueva, 2022).

cluding firm-level outcomes. Third, our paper draws on two sources of variation in opt-outs; besides pirate agreements, we study the retailer opt-out event.

Dahl et al. (2013) study gradual decentralization in Denmark using longitudinal data and a fuzzy approach based on occupation and sector codes to identify job-level bargaining regime shifts. Gürtzgen (2016) studies the effects of manufacturing and mining firms leaving industry-level agreements in Germany, however drawing on survey data to infer the opt-out. This paper extends the analysis in Dahl et al. (2013) and Gürtzgen (2016) by identifying opting-out events from administrative data and by drawing on sharper variation. Moreover, our setting, Italy, features a significantly more rigid (at baseline) centralized bargaining system compared to Denmark or Germany (see, e.g., Dustmann et al., 2014; Jäger et al., 2022). Finally, our analysis covers a period of remarkable difficulties for European labor markets – a period perhaps particularly relevant for the debate that motivates our study – whereas both Dahl et al. (2013) and Gürtzgen (2016) study a period of stability (1992-2001 and 1999-2007, respectively).

The paper is organized as follows. Section 3.2 and Section 3.3 illustrate the institutional background and the opt-out events. Section 3.4 describes the data and Section 3.5 outlines the empirical approach. Section 3.6 presents the results and Section 3.7 the heterogeneity analysis. Section 3.8 concludes.

3.2 Institutional Background

Industrial relations in Italy are chiefly based on national sector-level collective agreements. While there is a second tier at the firm-level, which we review below, it does not provide flexibility downwards (as firms can only deviate upwards from the national CBAs).

Nationwide Sector-Level Collective Agreements. National, sector-level CBAs (*Contratti Collettivi Nazionali del Lavoro*, which we simply denote by CBAs) have historically been signed by the most representative employer and employee associations. While the law does not define representativeness criteria, the term "representative" has generally been used to denote CBAs signed by the three main unions in Italy.[4] A sector's representative CBA is effectively extended *erga omnes*, that is, it also applies to workers who are not members of the unions that signed the CBA and to employers not part of the signatory employer association. Besides establishing broad employment conditions (e.g., vacations and working hours), CBAs set a *schedule* of wage floors for different (typically eight) job titles, which roughly correspond to different occupations. CBAs typically last three years. Wage floors are periodically adjusted following a predetermined schedule, reflecting expected inflation. At the expiration, CBAs should be immediately renewed. In the frequent case of delays, the expired CBA remains in force.

The actual wage is the CBA wage floor plus a potential job-level premium. Hence, the actual wage moves one to one with the floor unless the premium is actively reduced. Hence, shifts in CBA wage floors affect actual wages even in the presence of a premium (consistent with empirical evidence for a significant pass-through in Fanfani, 2023; Faia and Pezone, 2023).

Firm-Level Agreements. At the second tier are firm-level agreements negotiated between employers and firm-level union delegations (for a summary, see Boeri, 2014, and Dell'Aringa, 2017). Subordinated to sector-level bargaining, firm-level agreements can only regulate matters when explicitly allowed to do so by the CBA ("non-repeatability" clause) and can deviate only to set better conditions for the worker ("favorability" clause). As a result, only around 20 percent of firms larger than 20 employees have negotiated directly with workers between 2010 and 2016 (D'Amuri and Nizzi, 2017), providing relatively limited decentralization (Boeri, 2014). These constraints have contributed to the opt-out events that are the focus of our paper and described in Section 3.3 below.

[4] These unions are Confederazione Generale Italiana del Lavoro (*CGIL*), Confederazione Italiana Sindacati Lavoratori (*CISL*), and Unione Italiana del Lavoro (*UIL*), which have long dominated the Italian industrial relations.

3.3 The Opt-Out Events

We now describe the two opt-out events that have allowed Italian firms to abandon their CBA: individual firms exploiting an emerging loophole that permitted them to switch into lower-wage "pirate" agreements (Section 3.3.1), and a coordinated departure of large retailers from their employer association (Section 3.3.2).

3.3.1 Adoptions of Pirate Agreements

Background. The first facet of opting out is the emerging adoption of so-called "pirate" agreements.[5] Here, an individual firm opts out of its assigned CBA (typically subscribed by the three main Italian unions) by signing a new CBA with minor and less representative unions. Pirate CBAs often entailed lower wages and more flexible working conditions.

Pirate agreements exploit a loophole in Italian labor laws. In principle, an Italian firm must adopt the wage floors established by the representative CBA of their sector of activity. However, as noted in Section 3.2, the law does not clearly define the concept of a "representative CBA" for a given firm or sector – leaving legal room for employers to try and abandon the main CBA and adopt another CBA signed by "pirate unions" rather than the three main unions.[6] This loophole has been increasingly exploited, especially after the Great Recession, as more and more employers have faced challenges in meeting the labor standards set by centralized bargaining.

[5] This term evokes the disruptive effects on the traditional system of industrial relations, as in the following quote: "Although the pirates we are dealing with today are not equipped with sabers or muskets, but, probably, with pens, smartphones and tablets, the metaphor seems to hit the mark: the proliferation of "pirates" in the industrial relations system - or in inter-union system, [...] is a symptom of the insufficient organization of this system and its (in)ability to repel «incursions» and «hostile acts.»" (Centamore, 2018, authors' translation).

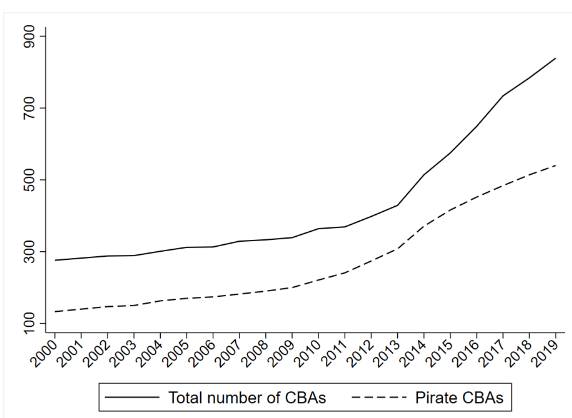
[6] "Pirate unions" were either historical, minor associations that competed with the three main ones, or newborn unions created after 2010. In practice, firms did not renew their existing CBA after expiration and instead adopted a new, pirate contract. There have been cases (subsequently condemned by labor courts) in which firms did so before expiration. Firms that had not signed any CBA in the first place, but that were just applying the representative CBA for their sector, could begin adopting a new CBA immediately.

Implications for Flexibility and Wage Setting. In Appendix 3.A, we present a case study of the pirate agreements. We notice how the pirate contract specified lower wage floors, in particular at the low end of the distribution, and allowed for regional differentiation in pay.

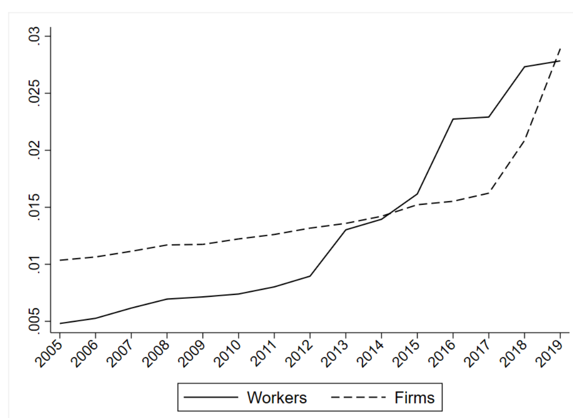
Time Series and Geographic Distribution. Drawing on data we describe below in Section 3.4, we discuss some basic descriptives regarding the trends of pirate agreements and the workers and firms subject to them. Figure 3.1 illustrates the growth of pirate agreements following the Great Recession. Panel (a) shows the number of pirate agreements signed each year; Panel (b) displays the share of firms and workers covered by pirate agreements. Despite being the majority of CBAs in Italy, pirate contracts still cover only about 3 percent of private sector workers and firms in 2019, roughly half a million workers and about 40,000 firms. This skew reflects the fact that standard CBAs by their nature cover most firms, and pirate agreements are tailored contracts and signed by small unions.

Figure 3.1. The Evolution of Pirate Agreements

(a) Counts of Contracts



(b) Pirate Share of Workers and Firms



Notes: Panel (a) depicts the total number of collective bargaining agreements in Italy per year. Pirate agreements defined as those not signed by at least one union in the union triad. Panel (b) shows this comparison for the fraction of firms (dashed line) and workers (solid line). For firms, the share is computed as the number of firms adopting a pirate contract for at least one employee as a share of the total number of private-sector firms in the INPS data each year. For workers, the share is computed as the total number of workers covered by a pirate contract as a fraction of the total number of private-sector workers in the INPS data each year.

Appendix Figure B3.1 depicts the geographic heterogeneity: as of 2019, pirate contracts were concentrated in the Center-South – consistent with the findings of Boeri et al. (2021), who note that wage floors are particularly binding in these less productive regions. Out of all workers covered by pirate agreements, roughly 35 percent were in Southern regions compared to 23 percent for non-pirate workers. For firms, these shares are 15 and 8 percent, respectively.

Descriptives: Worker and Firms. Appendix Table B3.1 compares workers covered by pirate agreements to other workers, showing a higher share of women and part-time workers, and lower average wages. Appendix Table B3.2 does the same comparison for firms, and shows that firms using pirate CBAs tend to be larger and younger and employ more women and part-time workers. Finally, Appendix Table B3.3 provides information on the industry distribution of workers covered by pirate agreements. These CBAs are largely concentrated in services sectors. Conversely, the share in manufacturing is 20 percent, as opposed to 27 percent for workers covered by representative contracts.

3.3.2 The 2011 Secession of Mass Retailers

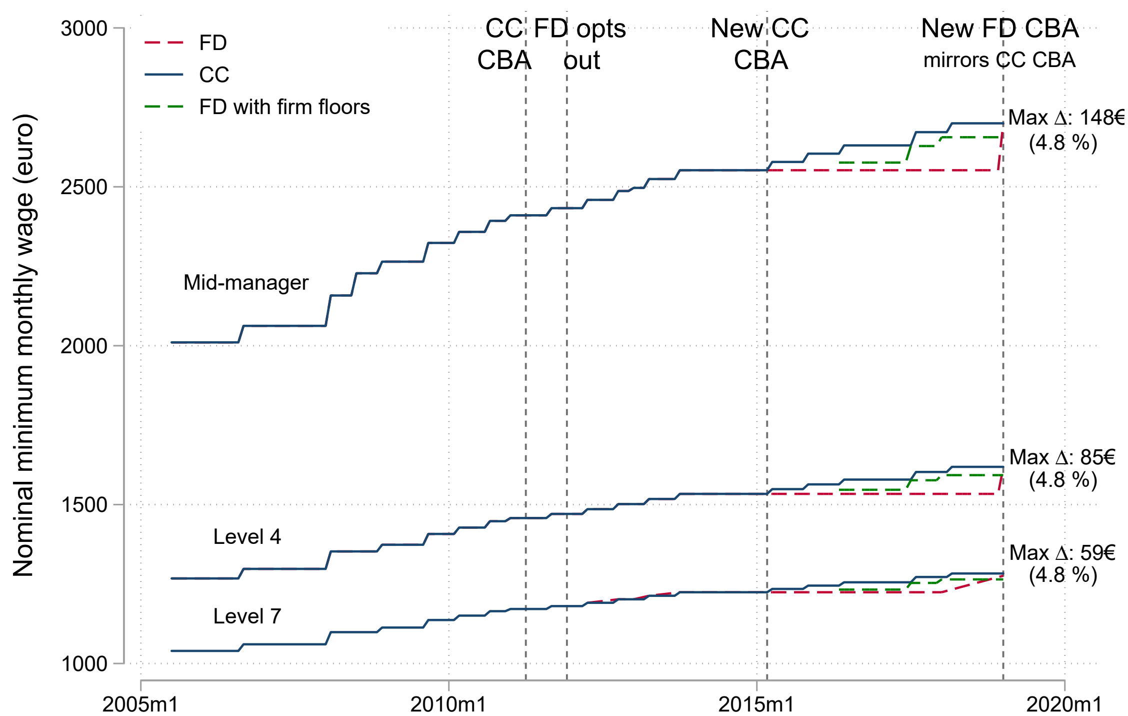
Background. The second opt-out event is the 2011 secession of a group of large retailers – *Federdistribuzione* (FD) – from their employer association, *Confcommercio* (CC), to negotiate a separate CBA. CC comprised 90 employer sub-associations (e.g., of butchers, hotels, stationers,...). FD was the sub-association for mass retailers, such as large hypermarkets (e.g., Carrefour), clothing (e.g., Coin), furniture (e.g., Ikea) and home improvement and gardening (e.g., Leroy Merlin). Several companies correspond to the Italian branches of foreign multinational corporations. As of 2010, there were 56 firms in FD, representing around one-fourth of retail employment. On December 23, 2011, FD announced the exit of its members from CC, in order to lobby independently and negotiate a separate FD-CBA. The reason was a strategic disagreement between the large FD employers and the small retailers that dominated CC and opposed FD's push for liberalization of opening hours and large shopping malls.

Implications for Flexibility and Wage Setting. While not the primary motivation, the opt-out had important consequences for wage floors in FD firms vis-à-vis CC firms. Figure 3.2 plots the evolution of wage floors between 2005 and 2020 for those groups, for three job titles/job levels: level 7, the lowest rank aside from apprentices, level 4, the modal job title, and for middle-level managers, or *Quadri* (managers above this level have a separate CBA).

Before the opt-out in 2011, FD was part of CC and hence was subject to CC wage floors. FD would also be bound by the 3-year CBA that had gone into force in January 2011, expected to last through 2013. However, in 2014, wages did not diverge because of delay in the bargaining process between CC and the unions. The new CC-CBA was finally agreed on on March 30, 2015, covering the period of April 2015 to March 2018. Hence, wage floors in FD firms remained frozen at the 2013 levels and diverged substantially starting in 2015, with the gap's 2018 peak being around 4.8% (homogeneous across job titles).

Figure 3.2 also reports (green dashed line) the wage policies FD voluntarily and *unilaterally* self-imposed from 2015 to 2018. During that period, unions rejected FD's proposal to merely replicate CC-CBA wage floor increases – whereas unions believed the large FD firms should provide larger real wage increases than those designed for small and struggling CC retailers. As part of this conflict, the unions implemented three short strikes between November, 2015, and May, 2016. To partially accommodate unions' demands, FD firms implemented unilateral raises that took effect on May, 2016 and June, 2017. However, FD firms also engaged in hostile industrial relations actions, e.g., cutting the generosity of supplementary health insurance and abolishing lower-level work councils (*Enti Bilaterali Territoriali*). We view these events as *outcomes* of the opt-out, previewed here to complement our empirical analysis.

Figure 3.2. The 2011 Secession of Mass Retailers from their CBA: Timeline of Wage Floors for Three Job Titles



Notes: The figure shows minimum wages for the workers of firms remaining in Confcommercio (in solid blue lines) and for the Federdistribuzione firms that opt out of the collective agreement (in red dashed lines) between 2005 and 2020. Three out of eight occupational levels are displayed. The green dashed lines depict the minimum wages enforced by FD firms with unilateral raises.

This period of divergence in wage floors – and of adversarial industrial relations – lasted until January 2019, when a new FD contract went into force (signed on December 19, 2018). While formally a separate agreement, this contract replicated the wage floors of the prevailing CC-CBA (as well as most other provisions), and hence the two lines in Figure 3.2 converge again in 2019. While FD and CC continue to bargain separately, the 2022 CBAs for both associations remain close on all dimensions, including wage floors.

3.4 Data

We now describe the linked datasets on which we base our study: matched employer-employee data and CBA data. We then provide details on how we identify the opt-out events described in the previous section in these datasets.

Matched Employer-Employee Data. We draw on data for the universe of worker-firm matches in the private sector between 2005 and 2019, on earnings, weeks worked, type of contract (part- versus full-time, temporary versus permanent), occupation (apprenticeship, blue-collar, white-collar, mid-management, manager), as well as demographic information such as date of birth and gender. These data – which we refer to as "INPS data" – are collected by the Italian Social Security Institute (INPS), which we access as VisitINPS Scholars. Crucially, unlike in most other settings, this dataset in Italy contains, for each job spell, the CBA that the firm applies, with a unique code that we describe next. This peculiarity, that firms have to report applicable CBAs, is owed exactly to the coverage rules in Italy, which require a firm to pay the worker according to the (representative) CBA.

Contract Data. All CBAs are recorded in the web archive of CNEL (National Council for Labor and Economic Policies), along with details including their signatory parties.[7] This allows us to track the diffusion of pirate agreements over time by looking at the number of CBAs not signed by the three dominant unions. Most importantly, we link this information with the administrative INPS data described above, using a cross walk:

INPS-CNEL Cross-walk. We merge the CNEL CBA data and the INPS data by drawing on a INPS-CNEL cross-walk provided by CNEL (also used in Daruich et al., 2023; Faia and Pezone, 2023).

Firm Financials. We also access key accounting variables for all nonfinancial incorporated firms between 2005 and 2018. These data are collected by the Cerved Group and can be merged to the INPS data using a unique national tax firm identifier (*codice fiscale*).

[7] The archive can be accessed at this link: `https://www.cnel.it/Archivio-Contratti`.

Identifying Pirate Agreements. Following Lucifora and Vigani (2021), we classify pirate agreements as those (many) CBAs in the INPS database that are not assigned a unique identifier but are classified as "Different Contract". By inspecting the CNEL records, we notice that virtually all these CBAs are signed by new, non-traditional unions.[8] We also define as pirate agreements those CBAs with a unique identifier in the INPS data (i.e., not recorded as "Different Contracts") but not signed by at least one of the three main unions in Italy according to the CNEL archive.[9] We provide additional details on the implementation in the econometric strategy in Section 3.5 below.

Identifying FD Firms. We obtained from FD the names of the firms that opted out of the CC-CBA in 2011. Using these company names, we manually match this list with the firm-level balance sheet data to retrieve their national tax identifiers. Out of the 56 firms in FD that opted out of the CC-CBA, we were able to match 52 in the INPS data.

3.5 Econometric Framework

This section presents the harmonized matched difference-in-differences (DiD) research design used to analyze the effects of the adoption of pirate agreements (Section 3.5.1) and of the FD opt-out event (Section 3.5.2). We then discuss the identification assumptions and economic predictions (Section 3.5.3).

3.5.1 Adoptions of Pirate Agreements

Specification. To study the effects of pirate agreements, we run a matched DiD design comparing workers subject to a *within-employer* transition from a regular

[8] The CNEL data show that there is a very low number of "Different Contracts" signed by the three main unions (around 30). These cannot be identified in the INPS data.

[9] In the latter case, we exclude sectors/occupations where traditional unions have not historically signed a national CBA. To give an example, managers in Italy have their own CBA. These manager-CBAs, however, are never signed by the three main Italian unions as they do not represent managers. Therefore, manager CBAs are not part of our definition of pirate CBAs.

CBA to a pirate CBA to observationally similar control workers who do not experience any CBA switch.[10] As the opt-out event can occur in different years, we implement a staggered DiD design. Our specification takes the following form:

$$y_{it} = \alpha_i + \delta_t + \sum_{k=a}^{b} \gamma_k \cdot \mathbb{1}[t = t_i^* + k] + \sum_{k=a, k\neq -1}^{b} \beta_k \cdot \mathbb{1}[t = t_i^* + k] \cdot T_i + v_{it}, \tag{3.1}$$

where y_{it} represents an outcome of individual i in year t (e.g., total earnings); α_i and δ_t represent worker and year fixed effects, respectively; T_i is an indicator equal to 1 for a worker i who experiences a within-employer transition to a pirate CBA and t_i^* represents the year in which that event takes place for that worker. For a control worker, this is the year of the adoption of the pirate agreement of the treated worker matched to this particular control worker (the next paragraph describes the matching algorithm). For the few treated workers who experience a within-employer CBA transition more than once over the 2005-2019 period, we refer to the first transition experienced by the worker.

The coefficients β_t represent the difference-in-differences coefficients of interest. We let $t_i^* \in [2008, 2009, \ldots, 2016]$ in order to have sufficient periods before and after each event (i.e., $a = -5$ and $b = 5$ in Equation (3.1)) and normalize the coefficients relative to β_{-1}. Standard errors are clustered at the level of the $t_i^* - 1$ employer.

Sample and Matching. To obtain a comparable set of workers, we implement a matching algorithm that assigns each treated worker to a similar control worker in the years prior to the opt-out event. The pool of potential control workers is represented by workers who have the same CBA as treated workers prior to the

[10]Specifically, the opt-out event occurs in month m if a worker is employed with a regular CBA in months $m-2$ and $m-1$ of the same spell but is then moved to a pirate CBA in month m and is still covered by that pirate CBA in month $m+1$. We remove from this set of transitions the ones that occurred in the automotive sector in 2012 and in the retail sector in 2015. The transitions observed in 2015 basically represent workers in FD firms, and the 2012 transitions capture the opt-out of the large car manufacturer Fiat, which, being an individual firm, VisitINPS privacy rules do not permit us to study. We also drop the (very few) spells in which at least two transitions of the same nature (e.g., standard-to-pirate) occur within 12 months or those where the worker returns to be hired under their regular CBA within the same calendar year.

transition to a pirate CBA (period $t_i^* - 1$) and were never employed at a firm that ever used a pirate CBA. We also exclude managers and apprentices as well as workers employed in agriculture and public administration (see Footnote 9). We also focus on the sample of workers with at least four years of labor market experience and three years of tenure with their $t_i^* - 1$ dominant employer, as typical in the job displacement literature (e.g., Bertheau et al., 2023; Schmieder et al., 2023).[11] We then implement a propensity score model for the probability of treatment (being subject to an opt-out), similar to what is done in, e.g., Goldschmidt and Schmieder (2017) or Jäger and Heining (2022). The model controls for region fixed effects and worker age, tenure, gender, contract type (temporary versus permanent, full- versus part-time), broad occupation, log weekly wage in the three years before the opt-out as well as firm size and sector. For each treated worker we pick a single control worker with the closest propensity score (without replacement).

Descriptives. Table 3.1 compares baseline characteristics of treated and control workers, before and after matching. Columns (1) and (2) show descriptives before matching. Column (1) reports treated workers' characteristics as of period $t_i^* - 1$ (i.e., workers as of that year under a standard CBA that will transition to a pirate CBA in the next year). Column (2) does so for other workers – those under a standard CBA that will not undergo treatment (and draws on years 2008-2016). Treated workers feature a larger share of women, part-time and white collars. Columns (3) and (4) show the same descriptives for the matched sample used to fit Equation (3.1). Out of the almost 200,000 within-spell pirate agreement opt-outs between 2008 and 2016, we match around 25,000.

[11] A dominant employer is defined as the employer that employed a worker for the most weeks within a given year.

Table 3.1. Pirate Agreement Adoptions: Descriptive Statistics of Workers

	Full sample		Matched sample	
	(1) Treated	(2) Controls	(3) Treated	(4) Controls
Woman	0.47	0.41	0.53	0.53
	(0.50)	(0.49)	(0.50)	(0.50)
Age	39.32	39.52	41.51	41.48
	(10.14)	(10.91)	(9.33)	(9.58)
Full-time	0.71	0.75	0.68	0.67
	(0.46)	(0.44)	(0.47)	(0.47)
Temporary contract	0.11	0.19	0.05	0.06
	(0.31)	(0.39)	(0.23)	(0.23)
Log weekly wage	6.07	6.03	6.01	6.00
	(0.49)	(0.51)	(0.42)	(0.45)
Blue collar	0.40	0.56	0.48	0.48
	(0.49)	(0.50)	(0.50)	(0.50)
White collar	0.51	0.35	0.49	0.48
	(0.50)	(0.48)	(0.50)	(0.50)
Mid manager	0.04	0.03	0.03	0.04
	(0.28)	(0.17)	(0.18)	(0.19)
Firm size	5,976	3,122	179	176
	(8,545)	(15,029)	(547)	(1,046)
South	0.21	0.24	0.28	0.27
	(0.41)	(0.43)	(0.45)	(0.45)
N. observations	199,429	116,749,156	24,952	24,952

Notes: This table reports descriptive statistics averaged between 2008 and 2016 by group. Columns (1) and (2) refer to the sample before the matching. In particular, Column (1) reports t^*-1 descriptives for workers on a standard CBA that will experience a within-job transition to a pirate CBA at t^*; Column (2) reports descriptives for potential control workers, that is, workers never covered by a pirate CBA. Columns (3) and (4) show descriptives for the matched sample, obtained as described in Section 3.5.1. Standard deviations are reported in parentheses. Firm size is worker-weighted. Log weekly wages are expressed in log euros.

3.5.2 The 2011 Secession of Mass Retailers

Specification. We study the effects of the FD opt-out with a similar design as in the pirate CBA analysis. Namely, we compare a worker employed in a FD firm in 2010 with a similar worker employed by a firm that applies the CC-CBA in 2010 and that has not opted-out from this CBA (henceforth a CC firm). We thus fit regression equations of the following type:

$$y_{it} = \alpha_i + \delta_t + \sum_{t \neq 2010} \beta_t \cdot FD_i + v_{it}, \tag{3.2}$$

here, FD_i represents an indicator equal to 1 if the dominant employer of worker i in the year 2010 is an FD firm. We normalize the coefficients of interest β_t relative to the year 2010, i.e., the year before the opt-out decision of FD firms. When fitting Equation (3.2), we cluster standard errors at the level of the 2010 employer.

Sample and Matching. The sample used to fit Equation (3.2) is represented by the set of workers employed by either a FD firm or a CC firm in 2010 who are similar along many observable characteristics up to the moment of the opt-out decision made by FD firms. To construct this sample, we follow the same approach as described above and fit a propensity score for the probability of being employed by an FD firm in 2010. Again, we restrict the sample to workers with at least four years of labor market experience and three years of tenure with their 2010 dominant employer. Differently from the pirate CBA design, we restrict the analysis to firms that had at least 15 employees in 2010.[12] The propensity score model comprises the same covariates as in the pirate CBA design, but does not include firm size (because doing so would lead to large violations of the overlap conditions) and sector (as this analysis focuses on the retail sector only).

Descriptives. Table 3.2 presents summary statistics by samples, before and after matching. Columns (1) and (2) present the average characteristics of workers who by 2010 have at least three years of experience in the labor market, have at least two years of tenure with their 2010 dominant employer and are employed by a FD firm (Column (1)) or by a CC firm (Column (2)). FD firms are much larger than CC firms. This reflects the context of the opt-out decision: FD firms are

[12]This restriction is imposed to reduce the imbalances in firm size that exists between FD firms and the typical firm that applies a CC-CBA, see also Table 3.2. We use 15 as a cutoff because this is the typical cutoff also by labor laws, e.g., Kugler and Pica (2008).

large retailers that want to escape a CBA designed for small and medium-sized firms. FD firms are also more intensive on the usage of part-time contracts which can, perhaps, also explain why we also see a higher fraction of women among FD firms. Interestingly, however, there are some margins where both FD and CC firms appear similar, such as wages. Columns (3) and (4) of Table 3.2 show the characteristics of the matched sample used to fit Equation (3.2). Matched workers are about 90% of the original sample of workers hired by FD firms and are very similar to the general population of FD employees.

Table 3.2. The 2011 Secession of Mass Retailers: Descriptive Statistics of Workers

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
	Treated	Control	Treated	Control
Woman	0.60	0.47	0.59	0.58
	(0.49)	(0.50)	(0.49)	(0.49)
Age	38.18	39.35	38.94	38.81
	(8.48)	(9.09)	(8.33)	(8.39)
Full time	0.58	0.82	0.63	0.63
	(0.49)	(0.38)	(0.48)	(0.48)
Temporary contract	0.01	0.02	0.01	0.01
	(0.09)	(0.12)	(0.09)	(0.09)
Log weekly wage	6.19	6.25	6.21	6.21
	(0.31)	(0.38)	(0.31)	(0.35)
Blue collar	0.07	0.35	0.08	0.08
	(0.26)	(0.48)	(0.26)	(0.27)
White collar	0.90	0.61	0.90	0.89
	(0.29)	(0.49)	(0.3)	(0.31)
Mid manager	0.16	0.20	0.16	0.16
	(0.16)	(0.20)	(0.16)	(0.16)
Firm size	3,292	62	3,292	69
	(4,516)	(204)	(4,516)	(227)
South	0.15	0.18	0.15	0.14
	(0.36)	(0.38)	(0.36)	(0.35)
N. observations	102,911	419,448	91,753	91,753

Notes: This table reports averages of the characteristics by group: workers in FD firms in 2010, and those in CC firms (but not FD) in 2010. Standard deviations are reported in parentheses. Statistics are reported separately before (Columns (1)-(2)) and after (Columns (3)-(4)) matching. Firm size is firm-weighted. Log weekly wages are expressed in log euros.

3.5.3 Discussion: Identification Assumptions and Predictions

The research designs just described focus on *incumbent* workers. These are workers who are employed with a given firm prior to the opt-out event but are then hit with a "CBA shock" as their employer decides to opt out of centralized CBAs. This follows a line of inquiry in labor economics that analyzes how a firm-level decision (e.g. a mass layoff as in Jacobson et al., 1993 or an outsourcing event as in Goldschmidt and Schmieder, 2017) impacts workers. We now provide details on the identifying assumptions needed for causal interpretation of our results, discuss potential economic mechanisms and firm-level effects.

Identification. Causal interpretation of the difference-in-differences coefficients β_t in (3.1) and (3.2) relies on a parallel trends assumption. For example, in the context of the FD event, this amounts to assuming that differences in outcomes between workers employed by a FD firm in 2010 and those employed under a CC-CBA would have remained constant in the counterfactual scenario where FD firms decided to remain with the CC-CBA. A similar assumption is needed to evaluate the effects of the adoption of pirate CBAs. To assess the plausibility of this assumption, we are going to evaluate the evolution of outcomes in the years preceding the opt-out decision, with a particular focus on outcomes/time periods not directly targeted by the propensity score.

Economic Channels. The decision to opt-out is most likely taken by firms to pay lower wages and have more flexibility in determining other key job attributes, such as hours. The degree to which an individual firm opting out of a centralized CBA can actually lower wages depends on the structure and competitiveness of the labor market. In a perfectly competitive and frictionless labor market, an individual firm that lowers its wage from the prevailing (CBA) wage paid by the other firms will lose all employment as effectively the firm-specific labor supply curve is infinitely elastic, and hence will keep its wage unchanged (or not opt out to begin with). There are related implications for employment as an outcome variable. In the presence of labor-demand-determined employment, as would emerge when labor supply is rationed (consistent with high unemployment in Italy, for instance), a wage reduction would go along with higher employment stability.[13] Indeed, this trade-off between lower wages and higher employment is at the heart of the discussion regarding the costs/benefits of centralized collective bargaining agreements (Dustmann et al., 2009; Card et al., 2013; Boeri et al., 2021). However, employment at these firms might actually *decrease* if the (firm-specific) labor supply channel dominates: firms with increasing wage-setting power due to the new CBA can now move along their supply curve and target a combination with lower wages and lower employment. This is the mirror image of the monopsony result that increases in the minimum wage can lead to higher employment (Card and Krueger, 1994).

Additional Perspective: Firm-Level Analysis. Our empirical analysis assesses how opt-outs affect the workers directly impacted by these decisions. A related question of interest is what happens to firms opting-out from a centralized CBA. Having a potentially more flexible CBA can result in increases in efficiency not directly captured by the sole analysis of incumbent workers. We will thus present firm-level effects of the adoption of pirate agreements based on a matched difference-in-differences research design that mimics Equation (3.1) but conducted at the firm level, see Appendix 3.C for details.[14] This design requires stronger assumptions for its validity as we are now analyzing how an endogenous decision made by firms impacts their outcomes. Nevertheless, we view this analysis as still informative in painting a more comprehensive picture of the overall effects of opting out and also in providing the firm-level context for our main findings.

[13]The model in Saez et al. (2019) formalizes a similar rationale for why in the presence of unemployment and rationed labor supply, a labor cost reduction can raise employment at the firm level without a wage increase (consistent with the evidence presented there), although the payroll tax variation in that paper left workers' take-home pay unaffected.

[14]The small number of treated firms in the FD opt-out makes firm-level analysis for this event hardly feasible. We thus focus only on pirate agreements for the firm-level analysis.

3.6 Results

We now present the results, primarily focusing on the effects of the opt-out events on worker wages, employment, and earnings, and following the economic predictions described in Section 3.5.3 above. Section 3.6.1 does so for the pirate CBA adoptions using a staggered difference-in-differences design; in Section 3.6.2 we turn to the FD secession. Last, we look for heterogeneous patterns in the results.

3.6.1 Adoptions of Pirate Agreements

Figure 3.3 shows the effects of the adoption of pirate CBAs, as estimated in the staggered design of Equation (3.1). Table 3.3 shows the full associated regression results, focusing on the on-impact coefficient (t^*), the medium run (t^*+2), and the longer run (t^*+5), along with additional information including a formal pre-period test and control means.

Wages. Following our discussion of economic mechanisms in Section 3.5.3 above, we start by analyzing the effect of the opt-out event on wages – the primary channel through which opt-outs affect labor market outcomes. Figure 3.3 Panel (a) (and Table 3.3 Column (1)) shows that workers experience a decrease in log weekly wages when their employer opts out and moves them into a pirate agreement, compared to the evolution of matched control workers' wages.

The wage loss is about 2 log points. The negative wage effect remains stable at around 2.5 log points in the years following the opt-out.[15] Panel (a) additionally shows that the wage effects are indeed experienced by stayers – i.e., individuals that remain with their opting-out employer and are thus mechanically exposed to the differences in wage floors following the opt-out decision.[16] This indicates

[15] We will be able to estimate the wage pass-through in the FD opt-out. Doing so is more complex for the pirate CBA design, as we cannot observe wage floors for most pirate agreements and, as a result, we cannot properly quantify the drop in negotiated wage floors.

[16] To minimize selection issues arising from imposing a restriction based on an outcome affected by the treatment of the interest – remaining at the (t^*-1) employer – we fit Equation (3.1) just among pairs of matched treated and control workers where both the treated worker and their matched control are still employed by their (t^*-1) employer.

that the opt-out brings about a shift in the wage policy of the firms that opted out, rather than reflecting compositional effects through turnover to new employers.

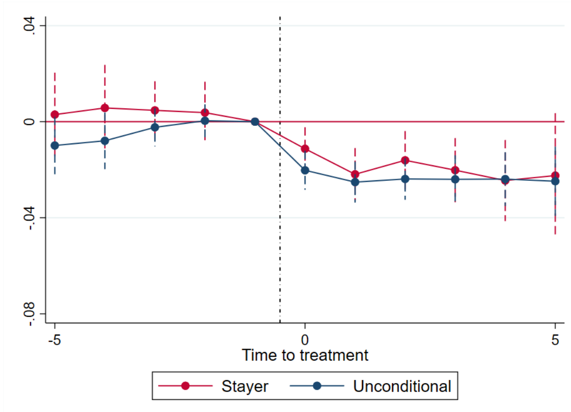
Employment. Overall, the negative response of wages is consistent with the basic economics of opting out discussed above in Section 3.5.3: firms make use of the flexibility that comes with opting out, and a key dimension of such flexibility is to downward-adjust wages. However, the degree to which firms can do this depends on the effects these adjustments have on employment. If firms face perfectly elastic labor supply, then firms attempting to lower wages would lose all employment. Our wage results do not suggest this pattern, as firms do appear to lower wages. However, the Italian context – of high unemployment potentially described by classical rationing of labor supply – raises another possibility: that employment may *go up* even when wages fall, as firms are more willing to retain workers in the face of, e.g., idiosyncratic shocks or as part of overall increasing labor demand. We therefore now turn to employment results.

Figure 3.3 Panel (b) and Table 3.3 Column (2) report the estimates of the effects on employment (defined as an indicator for having at least one day of recorded employment in a given calendar year). We estimate a *positive* employment effect, indicating that workers are more likely to stay employed if they are exposed to an opt-out that lowered their wage. The effect amounts to about 3 percentage points three years after the event and persists over time. This effect appears in part driven by a higher – but not precisely estimated – probability of staying with the opting-out firm (Panel (c) and Column (3)).

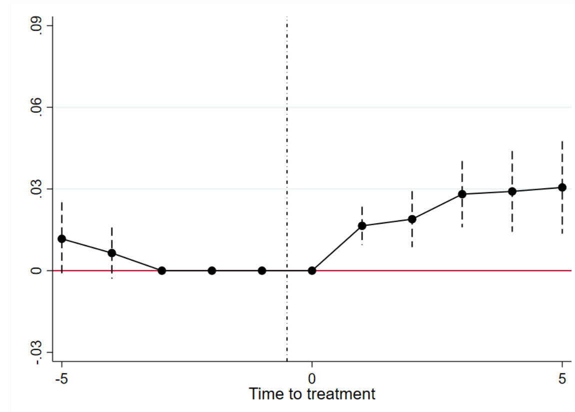
Labor Earnings. We now turn to our most comprehensive analysis of labor market outcomes for workers: total labor earnings. This perspective lets us, for instance, determine the net impact of negative wage effects (intensive margin) and positive employment and retention effects (extensive margin). That is, we now turn to earnings levels (rather than logs), assigning zero earnings to workers not employed in the private sector in a given year according to the INPS data and considering total annual income for all other workers (irrespective of where they work), in 2010 euros. The results are displayed in Panel (d) of Figure 3.3 (and Table 3.3 Column (4)). For this outcome measure, we find an overall neutral effect of the opt-out.

Figure 3.3. The Effects of Pirate Agreement Adoptions on Workers

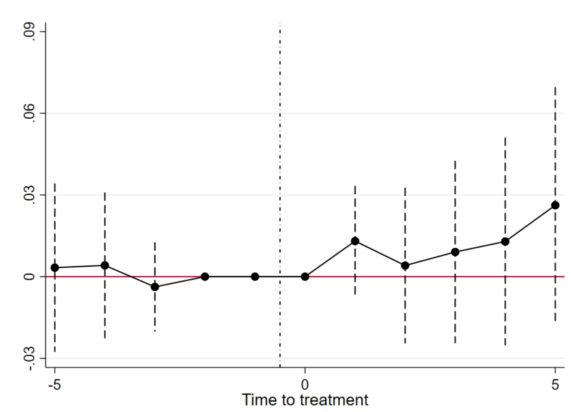
(a) Log Weekly Wages



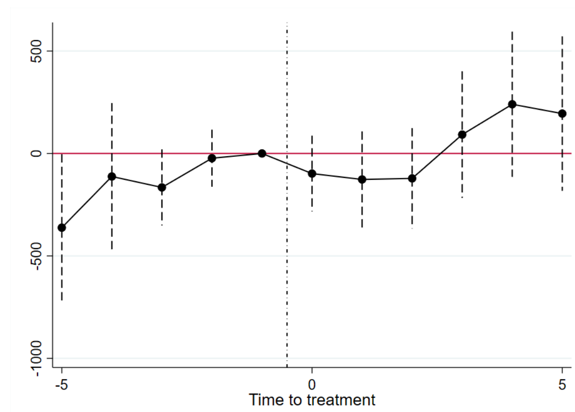
(b) Employed



(c) Employed at $(t^* - 1)$ Firm



(d) Earnings



Notes: This figure displays the event-study coefficients from Equation (3.1) estimated on the matched sample defined in Section 3.5.1. Panel (a): Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. We report these effects unconditionally, and additionally conditioning on the worker staying with their $t^* - 1$ employer. To construct the latter, we fit Equation (3.1) just among pairs of matched treated and control workers where they are both still employed with their of the $t^* - 1$ employer. All other outcomes in this figure are for the unconditional sample. Panel (b): Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Panel (c): the outcome is an indicator equal to 1 if a given worker in year t is employed by their $t^* - 1$ employer, where t^* denotes the year of transition to a pirate agreement. Panel (d): Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Table 3.3 reports these event-study coefficients along with additional summary statistics. Standard errors are clustered at the level of the $t^* - 1$ employer.

Interim Summary. Overall, it thus appears that the pirate opt-out decisions led to a labor demand based adjustment, with wages of incumbent workers decreasing – an effect driven by workers who stay with their opting-out employer – with their employment probability going up. These two effects approximately cancel each other out as treated workers do not appear to earn systematically more on average after the opting-out decision. The results are not consistent with opting-out firms facing an elastic firm-specific labor supply curve, but instead can be better rationalized by a model with rationed labor supply, where firms can hire more workers even though they lower wages.

Table 3.3. The Effects of Pirate Agreement Adoptions on Workers

	(1) Employed	(2) Employed at $(t^* - 1)$ Firm	(3) Log Weekly Wages	(4) Earnings
On Impact (t^*)	0.00	0.00	-0.02	-98.19
	(0.00)	(0.00)	(0.00)***	(94.40)
Medium Run $(t^* + 2)$	0.02	0.00	-0.02	-121.73
	(0.00)***	(0.01)	(0.00)***	(135.26)
Long Run $(t^* + 5)$	0.03	0.03	-0.02	194.57
	(0.01)***	(0.02)	(0.01)***	(192.38)
N. observations	521,022	521,022	477,920	521,022
Mean Outcome	0.94	0.81	6.001	18,561
Average of Pre-Event Coeffs	0.00	0.00	-0.00	-165.89
	(0.00)	(0.01)	(0.00)	(111.18)
p-value Pre-Event Coeffs = 0	0.11	0.92	0.22	0.14

Notes: This table reports three event-study coefficients from Equation (3.1) estimated on the matched sample defined in Section 3.5.1. 'On Impact' denotes the coefficient estimated in the opt-out year t^*, 'Medium Run' corresponds to the coefficient two years after the opt-out, and 'Long Run' refers to the coefficient five years after the opt-out. 'Mean Outcome' represents the mean of the outcome of interest in the five years before the opt-out, computed considering the control group only. The last rows show the mean event-study coefficient in the five years before the opt-out ('Average of Pre-Event Coeffs'), the standard error in parentheses, and the p-value of the test that the mean pre-event coefficient is equal to zero. Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Employed at $(t^* - 1)$ firm is an indicator equal to 1 if a given worker in year t is employed by their $t^* - 1$ employer. Log weekly wages are calculated for the dominant job, i.e., the job with higher weeks worked in a given year. Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010-euros. Standard errors, clustered at the firm level of the $t^* - 1$ employer, are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

3.6.2 The 2011 Secession of Mass Retailers

The pirate agreement adoptions are an important and ongoing facet of the erosion of centralized collective bargaining in Italy and provide us with a large sample of firm events. We now turn to the analysis of a prominent case-study – the FD opt-out – where we can dig more deeply into the reasons behind the opt-out decision and alleviate possible endogeneity concerns. Overall, we find strikingly similar patterns as in the pirate opt-out, both qualitatively and quantitatively.

Figure 3.4 reports the DiD effects of the FD opt-out on the cohort of workers employed at the FD firms in 2010. Complementing the year-specific DiD coefficient plot, Table 3.4 reports full regression results, for three horizons: on-impact results for the 2011 coefficient, the medium run for year 2013, and the longer run for year 2016.

Wages. Panel (a) of Figure 3.4 (and Table 3.4 Column (1)) study log weekly wages. We describe the dynamics across the three time windows in which the FD opt-out occurred – see Figure 3.2 and the account of the event in Section 3.3.2.

Pre-2011, we do not see any differential wage evolution, implying parallel pretrends and supporting our identification strategy (the informative window is 2005-2007, since we match on 2008-2010). In the first post-period time window, right after the 2011 opt-out, wages start to decrease. Effect sizes in this time window are moderate (around 1 log point) and imprecisely estimated. The bulk of the wage decrease is found post-2015, i.e., after the control firms signed the new CC-CBA with higher wage floors while FD firms had opted out and when wage floors start to diverge (see Figure 3.2). The decrease in wages is around 4 percent. Assuming an average drop in wage floors post opt-out of 7 percent (see Figure 3.2), such a drop in actually paid wages implies a pass-through of roughly 57 percent (similar to pass-through estimates stemming from non-opt-out wage floor variation in Portugal and Italy by Card and Cardoso, 2022; Fanfani, 2023). Again, the wage effect is driven by stayers at their 2010 employer, consistent with switches in the opting-out firms' wage policies following the opt-out event. As for the last time window, the effect of the opt-out appears to narrow somewhat in the least year of our sample, 2019, which is when FD firms effectively re-adopted the CC-CBA (see Section 3.3.2). Yet, even after the realignment in wage floors, workers subject to the FD opt-out still experience wage losses of about 2 percent.[17]

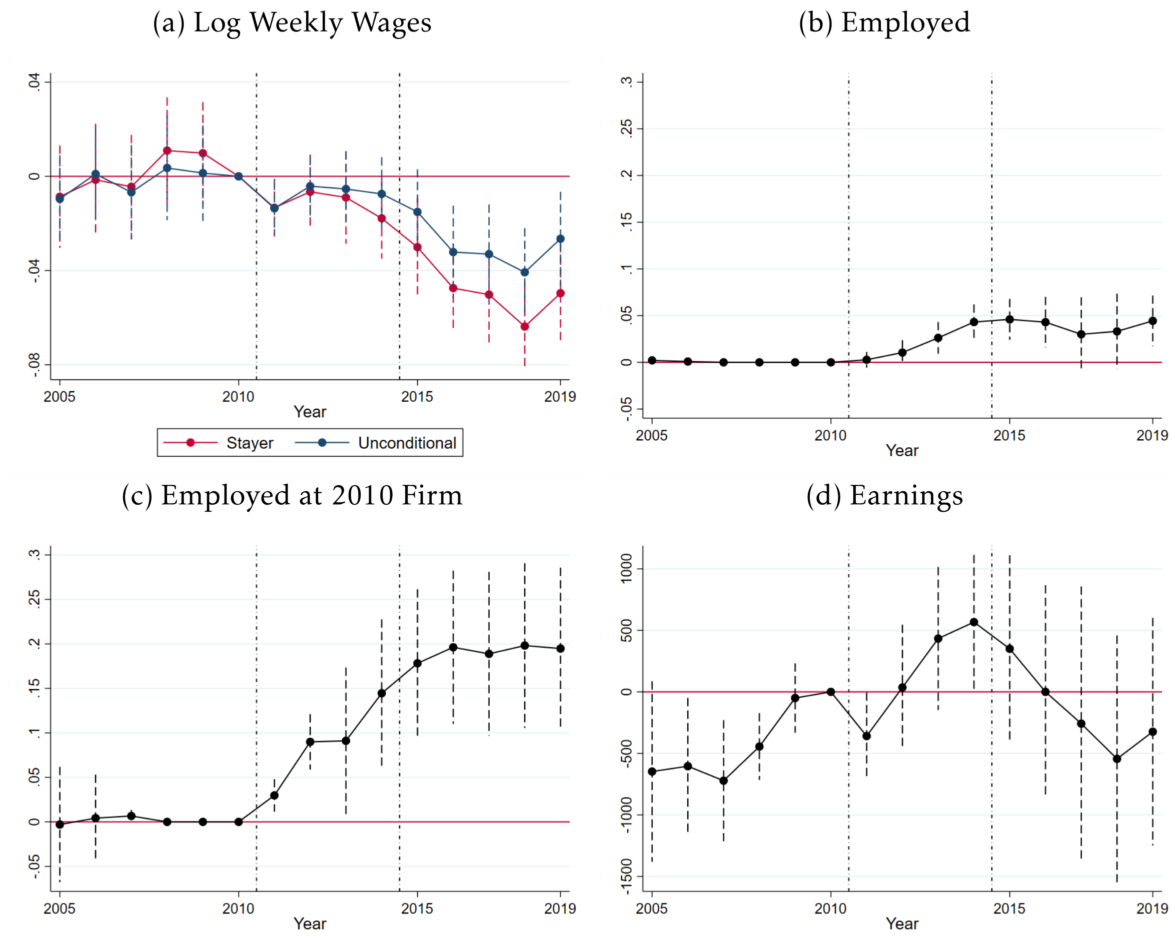
Employment. Panel (b) of Figure 3.4 and Table 3.4 Column (2) report the estimated the effects on workers' employment probability. Similarly to the pirate agreements analysis, we observe a positive impact on employment. The effect size is 3-4 percentage points. It starts emerging already in the "interim period" between 2011 and 2014, which corresponds to the years when FD opted out from the CC-CBA but before a new CC-CBA was signed (which occurred in March 2015). This initial effect can reflect responses to the moderate wage effects we documented above in the same time. Or, recognizing the long-term nature of employment relations in Italy where labor demand depends on the expected present value of labor costs, we can interpret the employment effects as capturing firms' optimal retention of workers in expectation of wage cuts.[18] The significant positive employment effect persists at a relatively stable level post-2015 through the end of our sample period, albeit somewhat less precisely estimated.

Again, we ask whether the effect is driven by interactions with the opting-out employer. Figure 3.4 Panel (c) (and Table 3.4 Column (3)) shows that the positive employment effect of the FD opt-out is driven by a significant increase in the probability of staying at the 2010 employer. The point estimate is around 15 percentage points, despite not seeing any significant differences in these outcomes even prior to 2008, when no tenure or employment restrictions were imposed.

[17] Appendix Figure B3.2 Panel (a) shows robustness of our results to dropping municipalities exposed to a contemporaneous reform of shopping hours (Decree 201/2011, *Salva Italia*, see Rizzica et al., 2023), which might have differentially affected the large FD retailers (a staggered liberalization of shop hours for municipalities most reliant on tourism had already started in 1998, which gives us a set of observations unaffected by the reform – we thank the authors of Rizzica et al. (2023) for sharing these data). Panel (b) of Appendix Figure B3.2 shows robustness to a dramatically weaker sample restriction, imposing a one-year tenure restriction only (rather than a four-year employment restriction with a three-year tenure restriction).

[18] Returning to implications for the wage effects, we speculate that the workers retained due to the opt-out may plausibly be "marginal" employees, explaining why we detect a smaller negative effect on wages prior to the signing of the new CBA, as discussed above.

Figure 3.4. The Effects of the 2011 Secession of Mass Retail Employers on Workers



Notes: This figure displays the event-study coefficients from Equation (3.2) estimated on the matched sample defined in Section 3.5.2. Panel (a): Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. We report these effects unconditionally, and additionally conditioning on the worker staying with their 2010 employer. To construct the latter, we fit Equation (3.2) just among pairs of matched treated and control workers where they are both still employed with their 2010 employer. All other outcomes in this figure are for the unconditional sample. Panel (b): Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Panel (c): the outcome is an indicator equal to 1 if a given worker in year t is employed by their 2010 employer. Panel (d): Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Table 3.4 reports these event-study coefficients along with additional summary statistics. The two vertical lines correspond to 2011—the year when FD abandoned the CC employer organization— and 2015, the year when a new CC-CBA was signed. Standard errors are clustered at the level of the 2010 employer.

Labor Earnings. As in the pirate adoptions event, workers' earnings seem unaffected by the FD opt-out, consistent with the idea of the positive employment effect canceling with the negative effect on wages – see Panel (d) of Figure 3.4 (and Table 3.4 Column (4)). Estimates are overall insignificant for the post-opt-out years, albeit with a slight positive upward trend in the pre-event years.

Table 3.4. The Effects of the 2011 Secession of Mass Retail Employers on Workers

	(1) Employed	(2) Employed at 2010 Firm	(3) Log Weekly Wages	(4) Earnings
On Impact	0.00	0.03	-0.01	-358.97
	(0.00)	(0.01)***	(0.00)**	(180.90)**
Medium Run	0.03	0.09	-0.00	-433.41
	(0.01)***	(0.04)**	(0.01)	(296.56)
Long Run	0.04	0.20	-0.03	0.03
	(0.01)***	(0.04)***	(0.01)***	(442.35)
N. observations	2,732,810	2,732,810	2,523,222	2,732,810
Mean Outcome	1.00	0.932	6.14	22,283.64
Average of Pre-Event Coeffs	0.00	.00	-0.00	-493.36
	(0.00)***	(0.02)	(0.01)	(207.92)**
p-value Pre-Event Coeffs = 0	0.01	0.89	0.80	0.02

Notes: This table reports three event-study coefficients from Equation (3.2) estimated on the matched sample defined in Section 3.5.2. 'On Impact' denotes the coefficient estimated for the year 2011, 'Medium Run' corresponds to the coefficient for the year 2013, and 'Long Run' refers to the coefficient for the year 2016. 'Mean Outcome' represents the mean of the outcome of interest over the period 2005-2010, computed considering the control group only. The last rows show the mean event-study coefficient between 2005 and 2010 ('Average of Pre-Event Coeffs'), the standard error in parentheses, and the p-value of the test that the mean pre-event coefficient is equal to zero. Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Employed at 2010 firm is an indicator equal to 1 if a given worker in year t is employed by their 2010 employer. Log weekly wages are calculated for the dominant job, i.e., the job with higher weeks worked in a given year. Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010-euros. Standard errors, clustered at the firm level of the 2010 employer, are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Interim Summary. The FD opt-out event is associated with strikingly similar effects for workers as in the pirate CBAs adoptions: lower wages, higher employment probability, and on net statistically insignificant earnings effects. Our set of results plausibly reflect effects that would be expected in an environment were wage floors from national CBAs are too high, above the level that some firms (those that opt out, for instance) would set if free to do so. The fact that the wage reductions are accompanied by, if anything, positive rather than negative employment effects suggests that at least in the context of Italy and for the sample of treated firms that opt out, labor supply to the firm is inelastic, and rationed.

Additional Results: Firm-Level Effects. We complement the worker-level evidence with a (pirate agreement) firm-level analog, which we detail in Appendix 3.C and present in Appendix Figure C3.1. This firm-level analysis provides an assessment of the effects on the full workforce rather than initial incumbents. We observe a decline in labor costs of about 3 percent immediately following the opt-out, which remains stable in the ensuing years. This reduction in firm labor costs is mirrored by a larger survival probability for treated firms relative to control firms in the opt-out year of about 4 percentage points. This probability remains positive for two years after the opt-out but declines towards zero afterward. We find a positive effect on firm size (log employment), but significant pretrends do not permit a causal interpretation.[19] This result – with the caveats on the firm-level design mentioned above in Section 3.5.3 – leaves room for the interpretation that the positive employment effects are driven by stayers while the hiring margin faces more elastic labor supply. Finally, we find an overall null effect on profits per worker (with wide confidence intervals).

3.7 Potential Mechanisms: Heterogeneity Analyses

We close our analysis by exploring how the effects of opting out vary with firm size and geographical location. Because FD firms are very few and tend to have limited variability in the margins we explore in this Section, we conduct this additional analysis only for the pirate CBAs.

[19] After netting out this linear pre-trend (see Dustmann et al., 2022, for a similar approach), it seems that the opt-out decision had overall a small positive impact on employment in the opt-out year, which does not persist in time and turns negative.

3.7.1 Small Versus Large Firms

Figure 3.5 reports our first heterogeneity analysis by firm size (employment). We split treated workers into those employed at small versus large firms, choosing a cutoff of 15 employees in the year before the opt-out. For each treated worker, we retain their matched control.

Workers in firms above 15 employees suffer a significantly larger wage loss than workers in small firms following the opt-out (Panel (a)). Conversely, workers employed in small firms experience insignificant changes in wages. Panel (b) shows that the employment effect of opt-outs is concentrated in firms above the size threshold, with treated workers being 5 percentage points more likely to be employed five years after the opt-out, driven by retention with the initial employer (Panel (c) in Figure 3.5). In contrast, treated workers in small firms are about 3 percentage points *less* likely to be employed five years after the opt-out, despite a smaller wage decline. Hence, on net, earnings effects are zero to positive in large firms, but negative in small firms (Panel (d)).

There are at least two potential explanations for this heterogeneity. First, the employment cutoff we have chosen is a natural one in Italy because labor laws impose more stringent employment protection rules from this firm size onward (Kugler and Pica, 2008; Boeri, 2011).[20] This shift may interact with CBAs, which also regulate aspects related to employment protection (Daruich et al., 2023).[21] This evidence is still suggestive, and our results might also reflect differential effects by firm size that are unrelated to the change in employment protection legislation. A possible rationale for the negative effect of opting-out on employment among workers employed by small firms is that these employers face small de-

[20]Specifically, these firms are obliged to reinstate workers on permanent contracts in case of "unfair" dismissals. This rule was relaxed in March 2015, when reinstatement was replaced by a severance payment, but this new law applied only to new hires (Boeri and Garibaldi, 2019). Moreover, firms with more than 15 employees face higher firing costs even after the reform because severance payments in case of dismissals are higher.

[21]Opting-out from centralized CBAs can be particularly advantageous for firms facing rigid employment protection rules. The wage flexibility from the adoption of a pirate CBA can increase the likelihood of adopting firms to survive or to convert temporary contracts into permanent ones, thus resulting in higher employment probabilities for incumbent workers (Daruich et al., 2023).

grees of labor market competition – the mirror image of the result that increases in the minimum wage can have positive effects on employment (Card and Krueger, 1994). We discuss this possibility further below.

Figure 3.5. The Effects of Pirate Agreements on Workers: Heterogeneity by Firm Size

Notes: This figure displays the event-study coefficients from Equation (3.1) estimated on the matched sample defined in Section 3.5.1, separately for workers employed at firms below and above 15 employees at time t^*-1. Panel (a): Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. Panel (b): Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Panel (c): the outcome is an indicator equal to 1 if a given worker in year t is employed by their t^*-1 employer, where t^* denotes the year of transition to a pirate agreement. Panel (d): Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Standard errors are clustered at the level of the t^*-1 employer.

3.7.2 North vs. South

The centralized system of collective bargaining agreements has been viewed as one of the possible causes for the low levels of employment found in the South of Italy (Boeri et al., 2021).[22] The rationale for this argument is based on the observation that firms in the South of Italy tend to be less productive, and that the wage floors set nationally by centralized CBAs may be too high for Southern firms. Consistent with this view, Dustmann et al. (2014) argue that the decentralization of CBAs implemented by Germany – which allowed firms to flexibly adjust their labor costs depending on the levels of productivity – was a key ingredient for the country's good economic performance in the 2010s.

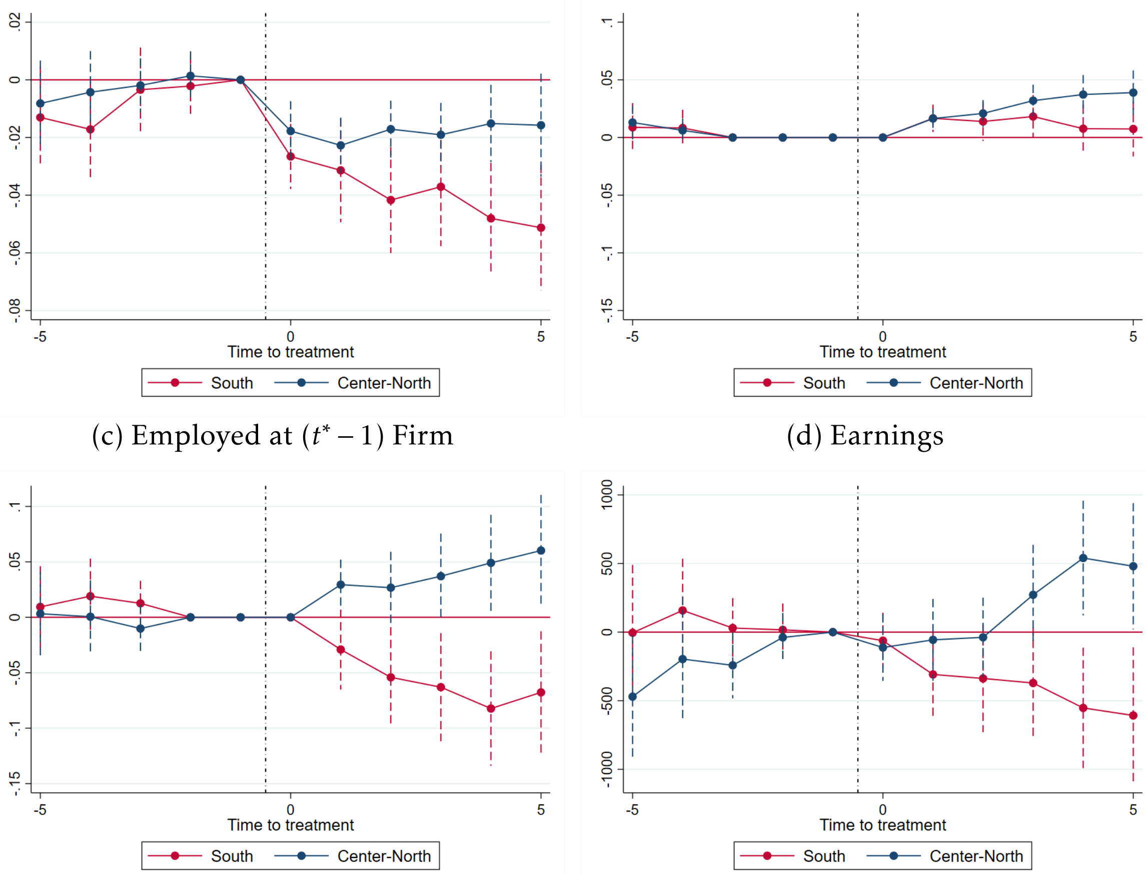
Figure 3.6 reports results by region (South and Center-North). In Panel (a) we detect larger and more precisely estimated negative wage effects for workers located in the South, consistent with wage floors being more binding in those regions. The employment effects are, if anything, lower in the South, where they are not significantly different from zero (Panel (b)). This is reflected in a *decline* in the probability of remaining with the same employer (5 percentage points five years after the opt-out event), contrasted by an increase in Northern regions of similar magnitude (Panel (c)). These results explain why earnings fall for workers employed in the South by about 500 euros at the end of our event window, whereas they rise by a similar amount for workers employed in the North (Panel (d)).

It appears, therefore, that workers in the South whose employer decided to opt-out of their centralized CBA *suffer* from this decision, as both their wages and employment are lower after the opt-out. Conversely, workers in the North see an increase in their employment probabilities and earnings. We emphasize that these findings refer to incumbent workers directly affected by the opt-out, and that the analysis does not uncover the *aggregate* effects of collective bargaining decentralization (e.g., on total employment in the local labor market).

[22] As of 2022, the employment rate in the South was just below 50 percent versus 70 percent in the North. The North-South employment gap has been rising over the past decades (De Philippis et al., 2022).

Figure 3.6. The Effects of Pirate Agreements on Workers: Heterogeneity by Geography

(a) Log Weekly Wages (b) Employed



Notes: This figure displays the event-study coefficients from equation (3.1) estimated on the matched sample defined in Section 3.5.1, separately for workers in the Center-North and South of Italy at time $t^* - 1$. Southern regions are Abruzzo, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily. Panel (a): Log weekly wages are calculated for the dominant job, i.e., the job with most weeks worked in a given year. Panel (b): Employed is an indicator equal to 1 if a given worker in year t has at least one day of employment according to social security records. Panel (c): the outcome is an indicator equal to 1 if a given worker in year t is employed by their $t^* - 1$ employer, where t^* denotes the year of transition to a pirate agreement. Panel (d): Earnings are calculated as the sum of labor earnings obtained by a worker in a given year and are expressed in 2010 euros. Standard errors are clustered at the level of the $t^* - 1$ employer.

To reconcile these findings, it is useful to note that the employment effect of opting-out might depend on the competitiveness of the labor market, as argued in 3.5.3. Opting-out from a centralized CBA may allow firms with wage-setting power to move along their supply curve and thus target an equilibrium allocation with both lower wages and employment. Therefore, if firms in the South tend to face, on average, *less* labor market competition, then differences in labor market competition faced by firms in the North compared to the South can provide a rationale for the findings of Figure 3.6.

3.8 Conclusion

Centralized collective bargaining regimes are common in many European countries. While often praised for redistributing productivity gains from firms to workers, they are also blamed for their rigidity. Unsurprisingly, there is intense debate about reforming collective bargaining frameworks by introducing additional flexibility to account for firm heterogeneity and local conditions. To date, however, very little is known about the effects of such reform on firms and workers. Focusing on Italy, a country characterized by particularly rigid industrial relations that came under intense scrutiny following the Great Recession, we analyze two types of opt-outs, one where firms left centralized collective bargaining agreements to reach arrangements with smaller and often local unions and another where a group of large employers renegotiated with national unions. We find evidence that workers subject to opt-outs suffer wage losses, but also experience higher employment stability. Additional heterogeneity checks uncover a more complex picture of the emerging cracks in the Italian system of industrial relation. In particular, our analysis suggests that firm opt-outs may have effects that depend on the level of competition in the local labor market.

We close by acknowledging and reiterating the limitations of our study. While our research design and data overcome a key challenge in the literature by identifying clear opt-out events at the level of individual firms and individual workers, we do not have at our disposal a definitively credible quasi-experiment. The ideal empirical setting would assign some firms the ability to opt out, while preventing others from doing so. As such, our study is subject to selection concerns, even though we have employed a rich matching strategy and have chosen our main focus to be on the effects on the initial cohort of workers employed at the firms.

Hence, we view our empirical study of the micro-level consequences of opting out as a complement to cross-sectional designs that use regional variation in productivity across Italy within a national wage setting system (e.g., Boeri et al., 2021).

3.A Appendix A

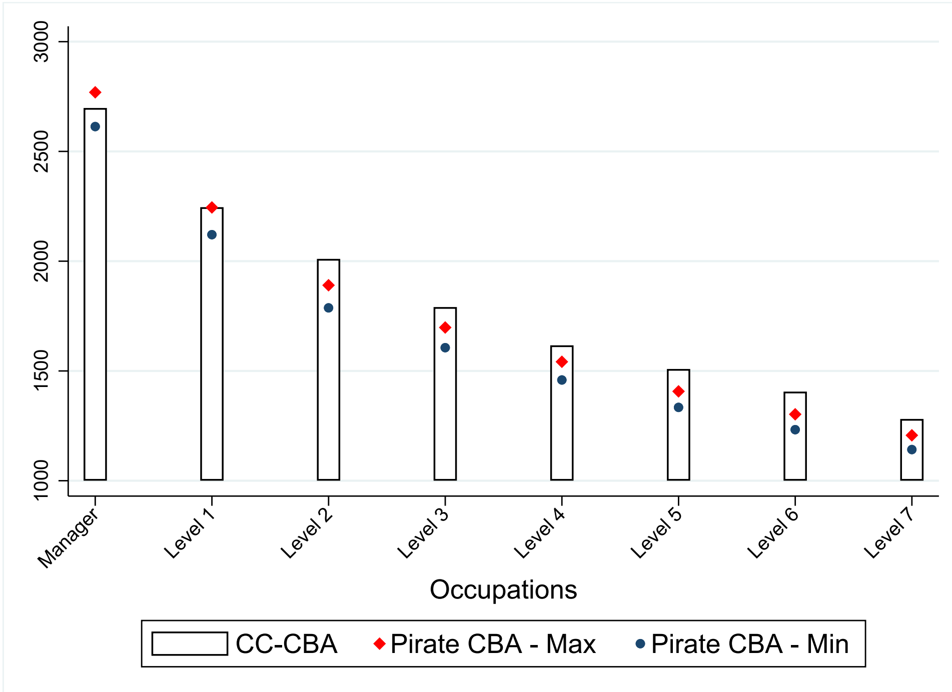
Here we present a case study comparing the CC-CBA, which is studied in the opt-out of mass retailers, to a pirate contract in the retail sector. We choose this sector as one of the most prominent pirate CBAs was signed there, covering roughly the same occupations as the corresponding standard CBA, which makes comparisons more meaningful. The CC-CBA (CNEL code H011) was first signed in 1967 by the dominant associations Confcommercio (employer) and CGIL, CISL and UIL (the three main unions). This contract had then been renewed periodically and became the "representative" CBA in the sector. In 2012, newborn employer and employee associations (Confazienda, Fedimpresa, Unica and Cisal) signed a new CBA in the sector (CNEL code H024), which by 2018 covered 731 firms and 12,000 workers (0.2 percent and 0.5 percent of the total number of firms and workers covered by the CC-CBA, respectively). Below is an excerpt of the text of the pirate contract:

> The old contracts prefer the death of companies and jobs rather than giving in, albeit marginally, to previous economic and regulatory achievements [...]. The system thus prefers to talk about "Pirate Contracts" whenever there is a search for a contractual solution compatible with the existing difficulties [...]. Any CBA that is not a bad copy of the corresponding text written down by the so-called "comparatively more representative trade unions at national level" is qualified as a "pirate" [...]. The knowledge of the market situation by all the parties involved (Companies, Workers, Trade Associations and Trade Unions) [...] is the only contractually possible way to effectively combat the crisis [...]. The Parties now find anachronistic the claim to define all the various contractual institutions and salaries in a homogeneous way for the entire national territory, which has many and significant heterogeneities [...]. The choice of this CBA is: (a) to lay down essential wages and standards which meet the primary needs of all workers; (b) to give priority to second-level bargaining; c) to recognize a Regional Equalization Element, proportionate to the Regional Cost of Living Indices, to reduce differences in purchasing power at the same nominal wage.

Notably, the signatory parties acknowledge the issue, posited in Boeri et al. (2021), that nominal wages should be adjusted to better reflect productivity levels across the country. To this purpose, a *Regional Equalization Element* is introduced on top of the national wage floor and larger in regions with a higher cost of living. Figure A3.1 provides a comparison of the wage floors envisaged by the CC-CBA to the floors introduced by the pirate CBA. Each CBA defines so-called *livelli di inquadramento* (granular occupations) and sets a wage floor for each level. While there is not necessarily a one-to-one correspondence between occupations across different CBAs (even if regulating the same sector), we inspect the contract texts to make sure that the occupation levels defined by these two contracts are broadly comparable. For the pirate CBA, two wage floors are depicted for each level representing the wage floor in the region with the highest (Lombardia, in the North) and lowest (Molise, in the South) Regional Equalization Element, respectively.[23] As expected, the wage floors set in the pirate CBA are lower than those in the CC-CBA, especially for occupations at the lower end of the wage distribution. Importantly, the advantages for firms when applying the pirate CBA extend to other aspects of the employment relationship, such as maternity leave. Law Decree n. 151/2001 imposes minimum maternity leave of five months (two before childbirth, three after) remunerated at 80 percent of pay. While the CC-CBA allows for longer maternity leave (up to five months after childbirth, at the mother's discretion) and envisages 100 percent remuneration, the pirate CBA does not extend the provisions set by the Law, "at least during the crisis".

[23]The Equalization Element amounts to roughly 5.3 percent of the (national) wage floor in Lombardia. In Molise, this percentage is 0.4 percent for managers up to 0.9 percent for the lowest occupation.

Appendix Figure A3.1. Wage Floors in the Wholesale and Retail Sector, 2018



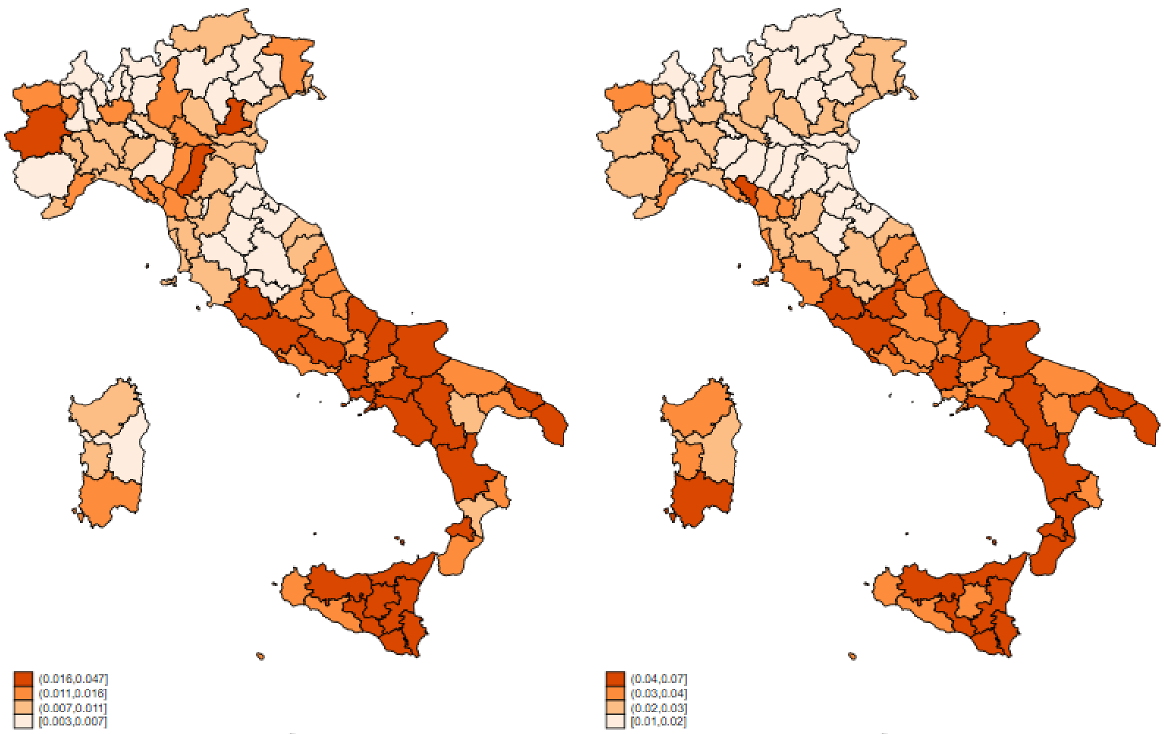
Notes: Wage floors across *livelli di inquadramento* for the CC-CBA (white bars) and pirate CBA (red and blue markers) in the wholesale and retail sector in 2018. For the pirate CBA, the top (red) marker is the wage floor for Lombardy (the region with the largest *Equalization element*) and the bottom (blue) marker is the wage floor for Molise (the region with the lowest *Equalization element*). See text for details.

3.B Appendix B

Appendix Figure B3.1. Geographical Distribution of Pirate Contracts, 2019

(a) Workers

(b) Firms



Notes: For workers, the share of pirate contracts is computed as the total number of workers covered by a pirate contract as a fraction of the total number of workers in the INPS data in each province in 2019. For firms, the share is computed as the number of firms applying a pirate contract to at least one employee as a share of the total number of firms in the INPS data in each province in 2019.

Appendix Table B3.1. Worker Characteristics by Adoption of Pirate Agreements

	(1) Pirate workers	(2) Other workers
Woman	0.47	0.41
	(0.50)	(0.49)
Age	39.67	39.46
	(10.70)	(10.89)
Full-time	0.67	0.74
	(0.47)	(0.44)
Temporary contract	0.18	0.19
	(0.38)	(0.39)
Log weekly wage	5.95	6.03
	(0.51)	(0.51)
Blue collar	0.50	0.56
	(0.50)	(0.50)
White collar	0.42	0.35
	(0.49)	(0.47)
Mid manager	0.03	0.03
	(0.18)	(0.17)
N. Observations	1,402,379	120,028,877

Notes: This table reports descriptive statistics of workers between 2008 and 2016. Column (1) refers to the sample of workers covered by a pirate agreement, column (2) to other workers. Standard deviations are reported in parentheses. Log weekly wages are expressed in log euros.

Appendix Table B3.2. Firm Characteristics by Adoption of Pirate Agreements

	(1) Pirate firms	(2) Other firms
Firm size	14.14	8.39
	(374.39)	(157.69)
Firm age	10.53	13.06
	(10.9)	(12.2)
Share of full-time	0.50	0.60
	(0.43)	(0.43)
Share of temporary	0.15	0.15
	(0.30)	(0.29)
Share of women	0.59	0.47
	(0.41)	(0.42)
Share of blue collar	0.43	0.58
	(0.46)	(0.43)
Share of white collar	0.47	0.31
	(0.45)	(0.41)
Share of apprentices	0.08	0.10
	(.23)	(.24)
Share of mid-managers	0.004	0.005
	(0.05)	(0.05)
Share of managers	0.006	0.002
	(0.06)	(0.03)
Mean worker age	37.1	37.8
	(8.7)	(9.0)
Log weekly wage	5.85	5.85
	(0.39)	(0.38)
Log assets	5.89	6.50
	(1.66)	(1.63)
Log productivity	3.33	3.51
	(0.92)	(0.87)
Leverage	0.81	0.81
	(0.33)	(0.32)
N. Observations	190,873	14,151,780

Notes: This table reports descriptive statistics between 2008 and 2016. Column (1) refers to the sample of firms using a pirate agreement for at least one worker, column (2) to firms not using pirate agreements. Standard deviations are reported in parentheses. Log weekly wages are expressed in log euros. Productivity is computed as value added per worker. Leverage is computed as 1-equity/assets (for pirate firms, it is computed in the year before the adoption of the pirate CBA).

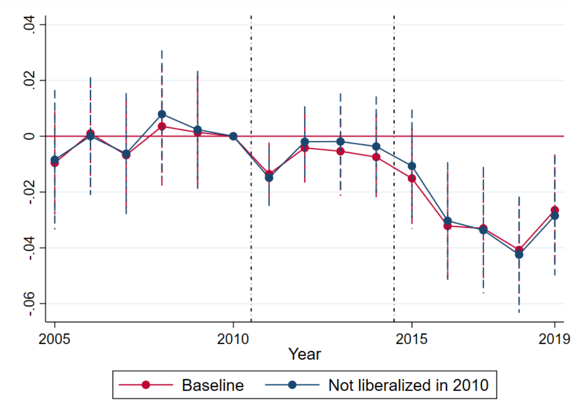
Appendix Table B3.3. Percent of Workers across Sectors by Adoption of Pirate Agreements

	(1) Pirate workers	(2) Other workers
Agriculture, Forestry and Fishing	2.11	0.63
Mining and Quarrying	0.01	0.35
Manufacturing	19.92	27.24
Electricity, Gas, Steam and Air Conditioning Supply	0.07	0.53
Water Supply, Sewerage, Waste Management	0.24	0.89
Construction	0.75	8.33
Wholesale and Retail Trade	22.29	15.16
Transportation and Storage	7.98	6.74
Accommodation and Food	1.89	9.59
Information and Communication	8.69	2.74
Finance and Insurance	0.54	3.94
Real Estate Activities	0.73	0.30
Professional, Scientific and Technical Activities	3.74	3.02
Administrative and Support Service Activities	12.11	9.95
Public Administration and Defence	0.67	0.57
Education	1.32	1.38
Human Health and Social Work	7.22	4.39
Arts, Entertainment and Recreation	1.64	0.58
Other Service Activities	7.63	3.34
Activities of Households as Employers	0.05	0.31
Activities of Extraterritorial Organisations	0.43	0.03

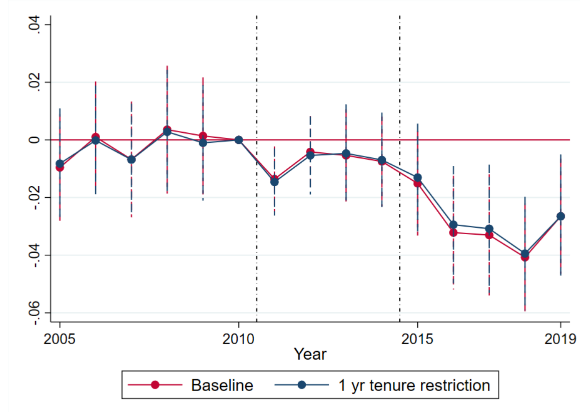
Notes: This table reports the percentage of workers in each sector between 2008 and 2016. Column (1) refers to the sample of workers covered by a pirate agreement, column (2) refers to the other workers.

Appendix Figure B3.2. The Effects of the 2011 Secession of Mass Retail Employers on Workers on Log Wage: Robustness Checks

(a) Robustness on liberalization of hours



(b) Robustness on tenure restriction



Notes: This figure displays the event-study coefficients from Equation (3.2) estimated on the matched sample defined in Section 3.5.2, using log weekly wages as outcome. In all charts, the red coefficients denote the baseline results. Panel (a): The blue event-study coefficients are obtained when excluding workers in municipalities where shop hours were not liberalized in 2010 (see text for details); Panel (b): The blue event-study coefficients are obtained when imposing a 1-year tenure restriction in our matching strategy, as opposed to 3-year restriction in the baseline analysis. Standard errors are clustered at the level of the 2010 employer.

3.C Appendix C

This Appendix describes our firm-level analysis of the pirate CBAs.

Econometric analysis. We employ a matched difference-in-differences strategy that compares treated firms – those that opt out – with a matched group of control firms that did not. We define the treatment year as the first year a firm uses a pirate CBA for at least one worker and, as in the worker-level design, focus on all firms for which the treatment year is included between 2008 and 2016, to have a sufficiently long number of years before and after the event, as our data span 2005 to 2019. We then run the following event-study regression:

$$y_{jt} = \alpha_j + \delta_t + \sum_{k=a}^{b} \gamma_k \cdot \mathbb{1}[t = t_j^* + k] + \sum_{k=a}^{b} \beta_k \cdot \mathbb{1}[t = t_j^* + k] \cdot T_j + v_{jt}, \qquad (3.3)$$

where y_{jt} is the outcome of interest for firm j in year t, α_j and δ_t are firm and year dummies and γ_k is a dummy denoting the number of periods relative to the event year, t_j^* (that is, the year of the opt-out).[24] T_j is a treatment indicator equal to one if firm j adopts the pirate agreement and zero otherwise. The coefficients of interest β_k capture the difference in y_{jt} between treated and control firms k years before/after the opt-out relative to the same difference in the year before the opt-out, which is normalized to zero. The main outcomes we focus on are the firm's average wage paid to its employees, its survival probability (a dummy taking value of one if the firm is observed in the data and zero otherwise), firm size and profits per worker.

Matching Strategy and Sample. We implement a matching algorithm that assigns each treated firm to a control one with similar characteristics prior to the opt-out. Potential control firms are all those that never applied a pirate contract.[25]

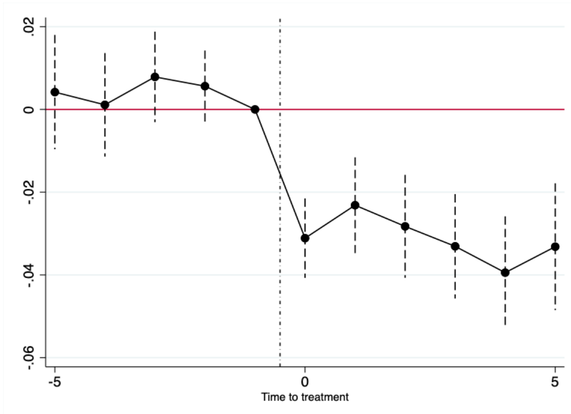
[24]For a control firm, this is the year of the adoption of the pirate agreement of the treated firm matched to this particular control firm. The next paragraph describes the matching algorithm that we implement.

[25]We drop firm in small sectors such as agriculture, public administration, activities of households as employer and extraterritorial organizations.

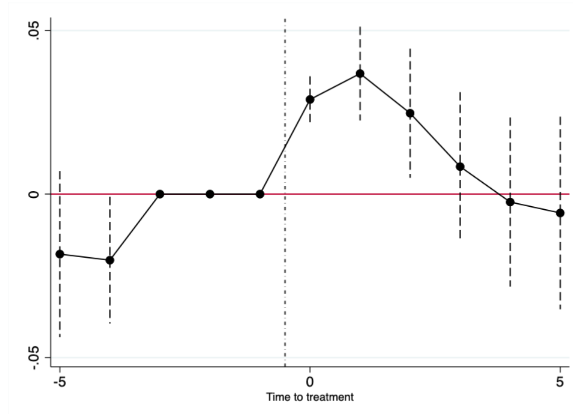
The propensity score model controls for the firm's average wage paid in the three years before the opt-out, firm size, sector, productivity, sales growth, profits-to-assets ratio and financial leverage. The matching procedure delivers a sample of 2,144 treated firms and an equal number of control firms. Table C3.1 reports descriptive statistics.

Appendix Figure C3.1. The Effects of Pirate Agreement Adoptions on Firms

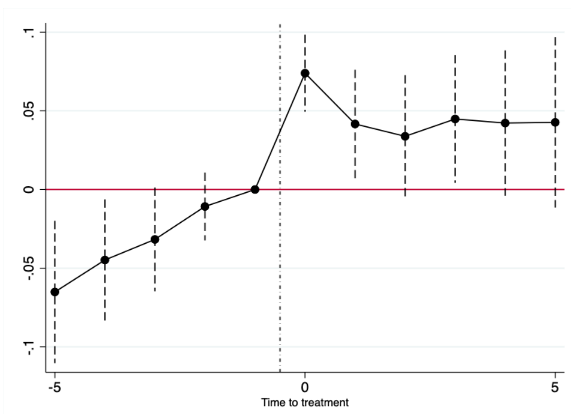
(a) Log Weekly Wages



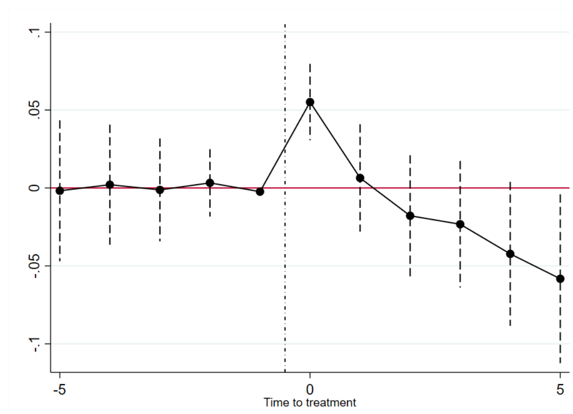
(b) Survival Probability



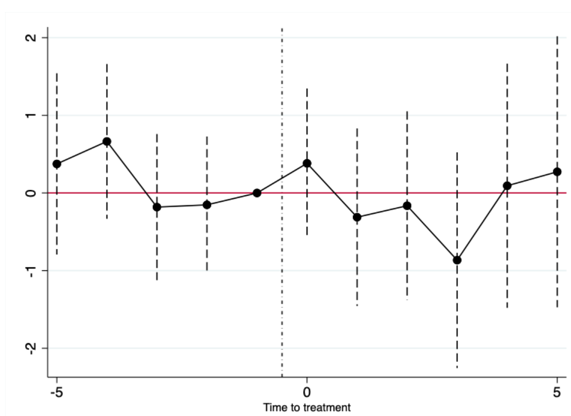
(c) Log Firm Size



(d) Log Firm Size (De-trended)



(e) Profits per Worker



Notes: This figure displays the event-study coefficients from Equation (3.3) estimated on the matched sample defined in Appendix 3.C. Panel (a): Log weekly wages is the mean log weekly wage paid by firm j to its workers, expressed in euros; Panel (b): the outcome is a year-to-year binary indicator taking value of 1 if firm j is observed in the data; Panel (c): the outcome is the logarithm of the total number of the firm's employees; Panel (d) reproduces the coefficients of Panel (c) after subtracting their linear time trend estimated in the pre-event periods and extrapolated for the later years as in Dustmann et al. (2022): Panel (e): the outcome is computed as the ratio between the firm's profits and total number of employees. Standard errors are clustered at the firm level.

Appendix Table C3.1. Pirate Agreement Adoptions: Descriptive Statistics of Firms

	Full sample		Matched sample	
	(1) Treated	(2) Controls	(3) Treated	(4) Controls
Firm size	28.18	8.13	118.61	29.92
	(370.35)	(143.45)	(865.14)	(156.48)
Firm age	10.39	13.07	14.19	14.60
	(11.6)	(12.2)	(11.54)	(11.32)
Share of full-time	0.53	0.60	0.66	0.68
	(0.42)	(0.43)	(0.34)	(0.34)
Share of temporary	0.19	0.15	0.18	0.16
	(0.32)	(0.29)	(0.24)	(0.24)
Share of women	0.55	0.47	0.49	0.48
	(0.40)	(0.42)	(0.33)	(0.36)
Share of blue collar	0.47	0.58	0.49	0.49
	(0.45)	(0.43)	(0.40)	(0.39)
Share of white collar	0.44	0.31	0.44	0.42
	(0.45)	(0.41)	(0.38)	(0.38)
Share of apprentice	0.07	0.10	0.06	0.05
	(0.22)	(0.25)	(0.13)	(0.14)
Share of mid-managers	0.006	0.005	0.01	0.01
	(0.05)	(0.05)	(0.05)	(0.03)
Share of managers	.006	.002	.005	.004
	(0.06)	(0.03)	(0.03)	(0.03)
Mean worker age	37.4	37.8	38.6	38.5
	(8.3)	(9.0)	(5.6)	(5.8)
Log weekly wage	5.84	5.85	5.92	5.92
	(0.40)	(0.38)	(0.32)	(0.32)
Log assets	6.21	6.50	6.94	6.64
	(1.85)	(1.62)	(1.78)	(1.52)
Log productivity	3.32	3.52	3.37	3.36
	(1.01)	(.87)	(.82)	(.82)
Leverage	0.83	0.81	0.80	0.81
	(0.30)	(0.33)	(0.27)	(0.28)
N. Observations	14,128	13,930,620	2,144	2,144

Notes: This table reports descriptive statistics averaged between 2008 and 2016 by group. Columns (1) and (2) refer to the firm sample before the matching. In particular, Column (1) reports $t^* - 1$ descriptives for firms that will transition to a pirate CBA at t^*; Column (2) reports descriptives for potential control firms, that is, firms that have never used a pirate CBA. Columns (3) and (4) show descriptives for the matched sample, obtained as described in Appendix 3.C. Standard deviations are reported in parentheses. Log weekly wages are expressed in log euros. Productivity is computed as value added per worker. Leverage is computed as $1 - equity/assets$ (for pirate firms, it is computed in the year before the adoption of the pirate CBA).

Appendix Table C3.2. The Effects of Pirate Agreement Adoptions on Firms

	(1) Log Weekly Wages	(2) Survival Probability
On Impact	-0.031	0.029
	(0.005)***	(0.004)***
Medium Run	-0.028	0.025
	(0.006)***	(0.010)**
Long Run	-0.033	-0.006
	(0.009)***	(0.015)
N	39,938	44,216
Mean outcome	5.92	0.94
Average of Pre-Event Coeffs	0.005	-0.010
	(0.005)	(0.005)*
p-value Pre-Event Coeffs = 0	0.326	0.067

Notes: This table reports three event-study coefficients from equation (3.3) estimated on the matched sample defined in Appendix 3.C. 'On Impact' denotes the coefficient estimated in the opt-out year t^*, 'Medium Run' corresponds to the coefficient two years after the opt-out, and 'Long Run' refers to the coefficient five years after the opt-out. 'Mean Outcome' represents the mean of the outcome of interest in the five years before the opt-out, computed considering the control group only. The last rows show the mean event-study coefficient in the five years before the opt-out ('Average of Pre-Event Coeffs'), the standard error in parentheses, and the p-value of the test that the mean pre-event coefficient is equal to zero. Log weekly wages is the mean log weekly wage paid by firm j to its workers, expressed in euros. Survival probability is a year-to-year binary indicator taking value of 1 if firm j is observed in the data. Standard errors are clustered at the firm level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Bibliography

Abadie, Alberto and Guido Imbens, "Matching on the Estimated Propensity Score," *Econometrica*, 2016, *84*, 781–807. 201

Abowd, John, Francis Kramarz, and David Margolis, "High Wage Workers and High Wage Firms," *Econometrica*, 1999, *67* (2), 251–334. 51, 67, 111

Adamopoulou, Effrosyni and Ernesto Villanueva, "Wage determination and the bite of collective contracts in Italy and Spain," *Labour Economics*, 2022, *76* (C), S0927537122000409. 219

Albanese, Giuseppe, Guido de Blasio, and Lorenzo Incoronato, "Government Transfers and Votes for States Intervention," *Working Paper*, 2023. 23, 26, 56, 85

Alesina, Alberto and George-Marios Angeletos, "Fairness and Redistribution," *American Economic Review*, 2005, *95* (4), 960–980. 179, 209

_ **and Nicola Fuchs-Schündeln**, "Good-bye Lenin (or Not?): The Effect of Communism on People's Preferences," *American Economic Review*, September 2007, *97* (4), 1507–1528. 145, 178

_ **and Paola Giuliano**, "Chapter 4 - Preferences for Redistribution," in Jess Benhabib, Alberto Bisin, and Matthew O. Jackson, eds., *Handbook of Social Economics*, Vol. 1, North-Holland, 2011, pp. 93–131. 145, 209

Alessandrini, Michele, Pietro Celotti, Erich Dallhammer, Helene Gorny, Andrea Gramillano, Bernd Schuh, Chiara Zingaretti, Maria Toptsidou, and Mailin Gaupp-Berghausen, *Implementing a place-based approach to EU industrial policy strategy*, European Committee of the Regions, 2019. 18

Alfani, Guido and Sergio Sardone, "Long-term trends in economic inequality in southern Italy. The Kingdoms of Naples and Sicily, 16th–18th centuries: First results," *In: Paper given at the economic history association conference, Nashville, 11–13 Sept 2015.*, 2015. 158

Allen, Treb and Costas Arkolakis, "Economic Activity across Space: A Supply and Demand Approach," *Journal of Economic Perspectives*, 2023, *37* (2), 3–28. 22

Angrist, Joshua and Miikka Rokkanen, "Wanna Get Away? Regression Discontinuity Estimation of Exam School Effects Away From the Cutoff," *Journal of the American Statistical Association*, 2015, *110* (512), 1331–1344. 58, 137, 139

Atalay, Enghin, Ali Hortaçsu, Mustafa Runyun, Chad Syverson, and Mehmet Fatih Ulu, "Micro- and Macroeconomic Impacts of a Place-Based Industrial Policy," *Working Paper*, 2022. 25

Austin, Benjamin A, Edward L Glaeser, and Lawrence H Summers, "Jobs for the Heartland: Place-Based Policies in 21st Century America," Working Paper 24548, National Bureau of Economic Research April 2018. 146

Autor, David and David Dorn, "The Growth of Low-Skill Service Jobs and the Polarization of the US Labor Market," *American Economic Review*, 2013, *103* (5), 1553–97. 25

Bartik, Timothy, "Using Place-Based Jobs Policies to Help Distressed Communities," *Journal of Economic Perspectives*, 2020, *34* (3), 99–127. 25, 88

_ , Brian Asquith, and Kathleen Bolter, "The Chips and Science Act Offers Funding for Place-Based Policies Unparalleled in U.S. History," 2022. 18

Becker, Sascha O., Stephan Heblich, and Daniel M. Sturm, "The impact of public employment: Evidence from Bonn," *Journal of Urban Economics*, 2021, *122*, 103291. 25

Becker, Sascha, Peter Egger, and Maximilian von Ehrlich, "Going NUTS: The effect of EU Structural Funds on regional performance," *Journal of Public Economics*, 2010, *94* (9-10), 578–590. 146, 147

_ , Thiemo Fetzer, and Dennis Novy, "Who voted for Brexit? A comprehensive district-level analysis," *Economic Policy*, 2017, *32* (92), 601–650. 146

Benabou, Roland, "Ideology," *Journal of the European Economic Association*, 05 2008, *6* (2-3), 321–352. 179, 209

_ and Efe Ok, "Social Mobility and the Demand for Redistribution: The Poum Hypothesis," *The Quarterly Journal of Economics*, 2001, *116* (2), 447–487. 144

Benoit, Kenneth, Michael Laver, and Slava Mikhaylov, "Treating Words as Data with Error: Uncertainty in Text Statements of Policy Positions," *American Journal of Political Science*, 2009, *53* (2), 495–513. 151

Bertanha, Marinho and Guido Imbens, "External Validity in Fuzzy Regression Discontinuity Designs," *Journal of Business & Economic Statistics*, 2020, *38* (3), 593–612. 58, 137, 138

Bertheau, Antoine, Edoardo Maria Acabbi, Cristina Barceló, Andreas Gulyas, Stefano Lombardi, and Raffaele Saggio, "The Unequal Consequences of Job Loss Across Countries," *American Economic Review: Insights*, 2023, *5* (3), 393–408. 230

Bianchi, Nicola and Michela Giorcelli, "Reconstruction Aid, Public Infrastructure, and Economic Development: The Case of the Marshall Plan in Italy," Working Paper 29537, National Bureau of Economic Research December 2021. 146

_ and _ , "Reconstruction Aid, Public Infrastructure, and Economic Development: The Case of the Marshall Plan in Italy," *The Journal of Economic History*, 2023, *83* (2), 501–537. 38

Boeri, Tito, *Institutional Reforms and Dualism in European Labor Markets*, Vol. 122, Amsterdam and New York: Elsevier, 2011. 245

_ , "Two-Tier Bargaining," IZA Discussion Papers 8358, Institute of Labor Economics (IZA) 2014. 221

_ and Pietro Garibaldi, "A Tale of Comprehensive Labor Market Reforms: Evidence from the Italian Jobs Act," *Labour Economics*, 2019, *59* (C), 33–48. 217, 245

_ , Andrea Ichino, Enrico Moretti, and Johanna Posch, "Wage Equalization and Regional Misallocation: Evidence from Italian and German Provinces," *Journal of the European Economic Association*, 2021, *19* (6), 3249–3292. 216, 217, 219, 224, 235, 247, 250, 251

Borgomeo, Letizia, "Determinants and outcomes of industrial policies: evidence from Italy," *PhD thesis*, *University of Warwick*, 2018. 148

Bronzini, Raffaello and Guido de Blasio, "Evaluating the impact of investment incentives: The case of Italy's Law 488/1992," *Journal of urban economics*, 2006, *60* (2), 327–349. 28

Buscemi, Tancredi and Giulia Romani, "The Political Economy of Regional Development: Evidence from the Cassa per il Mezzogiorno," Working Papers 2022:18, Department of Economics, University of Venice "Ca' Foscari" 2022. 26, 146, 148

Busso, Matias, Jesse Gregory, and Patrick Kline, "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, 2013, *103* (2), 897–947. 23, 54, 117

_ , _ , and _ , "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, 2013, *103* (2), 897–947. 146

Calmfors, Lars and John Driffill, "Bargaining Structure, Corporatism and Macroeconomic Performance," *Economic Policy*, 1988, *3* (6), 14–61. 217

Calonico, Sebastian, Matias Cattaneo, and Rocio Titiunik, "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 2014, *82*, 2295–2326. 43, 86, 96

_ , _ , and _ , "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 2014, *82*, 2295–2326. 155, 157, 160, 161, 163

_ , _ , and _ , "Optimal Data-Driven Regression Discontinuity Plots," *Journal of the American Statistical Association*, 2015, *110* (512), 1753–1769. 155, 157

Caprettini, Bruno, Lorenzo Casaburi, and Miriam Venturini, "Redistribution, Voting and Clientelism: Evidence from the Italian Land Reform," CEPR Discussion Papers 15679, C.E.P.R. Discussion Papers 2021. 145

Card, David and Alan Krueger, "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," *American Economic Review*, 1994, *84* (4), 772–93. 235, 246

_ and Ana Rute Cardoso, "Wage Flexibility under Sectoral Bargaining," *Journal of the European Economic Association*, 2022, *20* (5), 2013–2061. 240

_ , Jörg Heining, and Patrick Kline, "Workplace Heterogeneity and the Rise of West German Wage Inequality," *The Quarterly journal of economics*, 2013, *128* (3), 967–1015. 219, 235

Carillo, Mario F., "Fascistville: Mussolini's new towns and the persistence of neofascism," *Journal of Economic Growth*, 2022, *27* (4), 527–567. 145

Cattaneo, Matias D. and Rocío Titiunik, "Regression Discontinuity Designs," *Annual Review of Economics*, 2022, *14* (1), 821–851. 36

Cattaneo, Matias, Rocio Titiunik, and Gonzalo Vazquez-Bare, "Inference in regression discontinuity designs under local randomization," *Stata Journal*, 2016, *16* (2), 331–367. 43, 94

Centamore, Giulio, "Contratti Collettivi o Diritto del Lavoro «Pirata»?," *Variazioni su Temi di Diritto del Lavoro*, 2018, *2*, 471. 222

Cerqua, Augusto and Guido Pellegrini, "Do subsidies to private capital boost firms' growth? A multiple regression discontinuity design approach," *Journal of Public Economics*, 2014, *109* (C), 114–126. 28, 54

Cerrato, Andrea, "How Big Is the Big Push? The Macroeconomic Effects of a Large-Scale Regional Development Program," *Working Paper*, 2024. 26

Charles, Kerwin Kofi, Erik Hurst, and Mariel Schwartz, "The Transformation of Manufacturing and the Decline in US Employment," *NBER Macroeconomics Annual*, 2019, *33* (1), 307 – 372. 25

Chaurey, Ritam, "Location-based tax incentives: Evidence from India," *Journal of Public Economics*, 2017, *156* (C), 101–120. 54, 117

Choi, Jaedo and Andrei Levchenko, "The Long-Term Effects of Industrial Policy," CEPR Discussion Papers 16534, C.E.P.R. Discussion Papers 2021. 24

Cingano, Federico, Filippo Palomba, Paolo Pinotti, and Enrico Rettore, "Making Subsidies Work: Rules vs. Discretion," CEPR Discussion Papers 17004, C.E.P.R. Discussion Papers 2022. 24, 28, 54

Colella, Fabrizio, Rafael Lalive, Seyhun Orcan Sakalli, and Mathias Thoenig, "Inference with Arbitrary Clustering," IZA Discussion Papers 12584, Institute of Labor Economics (IZA) 2019. 166

Colussi, Tommaso, Giampaolo Lecce, Marco Manacorda, and Massimiliano Gaetano Onorato, "The Politics and Economics of Government Aid: Evidence from the Italian Cassa per il Mezzogiorno," *Working Paper*, 2020. 26, 145, 146, 158, 196

Conley, Timothy, "GMM estimation with cross sectional dependence," *Journal of Econometrics*, 1999, *92* (1), 1–45. 42, 86, 93, 128, 130, 131, 132, 133, 134, 135, 164, 166, 194

Corneo, Giacomo and Hans Peter Gruner, "Individual preferences for political redistribution," *Journal of Public Economics*, 2002, *83* (1), 83–107. 209

Criscuolo, Chiara, Ralf Martin, Henry Overman, and John van Reenen, "Some Causal Effects of an Industrial Policy," *American Economic Review*, 2019, *109* (1), 48–85. 23, 24, 54

d'Adda, Giovanna and Guido de Blasio, "Historical Legacy and Policy Effectiveness: The Long-Term Influence of Preunification borders in Italy," *Journal of Regional Science*, 2017, *57* (2), 319–341. 158

Dahl, Christian, Daniel le Maire, and Jakob Munch, "Wage Dispersion and Decentralization of Wage Bargaining," *Journal of Labor Economics*, 2013, *31* (3), 501 – 533. 219, 220

D'Amuri, Francesco and Raffaella Nizzi, "Recent Developments of Italy's Industrial Relations System," Questioni di Economia e Finanza (Occasional Papers) 416, Bank of Italy, Economic Research and International Relations Area 2017. 221

Daruich, Diego, Sabrina Di Addario, and Raffaele Saggio, "The Effects of Partial Employment Protection Reforms: Evidence from Italy," *Review of Economic Studies*, 2023, *90* (6), 2880–2942. 227, 245

Dell, Melissa, "The Persistent Effects of Peru's Mining Mita," *Econometrica*, 2010, *78* (6), 1863–1903. 85

Dell'Aringa, Carlo, "Dai Minimi Tabellari ai Salari di Garanzia," in Carlo Dell'Aringa, Claudio Lucifora, and Tiziano Treu, eds., *Produttività, Diseguaglianze*, Il Mulino, 2017. 221

Durante, Ruben, Paolo Pinotti, and Andrea Tesei, "The Political Legacy of Entertainment TV," *American Economic Review*, July 2019, *109* (7), 2497–2530. 167

Duranton, Gilles and Anthony Venables, "Place-Based Policies for Development," NBER Working Papers 24562, National Bureau of Economic Research, Inc 2018. 24

_ **and Diego Puga**, "Micro-foundations of urban agglomeration economies," in J. V. Henderson and J. F. Thisse, eds., *Handbook of Regional and Urban Economics*, 1 ed., Vol. 4, Elsevier, 2004, chapter 48, pp. 2063–2117. 20

Dustmann, Christian, Attila Lindner, Uta Schönberg, Matthias Umkehrer, and Philipp Vom Berge, "Reallocation effects of the minimum wage," *The Quarterly Journal of Economics*, 2022, *137* (1), 267–328. 244, 261

_ **, Bernd Fitzenberger, Uta Schönberg, and Alexandra Spitz-Oener**, "From Sick Man of Europe to Economic Superstar: Germany's Resurgent Economy," *Journal of Economic Perspectives*, 2014, *28* (1), 167–88. 219, 220, 247

_ **, Johannes Ludsteck, and Uta Schönberg**, "Revisiting the German Wage Structure," *The Quarterly journal of economics*, 2009, *124* (2), 843–881. 235

Faggio, Giulia and Henry Overman, "The effect of public sector employment on local labour markets," *Journal of Urban Economics*, 2014, *79* (C), 91–107. 25

Fai, Felicia, "Place-Based Perspectives on the UK Industrial Strategy," 2018. 18

Faia, Ester and Vincenzo Pezone, "The Cost of Wage Rigidity," *Review of Economic Studies*, 2023, p. rdad020. 221, 227

Falck, Oliver, Stephan Heblich, and Stefan Kipar, "Industrial innovation: Direct evidence from a cluster-oriented policy," *Regional Science and Urban Economics*, 2010, *40* (6), 574–582. 24

Fanfani, Bernardo, "The Employment Effects of Collective Wage Bargaining," *Journal of Public Economics*, 2023, *227*, 105006. 221, 240

Felice, Emanuele, "The Roots of a Dual Equilibrium: GDP, Productivity and Structural Change in the Italian Regions in the Long-run (1871-2011)," Quaderni di storia economica (Economic History Working Papers) 40, Bank of Italy, Economic Research and International Relations Area 2017. 26

_ **and Amedeo Lepore**, "State intervention and economic growth in Southern Italy: the rise and fall of the 'Cassa per il Mezzogiorno' (1950-1986)," *Business history*, 2017, *59* (3), 319–341. 26, 141, 146, 147, 148

Finan, Frederico and Laura Schechter, "Vote-Buying and Reciprocity," *Econometrica*, 2012, *80* (2), 863–881. 144, 176

Fontana, Nicola, Tommaso Nannicini, and Guido Tabellini, "Historical Roots of Political Extremism: The Effects of Nazi Occupation of Italy," CEPR Discussion Papers 11758, C.E.P.R. Discussion Papers 2017. 158

Frandsen, Brigham R., Markus Frölich, and Blaise Melly, "Quantile treatment effects in the regression discontinuity design," *Journal of Econometrics*, 2012, *168* (2), 382–395. 53, 114

Freedman, Matthew, "Teaching new markets old tricks: The effects of subsidized investment on low-income neighborhoods," *Journal of Public Economics*, 2012, *96* (11), 1000–1014. 54

Gagliardi, Luisa, Enrico Moretti, and Michel Serafinelli, "The World's Rust Belts: The Heterogeneous Effects of Deindustrialization on 1,993 Cities in Six Countries," *Working Paper*, 2023. 25, 56, 178

Gagliarducci, Stefano, Massimiliano Onorato, Francesco Sobbrio, and Guido Tabellini, "War of the Waves: Radio and Resistance during World War II," *American Economic Journal: Applied Economics*, 2020, *12* (4), 1–38. 38, 68

Garin, Andrew and Jonathan Rothbaum, "The Long-Run Impacts of Public Industrial Investment on Regional Development and Economic Mobility: Evidence from World War II," *Working Paper*, 2022. 21, 24, 53

Gelman, Andrew and Guido Imbens, "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs," *Journal of Business and Economic Statistics*, 2019, *37* (3), 447–456. 160

Giorcelli, Michela and Bo Li, "Technology Transfer and Early Industrial Development: Evidence from the Sino-Soviet Alliance," CEPR Discussion Papers 16980, C.E.P.R. Discussion Papers 2022. 24, 25

Glaeser, Edward L. and Joshua D. Gottlieb, "The Economics of Place-Making Policies," *Brookings Papers on Economic Activity*, 2008, *39* (1 (Spring), 155–253. 24

Goldschmidt, Deborah and Johannes F Schmieder, "The Rise of Domestic Outsourcing and the Evolution of the German Wage Structure," *The Quarterly Journal of Economics*, 2017, *132* (3), 1165–1217. 218, 230, 234

Greenstone, Michael, Richard Hornbeck, and Enrico Moretti, "Identifying Agglomeration Spillovers: Evidence from Winners and Losers of Large Plant Openings," *Journal of Political Economy*, 2010, *118* (3), 536–598. 25

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano, "Do Fiscal Rules Matter?," *American Economic Journal: Applied Economics*, 2016, *8* (3), 1–30. 21, 38, 75

Gürtzgen, Nicole, "Estimating the Wage Premium of Collective Wage Contracts: Evidence from Longitudinal Linked Employer–Employee Data," *Industrial Relations: A Journal of Economy and Society*, 2016, *55* (2), 294–322. 219, 220

Hahn, Jinyong, Petra Todd, and Wilbert van der Klaauw, "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design," *Econometrica*, 2001, *69* (1), 201–09. 71

Hanlon, Walker, "The Persistent Effect of Temporary Input Cost Advantages in Shipbuilding, 1850 to 1911," *Journal of the European Economic Association*, 2020, *18* (6), 3173–3209. 22, 24

— **and Stephan Heblich**, "History and Urban Economics," NBER Working Papers 27850, National Bureau of Economic Research, Inc 2020. 24

Heblich, Stephan, Marlon Seror, Hao Xu, and Yanos Zylberberg, "Industrial Clusters in the Long Run: Evidence from Million-Rouble Plants in China," Working Paper 30744, National Bureau of Economic Research December 2022. 19, 24, 25

Helm, Ines, Alice Kuegler, and Uta Schoenberg, "Displacement Effects in Manufacturing and Structural Change," CReAM Discussion Paper Series 2313, Centre for Research and Analysis of Migration (CReAM), Department of Economics, University College London 2023. 25

Imbens, Guido and Thomas Lemieux, "Regression discontinuity designs: A guide to practice," *Journal of Econometrics*, 2008, *142* (2), 615–635. 71, 86, 156

— **and Tristan Zajonc**, "Regression Discontinuity Design with Multiple Forcing Variables," *Report, Harvard University*, 2011, (972). 155

Inglehart, R., C. Haerpfer, A. Moreno, C. Welzel, K. Kizilova, J. Diez-Medrano, M. Lagos, P. Norris, E. Ponarin, B. Puranen, and et al. (eds), "World Values Survey: Round Five - Country-Pooled Datafile," *JD Systems Institute and WVSA Secretariat*, 2018, *Madrid, Spain and Vienna, Austria.* 209

Iuzzolino, Giovanni, Guido Pellegrini, and Gianfranco Viesti, "Convergence among Italian Regions, 1861-2011," Quaderni di storia economica (Economic History Working Papers) 22, Bank of Italy, Economic Research and International Relations Area 2011. 26

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan, "Earnings Losses of Displaced Workers," *The American economic review*, 1993, pp. 685–709. 234

Jäger, Simon and Jörg Heining, "How Substitutable are Workers? Evidence from Worker Deaths," Technical Report, National Bureau of Economic Research 2022. 230

— **, Shakked Noy, and Benjamin Schoefer**, "The German Model of Industrial Relations: Balancing Flexibility and Collective Action," *Journal of Economic Perspectives*, 2022, *36* (4), 53–80. 220

Jimeno, Juan and Carlos Thomas, "Collective Bargaining, Firm Heterogeneity and Unemployment," *European Economic Review*, 2013, *59* (C), 63–79. 217

Jones, Erik and Gianfranco Pasquino, *The Oxford Handbook of Italian Politics*, Oxford University Press, 11 2015. 167

Juhász, Réka, "Temporary Protection and Technology Adoption: Evidence from the Napoleonic Blockade," *American Economic Review*, 2018, *108* (11), 3339–76. 24

— **, Nathaniel Lane, and Dani Rodrik**, "The New Economics of Industrial Policy," *Working Paper*, 2023. 19, 24

Kantor, Shawn and Alexander Whalley, "Moonshot: Public R&D and Growth," *Working Paper*, 2022. 24

Keele, Luke J. and Rocio Titiunik, "Geographic Boundaries as Regression Discontinuities," *Political Analysis*, 2015, *23* (1), 127–155. 159

Kim, Minho, Munseob Lee, and Yongseok Shin, "The Plant-Level View of an Industrial Policy: The Korean Heavy Industry Drive of 1973," Working Paper 29252, National Bureau of Economic Research September 2021. 25

Kline, Patrick, "Place Based Policies, Heterogeneity, and Agglomeration," *American Economic Review*, May 2010, *100* (2), 383–87. 58, 88, 90

— **and Enrico Moretti**, " Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority," *The Quarterly Journal of Economics*, 2 2014, *129* (1), 275–331. 24, 146, 171

— **and** — , "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs," *Annual Review of Economics*, 2014, *6* (1), 629–662. 18, 24, 40, 88, 90

Krugman, Paul, *Geography and Trade*, 1 ed., Vol. 1, The MIT Press, 1992. 53, 64

Kugler, Adriana and Giovanni Pica, "Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform," *Labour Economics*, 2008, *15* (1), 78–95. 232, 245

Lane, Nathaniel, "Manufacturing Revolutions: Industrial Policy and Industrialization in South Korea," EconStor Preprints, ZBW - Leibniz Information Centre for Economics 2022. 24, 25

Lang, Valentin, Nils Redeker, and Daniel Bischof, "Place-Based Policies and Inequality Within Regions," Aug 2022. 53

Lapoint, Cameron and Shogo Sakabe, "Place-Based Policies and the Geography of Corporate Investment," *Working Paper*, 2022. 24, 54, 117, 119

Leonardi, Marco and Enrico Moretti, "The Agglomeration of Urban Amenities: Evidence from Milan Restaurants," CEPR Discussion Papers 16937, C.E.P.R. Discussion Papers 2022. 49

Leuven, Edwin and Barbara Sianesi, "PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing," 2018. 201

Lu, Yi, Jin Wang, and Lianming Zhu, "Place-Based Policies, Creation, and Agglomeration Economies: Evidence from China's Economic Zone Program," *American Economic Journal: Economic Policy*, August 2019, *11* (3), 325–60. 24, 54, 117, 119

Lucifora, Claudio and Daria Vigani, "Losing Control? Unions' Representativeness, Pirate Collective Agreements, and Wages," *Industrial Relations: A Journal of Economy and Society*, 2021, *60* (2), 188–218. 219, 228

Manacorda, Marco, Edward Miguel, and Andrea Vigorito, "Government Transfers and Political Support," *American Economic Journal: Applied Economics*, 2011, *3* (3), 1–28. 140, 144, 176

McCrary, Justin, "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics*, 2008, *142* (2), 698–714. 70, 156, 184

Meltzer, Allan and Scott F Richard, "A Rational Theory of the Size of Government," *Journal of Political Economy*, 1981, *89* (5), 914–27. 144

Millán-Quijano, Jaime, "Fuzzy difference in discontinuities," *Applied Economics Letters*, 2020, *27* (19), 1552–1555. 39, 75, 77

Mitrunen, Matti, "Industrial Policy, Structural Change and Intergenerational Mobility: Evidence from the Finnish War Reparations," *Working Paper*, 2020. 24

Moretti, Enrico, "Local Multipliers," *American Economic Review*, May 2010, *100* (2), 373–77. 21, 25, 47, 48, 50

—, "Local Labor Markets," in O. Ashenfelter and D. Card, eds., *Handbook of Labor Economics*, 1 ed., Vol. 4B, Elsevier, 2011, chapter 14, pp. 1237–1313. 20

_ , *The new geography of jobs*, Houghton Mifflin Harcourt Boston, Mass, 2012. 25

_ **and Per Thulin**, "Local Multipliers and Human Capital in the US and Sweden," Working Paper Series 914, Research Institute of Industrial Economics 2012. 48

Navaretti, Giorgio Barba and Borislav Markovic, "Place-based policies and the foundations of productivity in the private sector," *OECD-EC high-level expert workshop Productivity Policy for Places*, 2021. 25

Neumark, David and Helen Simpson, "Chapter 18 - Place-Based Policies," in Gilles Duranton, J. Vernon Henderson, and William C. Strange, eds., *Handbook of Regional and Urban Economics*, Vol. 5 of *Handbook of Regional and Urban Economics*, Elsevier, 2015, pp. 1197–1287. 24, 142

O, Ana L. De La, "Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico," *American Journal of Political Science*, 2013, *57* (1), 1–14. 140, 145

OECD, "Negotiating Our Way Up," 2019, p. 270. 216, 219

Palomba, Filippo, "getaway: Getting Away from the Cutoff in Regression Discontinuity Designs," *Working Paper*, 2023. 137, 139

Philippis, Marta De, Andrea Locatelli, Giulio Papini, and Roberto Torrini, "Italian economic growth and the North-South gap: Historical trends and future projections in light of the recent demographic scenarios," *Bank of Italy Occasional Paper*, 2022, (683). 26, 247

Pop-Eleches, Cristian and Grigore Pop-Eleches, "Targeted Government Spending and Political Preferences," *Quarterly Journal of Political Science*, 2012, *7* (3), 285–320. 140

Porter, Michael E., "Location, Competition, and Economic Development: Local Clusters in a Global Economy," *Economic Development Quarterly*, 2000, *14* (1), 15–34. 18

Rajan, Raghuram, "The Third Pillar: How Markets and the State Leave the Community Behind," *Penguin Publishing Group*, 2019. 146

Rizzica, Lucia, Giacomo Roma, and Gabriele Rovigatti, "The Effects of Deregulating Retail Operating Hours: Empirical Evidence from Italy," *The Journal of Law and Economics*, 2023, *66* (1), 21–52. 241

Rodrik, Dani, "Where Are We in the Economics of Industrial Policies?," *Frontiers of Economics in China*, 2019, *14* (3), 329–335. 19

_ , "An Industrial Policy for Good Jobs," 2022. September 2022. 59

_ **and Stefanie Stantcheva**, "Fixing capitalism's good jobs problem," *Oxford Review of Economic Policy*, 2021, *37* (4), 824–837. 22, 25, 59

Rodríguez-Pose, Andres, "The revenge of the places that don't matter (and what to do about it)," *Cambridge Journal of Regions, Economy and Society*, 2018, *11* (1), 189–209. 146

Saez, Emmanuel, Benjamin Schoefer, and David Seim, "Payroll Taxes, Firm Behavior, an Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden," *American Economic Review*, 2019, *109* (5), 1717–1763. 235

Schmieder, Johannes F, Till Von Wachter, and Jörg Heining, "The Costs of Job Displacement over the Business Cycle and its Sources: Evidence from Germany," *American Economic Review*, 2023, *113* (5), 1208–1254. 230

Schumann, Abel, "Persistence of Population Shocks: Evidence from the Occupation of West Germany after World War II," *American Economic Journal: Applied Economics*, July 2014, *6* (3), 189–205. 172

Siegloch, Sebastian, Nils Wehrhöfer, and Tobias Etzel, "Spillover, Efficiency and Equity Effects of Regional Firm Subsidies," ECONtribute Discussion Papers Series 210, University of Bonn and University of Cologne, Germany 2022. 23, 24, 25, 54

Slattery, Cailin, "The Political Economy of Subsidy-Giving," *Working Paper*, 2022. 145

Visser, Jelle, "Wage Bargaining Institutions – from Crisis to Crisis," European Economy - Economic Papers 2008 - 2015 488, Directorate General Economic and Financial Affairs (DG ECFIN), European Commission 2013. 216

Volkens, Andrea, Judith Bara, Ian Budge, Michael D McDonald, and Hans-Dieter Klingemann, *Mapping Policy Preferences From Texts: Statistical Solutions for Manifesto Analysts*, Oxford: Oxford University Press, 2013. 151

_ , Werner Krause, Pola Lehmann, Theres Matthieß, Nicolas Merz, Sven Regel, and Bernhard Weßels, "The Manifesto Data Collection. Manifesto Project (MRG/CMP/MARPOR). Version 2019b," 2019. 141, 150

von Ehrlich, Maximilian and Henry G. Overman, "Place-Based Policies and Spatial Disparities across European Cities," *Journal of Economic Perspectives*, August 2020, *34* (3), 128–49. 24

_ and Tobias Seidel, "The Persistent Effects of Place-Based Policy: Evidence from the West-German Zonenrandgebiet," *American Economic Journal: Economic Policy*, 2018, *10* (4), 344–74. 23, 41, 53, 147, 158, 160, 178