The Spatial Organization of Firms in Switzerland

Three Essays on Infrastructure, Place-Based Policy, and Corporate Networks

DOCTORAL THESIS

Submitted to the Faculty of Management, Economics and Social Sciences
at the University of Fribourg (Switzerland)

by
Yannick Schmutz
Courtepin, FR

in fulfillment of the requirements for the degree of
Doctor of Philosophy in Economics (PhD)

Approved by the Faculty of Management, Economics and Social Sciences
on June 8, 2026, at the proposal of
Prof. Dr. Mark Schelker - First supervisor
Prof. Dr. Maximilian von Ehrlich - Second supervisor
Prof. Dr. Thierry Madiès - Chairman of the committee

Fribourg, 2026

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It has been a journey. From the lakeside of Gerzensee and its Study Center, to Melbourne's skyscrapers. From my hometown of Fribourg to Barcelona, Bordeaux, Marseille, Oxford, Prague, or Zürich. From a high-school graduate full of doubts to a doctoral student still full of doubts. From grief, to the most beautiful day of my life when I met my wonderful daughter three years ago. All these experiences were possible thanks to the support I received from many people, and I want to acknowledge them. While reading this thesis, keep in mind that I did not achieve this alone. In fact, I would not have been able to do it on my own. The silence between successes can feel endless during a PhD. Each person I mention was essential in helping me through these periods. In many ways, they wrote this thesis as much as I did.

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Introduction

This thesis studies how infrastructure, public policy, distance, and borders shape the spatial organization of firms in Switzerland. It focuses on the 1940s to early 2000s, a period during which a small, highly open economy with strong internal borders (linguistic, religious, and cantonal) gradually built a unified national market through large-scale transport investments and regional policies. The three essays combine new historical data on all Swiss corporations with information on transport infrastructure, place-based interventions, and firm connections via shared board members to shed light on where economic activity takes place, how it is reorganized over time, and how it is coordinated through corporate networks.

Switzerland offers a rich setting for this purpose. The federal structure and strong cantonal autonomy have historically produced sharp internal borders in regulation, taxation, and political institutions. These borders overlap with long-standing linguistic and religious cleavages that segment social and economic interactions. At the same time, the second half of the twentieth century saw efforts to integrate the domestic market: the construction of a national highway network from the 1960s onward, the introduction of large regional development programs in response to structural crises in the 1970s, and the weakening of internal borders in the late part of the past century. Following those changes, firms, workers, and capital reallocated across space, while corporate elites formed dense networks through shared board memberships. Understanding how these forces jointly shape the geography and structure of firms is central for both economic geography and the design of regional and industrial policies.

All three chapters exploit a newly constructed historical database that digitizes the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften / Répertoire des administrateurs des sociétés anonymes suisses*", a registry listing directors and all their mandates in Swiss-based corporations between 1934 and 2003. From these publications, we reconstruct the universe of Swiss corporations and collect information on their headquarters locations, nominal capital, and board composition. We match firms across years to construct a long municipal-level panel of stocks, births, deaths, and relocations. We merge our data with external information on transport infrastructure, regional policy coverage, population and employment, and various geographic and urban-structural measures. The resulting datasets allow us to study both firm dynamics and the corporate network at a fine spatial scale over six decades.

The chapters address three related sets of questions. First, how does improved market access through road infrastructure construction affect the number, size, and dynamics of firms? Second, what are the consequences of large place-based industrial policies on firms, workers, and taxpayers in targeted municipalities and their neighbors? Third, how is the corporate network embedded in space, and how do distance, cantonal borders, and cultural borders shape connections between firms and directors? Taken together, the essays show how reductions in physical distance, targeted public interventions, and organizational ties jointly structure the spatial economy.

The first chapter, "*The effects of highway access on firm creations, deaths, and relocations*", examines the consequences of the rollout of the Swiss national highway network for firm dynamics in non-urban municipalities between 1960 and 2003. The analysis focuses on municipalities that obtain nearby access to the high-speed road network and exploits the staggered timing of openings to identify their local impact on firms. Using Poisson Quasi-Maximum Likelihood estimators in a staggered difference-in-differences framework, the chapter estimates the dynamic effects of highway access on the local stock of firms, firm births and deaths, and firm relocation across municipalities. The results show that new access points generate a persistent increase in the number of firms in newly connected municipalities. This growth is driven primarily by higher net firm creation rather than by pure relocation: both entries and exits rise, but the increase in firm births dominates, consistent with stronger competition and selection effects. The chapter also documents heterogeneity by firm size (proxied by nominal capital and board size) and tests robustness to alternative treatment definitions and estimation strategies. Connected municipalities near urban centers are experiencing only an increase in firm exits. However, in municipalities far from cities, we observe an increase in the number of firms, with positive effects on both creations and deaths. These patterns suggest that exits in municipalities near cities are not due to increased local competition, but rather to urban shadows. These findings speak to the debate on whether infrastructure investments actually expand economic activity or merely reorganize it across space.

The second chapter, "*Place-Based Policy, Firm Dynamics, Local and Spatial Effects: Evidence from Swiss Firm Data, 1960-1993*", turns from infrastructure to regional policy. We study the long-run local effects of the 1978 Federal Decree on the Promotion of Economically Endangered Areas (Lex Bonny), which offered tax relief, subsidized credit, and guarantees to firms investing in distressed municipalities. Exploiting spatial variation in policy eligibility, the chapter combines several difference-in-differences designs to estimate how the program affects the number of firms, their creations, deaths, and relocations, as well as employment and tax base at the municipal level. By comparing treated municipalities with carefully defined control groups and incorporating spatial spillovers, the chapter assesses whether Lex Bonny succeeded in fostering durable local economic development. In doing so, we contribute to current debates on the effectiveness of place-based and industrial policies.

The third chapter, "*Firms, Directors, and the Geography of Corporate Network: Evidence from Swiss Firms, 1960–2000*", shifts the lens from firm counts and policy interventions to the structure of corporate networks. We reconstruct the bipartite network linking all Swiss limited companies to their directors from 1960 to 2000 and project it into firm-to-firm and director-to-director networks. Aggregating these networks at the municipal level, the chapter documents how the intensity of connections between municipalities depends on geographic distance, cantonal borders, language regions, and religion. Dyadic gravity-style regressions track how border effects evolve as transport costs fall and the domestic market integrates. The analysis shows the extent to which internal administrative and cultural boundaries continue to segment the corporate network even as infrastructure improves, and how these effects vary across networks. This descriptive work bridges the literature on interlocking directorates and on spatial and internal border effects by embedding corporate ties within space.

Across the three essays, the thesis makes three broad contributions. First, it assembles and exploits new long-run data on firms and directors that permit tracking the evolution of Switzerland's corporate landscape over six decades at a fine spatial scale. Second, it brings together quasi-experimental designs and spatial network analysis to study how infrastructure, place-based policy, and organizational networks shape the geography of firms. Across all chapters, it becomes clear that the core issue in spatial analysis is the appropriate selection of control units. Being close to treated regions can expose control units to spillovers, while municipalities located far from the treatment might be incomparable. The thesis uses various approaches to address these two issues. Third, it offers evidence that improvements in accessibility and targeted subsidies can have lasting impacts on firm creations, deaths, and locations, but that these forces operate within a corporate network whose structure remains strongly shaped by distance, internal borders, and urban hierarchy.

The remainder of the thesis is organized as follows. First, we conclude the introduction with abstracts of the four chapters. Then, Chapter 1 studies the impact of highway access on firm dynamics in rural municipalities. Chapter 2 evaluates the long-run effects of Lex Bonny on firms and employment in distressed regions. Finally, Chapter 3 documents how distance and internal borders structure the Swiss corporate network over time.

Note to the reader The chapters of this thesis are presented as independent and unlinked chapters that can be read independently. The following abstracts provide an overview of the key themes and findings of each study.

Abstracts

Chapter 1 - **The effects of highway access on firm creations, deaths, and relocations** We analyze how the construction of highways in Switzerland affects the stock, births, deaths, and relocations of firms at the municipal level. To achieve this, we construct a novel geo-referenced dataset containing all limited companies based in Switzerland between 1934 and 2003. We exploit variation in the timing of access openings and use a staggered difference-in-differences approach to estimate the effect of road development. We find positive and long-term effects on the number of firms in the treated municipalities. Using rich panel data, we show that highway access increases both the creation and death of firms. We do not find systematic evidence of firm relocations in our baseline specifications. The absence of relocations suggests that highways lead to pure economic growth, rather than reorganization. Allowing for heterogeneous effects by cohorts does not change the aggregated results. We also document heterogeneous effects depending on two measures of firm size. Deaths mostly occur in municipalities close to cities, while births happen further away. The results are consistent across various robustness checks.

Chapter 2 - **Place-Based Policy, Firm Dynamics, Local and Spatial Effects: Evidence from Swiss Firm Data, 1960-1993** This chapter evaluates the long-run effects of the Lex Bonny, a Swiss place-based regional policy introduced in the late 1970s to support distressed municipalities through investment incentives and tax relief. Using newly digitized data on the universe of Swiss corporations, we construct a municipal panel from 1960 to 1993 and examine how the program affects the number of firms and their entry, exit, or relocation in eligible municipalities. We exploit geographic variation in eligibility in a dynamic difference-in-differences design and use distance-based specifications to test for displacement. We find persistent increases in the number of firms in eligible municipalities, driven primarily by higher entry rates. Spatial estimates suggest effects are highly localized, with limited spillovers to nearby areas. However, some spatial effects appear in mid-range control units. Complementary analyses of workers and taxpayers indicate modest and heterogeneous responses: firm growth does not translate into employment gains, and changes in the tax base are concentrated among lower- and middle-income taxpayers. Overall,

Lex Bonny increased the number of firms in targeted areas, with no apparent effects on local labor-market and fiscal outcomes. However, we observe positive effects on employment and the number of taxpayers in nearby municipalities outside the targeted areas.

Chapter 3 - Firms, Directors, and the Geography of Corporate Network: Evidence from Swiss Firms, 1960–2000 This chapter documents how distance and internal borders shape the geography of Swiss corporate networks over the second half of the twentieth century. Using comprehensive data on all Swiss corporations and their boards of directors, we reconstruct the bipartite firm–director network and its firm-to-firm and director-to-director projections. We aggregate the three different networks to the municipal level. We combine descriptive evidence with gravity-style dyadic regressions that model the intensity of connections between pairs of municipalities as a function of distance and shared institutional or cultural boundaries (e.g., cantons, language regions, and religions). Our results suggest large and persistent effects of distances and borders over time. In comparison, improvements in transport accessibility do not seem to play a major role in municipal-level economic interactions.

Chapter 1

The effects of highway access on firm creations, deaths, and relocations*

1.1 Introduction

Highways, railroads, and air travel are central in shaping spatial economic activity. Enhanced infrastructure reduces transportation costs and, thus, facilitates the movement of goods, market accessibility, and the procurement of inputs. Highways increase economic activity in rural areas near them compared to regions farther from the road network (Redding and Turner 2015). In this sense, better infrastructure improves local economic integration by reducing transportation costs. Switzerland had no highways before WWII. Policymakers discussed the project immediately after the war ended, but concrete advances took additional years. The federal government largely defined the highway network in the 1950s and finalized the project at the end of the decade. The highways were then almost entirely constructed between 1960 and 2010. Although the plans clearly defined the main connections, the exact route was revised later.

This paper examines how the construction of the Swiss highway network affects firms in Swiss municipalities. The expansion of Switzerland's highway network reduced transportation costs and integrated domestic markets over several decades. We examine the change in the number of firms, their entry, exit, and relocations following the opening of new highway access.

To do so, we create a new historical dataset by collecting and digitizing information on all corporations ("*Aktiengesellschaften*" or "*Sociétés anonymes*") in Switzerland between 1934 and 2003 from the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*". This publication lists all individuals with at least one mandate

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*This chapter is a collaborative work with Mark Schelker. We thank Emilie Dousse and Julie Uldry for their fantastic and careful work in the data cleaning process. We benefited from constructive comments and helpful feedbacks from participants at the 16th Beyond Basic Questions (2024), the 80th Annual Congress of the International Institute of Public Finance (2024), the PhD Seminar at the University of Fribourg (2024), the Annual Congress 2025 of the Swiss Society of Economics and Statistics (2025), and the European Economic Association Conference (2025).

on the board of directors of a corporation in the country[2]. Starting with cross-sectional firm-level data, we first geolocalize firms and directors. After cross-sectional cleaning, we match firms across periods to construct our firm-level panel.

The geographical units of interest are the Swiss municipalities. Thus, we aggregate firm-level data into a municipal-level panel, including the number of firms, board-of-directors information, and nominal capital, along with the distribution of these variables in Switzerland. In addition to the number of firms, we measure the number of newly created firms every year for each municipality. We also know how many firms cease to exist or relocate. This information on the firm dynamics allows us to investigate what drives changes in the number of firms. We aim to answer the following questions: (i) Are the highways increasing economic activity measured by the number of firms? (ii) Are the changes due to firm creations, to firm deaths, or to relocation? (iii) Are the changes coming from small or large firms? (iv) Is access beneficial or not when connected municipalities are close to urban centers?

We rely on the inconsequential unit approach pioneered by Chandra and Thompson (2000) and on highway access data in Switzerland from Fretz et al. (2021). Fretz et al. (2021) collected data on the specific timing of highway openings, which connected different municipalities with a much faster alternative transport route. More specifically, we first evaluate the change in the number of firms at the municipal level after the opening of nearby access to the highway network. We compare municipalities receiving early access to highways with those receiving it later. This timing-based approach allows us to identify the effect of proximity to highways on the number of firms at the municipal level. Second, we track firms over time and determine whether the effects are due to firm creations, deaths, or relocations. Third, we evaluate the change in the distribution of firm size, measured by both nominal capital and the number of board members. These two variables serve as proxies for company size. This allows us to investigate heterogeneous effects by firm size. Finally, we observe different effects depending on the distances between connected municipalities and their nearest urban centers.

This paper contributes to the literature in three main aspects. Firstly, we provide evidence on whether reductions in transportation costs increase economic activity or primarily reorganize existing activity across space (Fogel 1964, Redding and Turner 2015). Our unique firm-level panel tracking births, deaths, and relocations allows us to contribute to the "growth versus reorganization" discussion. Secondly, we observe heterogeneous effects depending on distance to access, proximity to cities, and firm size. Firm entry increases primarily in municipalities farther from cities, while firm

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[2] For simplicity, we refer to those corporations as firms from now on.

exits are more pronounced in municipalities closer to urban centers, consistent with an urban shadow effect which predicts that connectivity intensifies competition from cities (Rauch 2013, Bosker and Buringh 2017, Cuberes et al. 2021). Importantly, the results show that this urban shadow effect is not uniform across rural municipalities but varies systematically with distance to cities. Thirdly, the effects differ across firm size classes: smaller firms exhibit higher turnover following highway access. By jointly analyzing these mechanisms, the paper shows that highways do not affect rural areas uniformly but instead reshape the spatial and organizational structure of firms through the interaction of agglomeration forces, competitive pressure from urban centers, and heterogeneous firm responses. Our results shed light on the effects of transportation improvements on spatial changes in economic activity.

We quantify the dynamic and long-run effects of receiving highway access on the variables of interest at time t relative to receiving it later. For the number of firms, we observe a statistically significant and positive effect 1 year after the opening of access in the specification that includes the full set of fixed effects and municipal time trends. The stock of firms accumulates over time after the construction of the highways, consistent with long-term changes in economic activity. We complement the main analysis with a range of robustness checks that support our results. These results are robust to alternative definitions of the treatment and control groups. We also examine whether the timing of highway access matters using an extended two-way fixed effects (ETWFE) estimator for staggered difference-in-differences, which allows for non-negative count data (Wooldridge 2021, 2023).

The effects of highway access on births, deaths, and relocations indicate that entries drive the increase in the number of firms. First, the number of newly created firms in the treated municipalities increases prior to the opening of access. Highways also increase the creation of firms in the long run. Second, the number of firm closures also increases, both before access opening and in the long-run, reflecting increasing competitive pressure and selection effects. Finally, the results on the relocation of firms to and from the treated municipalities are inconclusive. We do not find any relocation effects according to our estimates. This suggests that the increase in the number of firms is primarily driven by the creation of new firms, as we find no conclusive evidence of relocation effects.

Considering the distance between newly connected municipalities and urban centers helps us understand the mechanisms at play. Connected municipalities near urban centers are only experiencing an increase in firm exits. However, in municipalities far from cities, we observe an increase in the number of firms, with positive effects on both creations and deaths. These patterns suggest that exits in municipalities near cities are not due to increased local competition, but rather to urban shadows. For municipalities further away from cities, the increase in exits is less pronounced. This is likely because the only force driving firms out of business is the entry of new local competitors. Overall, increased competition (both locally and from other cities) put pressure on incumbent firms. The total effect on the number of firms remains positive nonetheless.

The structure of this chapter is as follows. First, in Section 1.2, we review the related literature and provide the theoretical framework. Section 1.3 describes the context in which the development of the highways in Switzerland occurred. Section 1.4 presents our data and provides descriptive statistics. In Section 1.5, we describe our empirical approach. Section 1.6 presents our empirical results, while Section 1.7 contains different robustness checks. Section 1.8 concludes the chapter.

1.2 Theoretical framework and related literature

1.2.1 Theoretical framework

We focus on investments in Switzerland's national high-speed road network and their effects on economic activity, specifically on firm creation, death, and relocation.

Transportation network developments improve market access for firms, resulting in comparative advantages in access to workers, managers, consumers, suppliers, or resources relative to competitors farther from these networks (Eberts and McMillen 1999, Bernard and Moxnes 2018). Bernard et al. (2019), for example, provide evidence that a comparative advantage occurs across firms within a country through reductions in transportation costs. In addition, transportation networks have long-term effects on economic integration and trade between regions (Barjamovic et al. 2019, Flückiger et al. 2021). Consequently, firms should be attracted to locations near highway access, given the long-lasting advantages they offer in addition to the short-term benefits. However, new firms imply stronger competition for incumbents. Increasing local competition could lead incumbent firms to exit or close if they are not productive enough (Melitz 2003).

Newly connected municipalities are now more closely connected to cities due to reduced transportation times. Thus, depending on the distance between them and the cities, rural firms might face increasing competition from urban firms. This can negatively affect incumbent firms in rural connected municipalities. This relates to the concept of urban shadows, which states that proximity to a large economic center negatively affects nearby smaller municipalities. A central result in the New Economic

Geography theory is the existence of such shadows, which prevent another urban area from forming near a larger urban area due to spatial price competition (Krugman 1991).

At the same time, proximity to large urban centers may foster firm creation through improved urban access. When transportation and commuting costs fall sufficiently, firms located in peripheral municipalities can partially benefit from urban agglomeration economies without relocating into cities. Improved access expands effective market size, allowing rural firms to reach urban consumers at lower cost and increasing expected demand and profitability. In this context, competition from urban firms does not necessarily lead to exit, with new firms entering to exploit local comparative advantages.

The basic trade-off between the two forces is intuitive. On the one hand, proximity to a large city implies stronger competition and greater difficulty surviving *in the shadows* of the city center. On the other hand, small municipalities benefit from this access through commuting and from the large demand in the center (Cuberes et al. 2021).

To summarize, highway access creates opposing effects on firm numbers. Highway access reduces spatial frictions, while distance to large urban centers determines whether this reduction primarily intensifies competition or facilitates access to urban markets and inputs. First, highways improve market access for inputs, consumers, workers, and managers while reducing transportation costs. This attracts firms and generates agglomeration economies. Second, highways intensify competition (locally and from urban firms), which pushes some firms to exit or close. These opposing forces make the net effect on firm numbers ambiguous. Meanwhile, remote locations lose their isolation advantage as lower transportation costs expose them to competition from urban centers (Proost and Thisse 2019).

1.2.2 Empirical evidence

The focus of this paper is on firm dynamics[3]. Datta (2012) studies the impact of upgraded roads in India on the inventory management of firms along the Golden Quadrilateral (GQ) program. Firms near the improved highways reduce their inventory and experience more efficient supplier turnover than those farther away. Reductions in transportation costs facilitate productive choices of nearby firms. Ghani et al. (2016) also investigated the Indian GQ program, focusing on the creation of new manufacturing plants and industry-level sorting near highways. They find a whopping 49% increase in output levels in districts crossed by the network over ten years after the program began. Incumbent firms are more productive and have a lower closure rate, despite higher competition from entries.

In a series of papers, Holl (2004a,b,c) focuses on manufacturing firms at the plant level. In her first study, she finds an increase in plant entries in municipalities located within the first 10 km near highways compared to those outside the 10 km corridor Holl (2004a). She uses the distance to the road itself, rather than the access to the road network, which might introduce errors in the effect of the highways, as being close to the roads is not equivalent to being close to the accesses. In a second study, Holl (2004b) shows that the positive effect is concentrated in this 10 km corridor near the highways for a variety of industries. Municipalities close to roads attract firms at the expense of those further away, especially those located between 20-30 km from the highways Holl (2004c).

More closely related are the papers from Duranton and Turner (2012), Percoco (2016), Gibbons et al. (2019), and Busso and Fentanes (2024). Duranton and Turner (2012) find positive results on employment in US cities. They estimate an increase of 1-5% in employment over 20 years when the initial "stock" of highways in cities increases by 10%. Percoco (2016) finds that the opening of highway access in Italy affects employment and plants with growths of approximately 4-5% and 2-3%, respectively.

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[3] Several studies examine the relationship between highways and segregation (Brinkman and Lin 2024, Mahajan 2024), land prices and urban development (Atack et al. 2010, Duranton and Turner 2012, Hornung 2015, Donaldson and Hornbeck 2016, Levkovich et al. 2019), political outcomes such as political participation (Huet-Vaughn 2019) or geographic polarization (Nall 2015). Volpe Martincus and Blyde (2013), Duranton et al. (2014), and Donaldson (2018) are examples of works estimating the effect of road development on trade, while Michaels (2008) investigates the increase in demand for trade-related skilled workers near new roads. The effects on employment are also regularly studied, and the results indicate positive effects following highway developments (Duranton and Turner 2012, Percoco 2016, Gibbons et al. 2019). Sanchis-Guarner (2012) provides evidence that changes in market accessibility affect wages and hours worked. Many studies investigate the effects of investments in transportation infrastructures on various outcomes in developing countries (Datta 2012, Faber 2014, Ghani et al. 2016, Jedwab and Moradi 2016, Storeygard 2016, Baum-Snow et al. 2017, Qin 2017, Volpe Martincus et al. 2017, Aggarwal 2018, Banerjee et al. 2020, Baum-Snow et al. 2020, Baragwanath Vogel et al. 2024, Dumas and Játiva 2024).

This growth in the number of firms is more pronounced in transport-intensive sectors. He also finds that population growth tends to concentrate near these access points. Gibbons et al. (2019) find positive effects on employment and the number of firms in Great Britain after road improvements. The increase in employment occurs through the entry and exit of establishments. Busso and Fentanes (2024) show that new highways in Mexico led to higher local labor productivity primarily through changes in firm dynamics. In their causal estimates, they find increases in firm entries and decreases in firm exits (by 1.6 and 3.3 percentage points, respectively). However, the contexts differ widely between Busso and Fentanes (2024) and our research. Their study examines Mexico between 1998 and 2018, while our study focuses on Switzerland between 1960 and 2003, a high-income but small country with mature infrastructure (railways and roads), and closely located urban centers.

Several studies directly examine the trade-off between urban shadows and improved urban access on economic growth and demographic outcomes in rural municipalities. Partridge et al. (2009) study population dynamics across the US urban hierarchy and finds that proximity to urban centers is associated with positive growth. Cuberes et al. (2021) study the evolution of the trade-off and find that between 1840 and 1920, urban shadows dominated. After 1920, proximity to large urban centers is positively associated with growth. The effect of proximity can vary with factors such as commuting and shipping costs. The empirical results do not reach a consensus. For example, Qin (2017) finds that incumbent firms in rural and newly connected municipalities in China are negatively affected by railway development. To our knowledge, no further studies have examined firm dynamics following changes in road connectivity with large urban centers.

Our paper relies on publicly available data on municipal-level highway access in Switzerland collected by Fretz et al. (2021). Therefore, there is a strong link with their paper. In their study, they focus on income segregation in municipalities within 10 km of highway access. Their findings reveal a long-term increase of 24% in the share of top-income taxpayers, coupled with a notable decrease of 8% in the share of low-income taxpayers. They also estimate a 4.9% increase in the number of workers living in those municipalities, due to the easier commute via the new roads. We focus on rural municipalities. Artz et al. (2016) find that agglomeration economies play a major role in determining the location choice between small and large rural municipalities. They find that rural areas with higher agglomeration economies serve as clusters that provide labor to workers and commuters near cities. Their findings align with the comparative advantage argument in Bernard et al. (2019). Even in rural areas, highways enhance agglomeration economies and attract firms to markets that are already benefiting more from such economies.

1.3 Context

Highways were non-existent in Switzerland before 1950, but the number of cars was rapidly growing. Initial estimations of the increase in traffic massively underestimated the rise in car use in the country. To address the significant increase in cars and motorized traffic, the federal authorities launched an ambitious program in 1951. On March 21, 1958, the Swiss population approved a popular initiative[4] to entrust the Confederation with the task of establishing a national road network. The Swiss Parliament approved the building program in 1960. After enacting the "*Federal Law on National Roads*", the Confederation and the cantons embarked on the realization of the Parliament-approved national network project, spanning 1811 km. The upper part of Figure 1.1 shows where the first sections of the network were built. The yellow dots on the maps indicate the access already opened in 1960. Similarly, the lower part shows the construction made until 2003.

The program aimed to link the largest cities (Basel, Bern, Geneva, Lausanne, and Zurich) and connect other important ones (Baden, Biel, Fribourg, Luzern, Neuchâtel, Olten, Sankt-Gallen, Solothurn, Winterthur, and Yverdon). Between those big cities, the planners considered several options. For example, the highway linking Bern to Lausanne had nine possible layouts before construction[5]. Ultimately, the highway section passes next to three important regional cities in addition to Fribourg: Bulle, Châtel-Saint-Denis, and Vevey. Their regional political influence and economic importance relative to other municipalities likely played a significant role in the planning[6]. The first completed sections included a segment of the A2 from Hergiswil to Luzern in 1962 and a section of the A1 between Geneva and Lausanne, which was opened in 1963.

The federal government initially planned construction over 20 years, with an estimated cost of 2.93 billion CHF. However, the cost estimates were even more inaccurate: in 1997, a report from the Control Committee of the National Council sounded the alarm about the costs of building highways. 34.5 billion CHF had already been spent by the Confederation, in addition to the 5.7 billion CHF invested by the

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[4] Federal decree of the 21st of March 1958 regarding the popular initiative on the improvement of the road network

[5] This makes the use of planned highways as instrumental variables impossible in our context. See Figure 1.A.2 in Appendix 1.A.3.

[6] This could raise endogeneity concerns as those municipalities do not receive access randomly. However, all the mentioned municipalities are either urban centers or suburban municipalities and are excluded from our analysis.

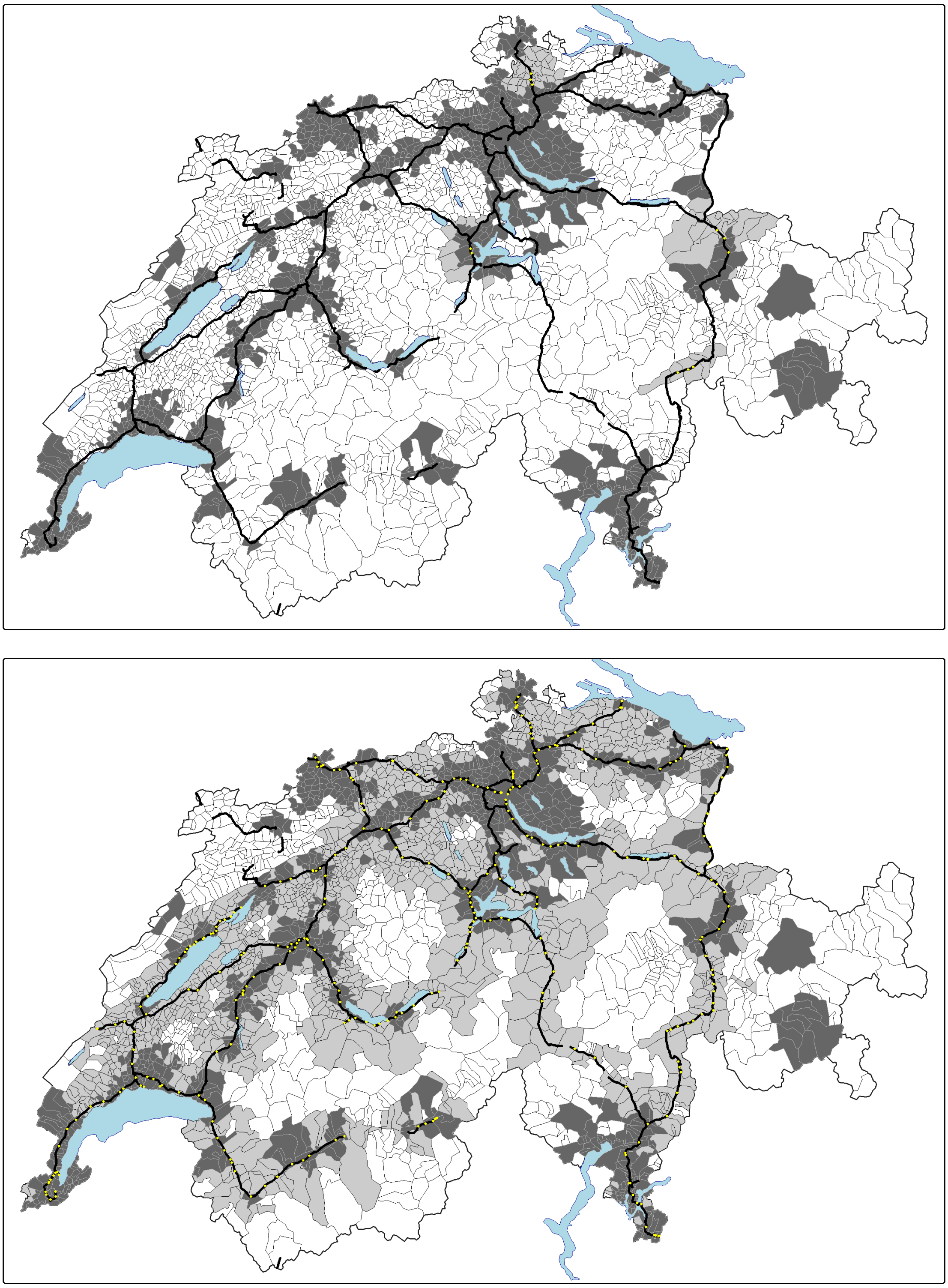
Cantons. They estimated the remaining costs at around 25 billion CHF, highlighting the complexity of the project. The increase in costs led to project delays, ensuring that funding could continue to be supported by fuel taxes.

The development of the Swiss highway system was not without opposition. Like many large-scale infrastructure projects, highway development faced delays, but also criticism from various groups. For example, the A1 highway was originally planned to be built over swamps, which were initially selected due to their low agricultural value. Various ecological groups opposed this layout, seeking to protect the swamps from destruction. This fierce opposition during the 1970s forced planners to consider other options, who were also facing severe criticism from the agricultural lobby. The bill for the A1 began to balloon in the mid-1980s when the wishes of opponents (such as environmentalists, farmers, and local authorities) were taken into account, necessitating major changes in the planned layouts.

This example, coupled with the financial delays mentioned earlier, is one of many cases in which the initial plans and the timing of its opening did not proceed as expected. In 1967, the authorities estimated that the network would be completed in 1987. In 1995, this estimation was delayed to 2012. The construction time doubled the initially planned duration. In 2010, the network was nearly complete, with 94.6% of the initial project completed. Figure 1.2 illustrates the number of accesses opened each year in addition to the completion rate. As we consider only treated municipalities, the completion rate reaches 100% at the end of the sample.

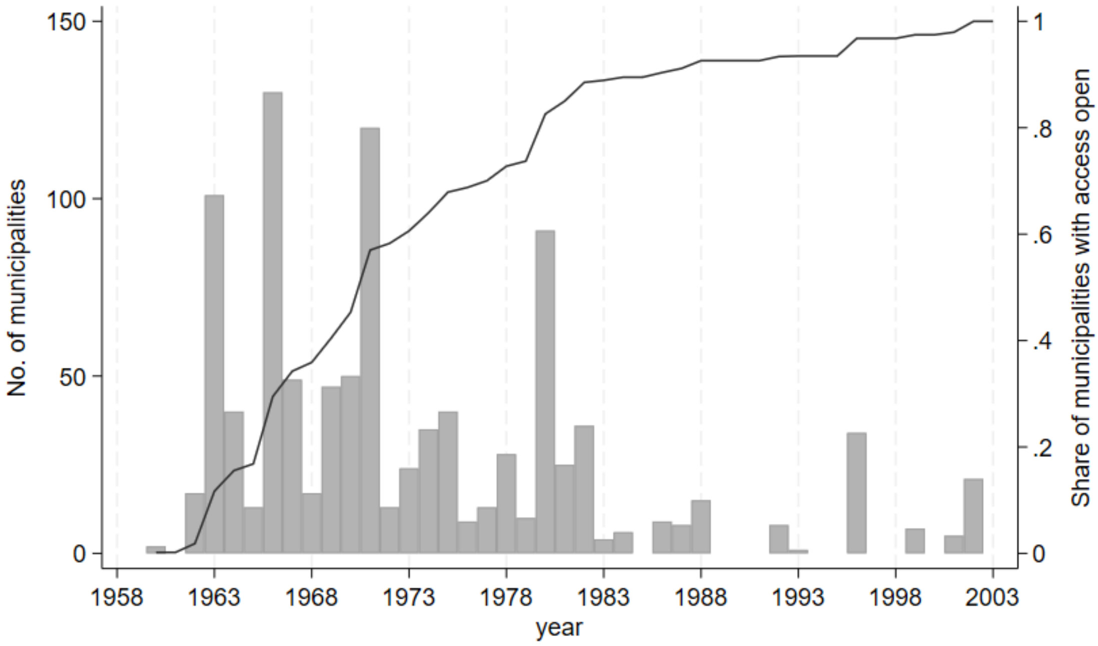
Of course, transport is not limited to roads. The Swiss railway network was developed long before any highways were planned. The major changes came late in the last century with the project *Rail 2000*. *Rail 2000*, launched in the mid-1980s and approved by popular vote in 1987, aimed at a major shift in Swiss rail policy. However, the first phase, consisting mostly of the construction of the Mattstetten–Rothrist high-speed line, was completed only in December 2004. Several planned segments, including the full Lausanne–Berne speed upgrade, were postponed. Other modifications were implemented, including reorganizing train schedules, modernizing rolling stock, and improving the regular interval timetable. Although some changes occurred, the focus remained on improving the commuter lines and timetables. No major new rail lines were constructed during our sample period (Swiss Federal Office of Transport 2007). In any case, municipalities affected by *Rail 2000* are mostly excluded from the analysis because they are either cities or part of agglomerations.

Figure 1.1: Location of all access built until 1960 and 2003



Notes: Yellow dots show the highway access. Dark grey areas show cities & municipalities within agglomerations. Light grey areas indicate rural municipalities with access within 15 km of their centroids. Based on Fretz et al. (2021) and Swiss Federal Statistical Office (2005).

Figure 1.2: Number of municipalities gaining access by year and share of municipalities with access (15 km or less)



Notes: This figure shows the number of municipalities receiving an access by year, as well as the share of total municipalities with an access. The right axis shows the share of treated municipalities, which sum to 100% because we restrict the sample to the eventually treated municipalities.

1.4 Data description

We digitized all existing editions of the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*" from 1934 to 2003. [7] These collections are based on the cantonal commercial registries and contain every person who holds a mandate on the board of directors of corporations in a given year, including information on their name, address, function on the board, firm name, firm location, and nominal capital. This publication allows us to extract the yearly cross-sectional universe of firms in Switzerland for the years 1934, 1943, 1960, 1962-1966, 1969, 1975, and 1979-2003. Using the resulting cross-sections, we construct a municipal-level panel of the stocks of firms[8]. Since we can track firms across periods, we can also generate more detailed outcome variables, including the number of births, deaths, and relocations within or outside of the municipalities. We exclude 1934 and 1943 because the former contains only a subsample of firms, and the latter is too distant from the next available year, 1960. Our final dataset covers the years 1960 to 2003[9].

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[7] Figure 1.A.1 in the Appendix shows an example of the publication.

[8] See Appendix 1.A.1 for details on the procedure.

[9] We explain how we deal with the gaps in our different outcome variables in Appendix 1.A.2.

Our dataset also includes the municipal categories defined by the Federal Statistical Office (Swiss Federal Statistical Office 2005). Our sample contains 27 "*urban centers*", 865 "*suburban municipalities*", and 1469 "*non-urban municipalities*". In our analysis, we use only non-urban municipalities to create our treatment and control groups[10]. Table 1.B.1 in the Appendix provides summary statistics by municipality type.

Regarding the evolution over the considered period, Table 1.B.2 illustrates the importance of each type of municipality at the beginning of the sample compared to the end. Let us take the number of firms as an example. Only 8% of the firms are located in the "*Non-urban connected municipalities*" between 1960 and 1970. At the end of the sample, between 1990 and 2003, this share increases to 12%. This suggests that the relative importance of the rural municipalities grows over time. The last line indicates the change in the importance of each type of municipality between before and after the construction of highways.

The interesting aspect of Table 1.B.2 is that urban centers experienced a decline in relative national importance across all variables: the number of firms, firm creations, firm deaths, firm relocations, and employment-related variables. Suburban municipalities experienced the largest relative increase. This finding aligns with those of Fretz et al. (2021), who also report some residential urban sprawl. Economic activities, as measured by our firm variables, appear to extend beyond cities between 1960 and 2003.

The firms are geolocated at their official headquarters. This could be an issue if the headquarters are elsewhere than the production sites. However, most firms are small, making it likely that they have both their headquarters and production facilities in the same location[11]. Regarding larger firms, they tend to have several distinct registered firms (e.g., holdings, management entities, etc.), increasing the likelihood that their headquarters location and production sites are somewhat aligned.

Our main variables of interest are the number, creation, closure, and relocation of firms within Switzerland. We extend the analysis and evaluate the change in the number of firms with nominal capital above and between certain percentiles (50th to 75th, 75th to 90th, 90th to 99th, and top 1% percentiles). We compute percentiles using the nationwide distribution of capital for each year. We also proxy for firm size using the size of their boards of directors, with the hypothesis that larger firms tend to have larger boards.

The number of firms in each municipality is a nonnegative integer. A common way

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[10] Because municipalities absorbed by urban sprawl by 2000 are classified as urban or suburban and are not in the sample, our analysis abstracts from urban sprawl and focuses on how highways affect non-urban municipalities.

[11] Around 37% of the firms in the sample have only one board member, as shown in Table 1.B.1.

to model count data is as a Poisson-distributed variable. The Poisson distribution explicitly accounts for the integer nature of the dependent variable and includes zero observations as a natural outcome. An analysis focusing on rural municipalities implies zero counts for observations in certain years. The proportion of zero observations is large in our sample. In the full sample, municipal-level observations without a corporation account for 20.60% of all observations. This percentage increases to 27.69% when we consider only non-urban municipalities. Figures 1.B.3 and 1.B.4, in Appendix 1.B.1, illustrate the skewness of the frequency distributions of the number of firms. These facts also apply to the more detailed outcome variables (creation, deaths, and relocations), which also feature zeros.

We rely on the publicly available data of Fretz et al. (2021) for the timing and location of the highway access. More specifically, we know the road distance from the centroid of each municipality to the nearest entry point of the high-speed network. We define the treatment group as the municipalities with access within 15 km. We allow for this definition to vary in our robustness checks. A more common distance is 10 km (Holl 2004a,b,c, Faber 2014, Fretz et al. 2021), but our results (see Table 1.1 in Section 1.6.3) suggest that part of the effect also occurs further away. We also use the variable indicating whether each municipality has a railway station from their dataset. We also make use of data from the Federal Population Census (Swiss Federal Statistical Office 2000a), the Business Census (Swiss Federal Statistical Office 1955), and the distance to the closest urban center from Fretz et al. (2021).

1.5 Empirical methodology

1.5.1 Identification approach

The empirical approach relies on the identification strategy and the data available in Fretz et al. (2021) on the specific timing of highway openings in Switzerland. We complement the analysis using Wooldridge (2021, 2023) to allow for potential heterogeneous effects across cohorts and over time. We exploit the timing-based data structure and restrict the analysis to ever-treated units to limit selection bias. As in Hornung (2015), Donaldson (2018), or Fretz et al. (2021), all municipalities are eventually treated in our baseline analysis. However, the timing of the opening of new highway access varies across the treated municipalities. This staggered treatment allows us to use municipalities that receive access in later periods as controls for the already treated ones.

Given the identification challenges inherent to infrastructure placement, our empirical strategy relies on several assumptions. First, conditional on municipality fixed effects, year fixed effects, municipality-specific time trends, and the restriction to ever-treated non-urban municipalities, municipalities would follow similar deviations from their long-run trajectories absent treatment. Second, we assume limited anticipatory responses prior to the chosen reference period. We allow for potential anticipation during the construction phase. Thus, identification requires that firms not enter or relocate earlier than this reference period. Third, we assume that treatment effects are not driven by municipalities directly influencing highway routing decisions. To support this assumption, we restrict the sample to non-urban municipalities that receive access primarily due to their geographic position between major cities, following the "*inconsequential place approach*" (Chandra and Thompson 2000). Finally, we assume no spatial spillovers in our baseline regressions, so that later-treated municipalities provide valid pre-treatment controls (i.e., the Stable Unit Treatment Value Assumption (SUTVA) holds).

To address potential omitted variable bias, we include unit fixed effects, year fixed effects, and municipality-specific time trends in our regressions. The fixed effects account for nationwide shocks and municipal time-invariant differences, while the municipality-specific time trends remove differential growth paths. Identification is therefore driven by within-municipality deviations from their long-run trajectories.

Of course, the positions of the access are not random and depend on the specific socio-economic and geographical contexts. On the one hand, it is no coincidence that Switzerland's main cities are well-connected and close to several highway exits. The purpose of such high-speed roads is to connect large cities and facilitate interactions between them. On the other hand, there are remote municipalities where it makes little economic sense to build such infrastructure. For these two reasons, treatment is not random. Thus, selection bias poses a major threat to identification. Never-treated municipalities may be incomparable to treated ones, but so are municipalities treated earlier compared to those treated later.

To mitigate this bias, we need to carefully construct treatment and control groups in which the treatment is quasi-exogenous. First, we exclude remote municipalities from our control group. Considering only ever-treated municipalities addresses part of the endogeneity problem posed by the non-random location of highway access. Second, we rely on the "*inconsequential place approach*" (Chandra and Thompson 2000). We restrict our analysis to non-urban municipalities, as they receive the treatment mostly due to their locations between major cities. Due to their small size, these municipalities are unlikely to influence highway routing decisions. As discussed in the context section, the main goal was to link big cities rather than to develop economic activities in remote municipalities.

Following the recommendations of Miller (2023), we avoid setting the reference period mindlessly. There is no straightforward solution in our context, so we must rely on a judgment call. To account for possible anticipatory effects, we set the reference period 4 years prior to the treatment (Duncan 2011). We use this reference period because, as stated by Fretz et al. (2021), data from specialized websites suggest that the construction time for large infrastructure, such as bridges or tunnels, is approximately 4 years on average.[12].

When multiple periods and groups are available, and the treatment has been implemented over several periods, the standard two-way fixed effects (TWFE) method might struggle to identify the average treatment effect. This is because it tends to use inadequate controls, assigns negative weights to treated units, and can result in biased estimates (Goodman-Bacon 2021). Although the conventional TWFE method has several limitations, it can efficiently estimate treatment effects, yielding results comparable to those of extended two-way fixed effects (ETWFE) approaches (Gardner 2022, Borusyak et al. 2024). Even though TWFE estimation may be sufficient, we implement Wooldridge's approach as a robustness check. Wooldridge (2021, 2023) shows that the ETWFE models can be adapted to allow for non-linear models. In brief, he proposes a set of saturated interaction effects to address potential bias issues in standard TWFE. We rely on his method to allow all treatment effects to vary across cohorts and over time.

We might face another issue if there are differences in railway accessibility between municipalities treated early and those treated later. Fortunately, as noted in Section 1.3, almost all railroads were already built by the beginning of the twentieth century, long before our panel begins (Büchel and Kyburz 2018). Additionally, our panel dataset ends in 2003, just prior to the expansion of the rail network in 2004. Therefore, there are no major railway construction projects during the period covered by our data. Finally, Fretz et al. (2021) provides a balance test of pre-existing railway access across treated cohorts, in which they do not observe differences between early- and late-treated municipalities. This limits potential confounding issues from different railway access.

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[12] Note that we observe only the moment when firms appear in the Commercial Registry. This means that anticipatory effects may not appear, as even if the opening or relocation is planned, the firms may not be officially registered or have not yet moved to their new location.

1.5.2 Specifications

Standard staggered difference-in-differences

We estimate the dynamic effects of highway access using the following regression equation[13]:

$$Y_{it} = \sum_{\tau=-7}^{15} \beta_\tau Access_{i,t-\tau} + \xi_i + \xi_t + \theta_i t + \epsilon_{it}. \tag{1.1}$$

In our baseline regression, Y_{it} represents the different outcome variables (number of firms, births, deaths, or relocations) in municipality i at time t. $Access_{i,t+\tau}$ is a dummy variable that equals 1 if municipality i has an entry to the highway network within 15 km of its centroid in period t. It is equal to 0 otherwise. We include municipality fixed effects ξ_i, year fixed effects ξ_t, and linear municipality time trends $\theta_i t$. Municipality-specific time trends are included to account for unobserved factors that evolve differently across municipalities and may generate differential pre-treatment trends, which could otherwise bias the estimated effect of highway access (Miller 2023). The municipality-time error term ϵ_{it} is clustered at the district level to account for possible autocorrelation over time, but also over space among municipalities close to each other[14].

The event-study equivalent of the previous regression in Equation (1.1) is:

$$\gamma_p = \begin{cases} -\sum_{\tau=p+1}^{r} \beta_\tau & \text{if } -8 \leq p \leq r-1 \\ 0 & \text{if } p = r = -4 \\ \sum_{\tau=r+1}^{p} \beta_\tau & \text{if } r+1 \leq p \leq 15. \end{cases} \tag{1.2}$$

The reference period r is -4 to account for potential anticipation of the treatment. The estimated coefficient γ_p is the effect on one of the outcome variables after the opening of an access at time p, relative to already having an access or having an access later.

The long-term cumulative effect is calculated following Davidson and MacKinnon (2004), as in Fretz et al. (2021). The reference year is -4, to match the reference year

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[13] For simplicity, in this section we write the model in the linear form. In practice, as all outcome variables are non-negative count data. Therefore, we estimate all our models via PQML regressions in Stata using the *ppmlhdfe* command.

[14] Districts are an administrative subdivision within cantons regrouping multiple municipalities.

in our dynamic estimations. Thus, Equation (1.1) becomes:

$$Y_{it} = \sum_{\tau=-3}^{15} \beta_\tau Access_{it} + \xi_i + \xi_t + \theta_i t + \epsilon_{it}. \tag{1.3}$$

Then, by adding and subtracting $\sum_{\tau=-3,\tau\neq 0}^{15} \beta_\tau Access_{it}$, we have:

$$Y_{it} = \gamma Access_{it} + \sum_{\tau=-3,\tau\neq 0}^{15} \beta_\tau (Access_{it-\tau} - Access_{it}) + \xi_i + \xi_t + \theta_i t + \epsilon_{it}. \tag{1.4}$$

This allows us to estimate $\gamma \equiv \sum_{\tau=-3}^{15} \beta_\tau$, which is the long-term effect in the second part of Table 1.C.4.

ETWFE estimator for staggered difference-in-differences (Wooldridge 2021, 2023)

The construction of the highways spans multiple decades. The staggered nature of the treatment can result in varying treatment effects across groups and over time. Therefore, we cannot rely solely on the standard TWFE as thoroughly shown in the recent literature on difference-in-differences (e.g., de Chaisemartin and D'Haultfœuille 2020 or Goodman-Bacon 2021). We rely on Wooldridge's (2021) approach, which extends the heterogeneous difference-in-differences framework to non-linear models and allows for different effects depending on time and units.

As outlined later in Wooldridge (2023), the standard ETWFE model identifies the average treatment effect under the assumption of linear parallel trends[15]. However, like in our case with count and non-negative dependent variables, this assumption may not hold. Wooldridge (2023) demonstrates that the linear ETWFE model can be adapted to accommodate non-linear models[16].

More formally, in the linear case, our baseline equation following Wooldridge (2021) is:

$$Y_{it} = \alpha + \sum_{g\in G} \sum_{t=g}^{T} \theta_{g,t} D_{i,g,t} + \xi_i + \xi_t + \epsilon_{i,t} \tag{1.5}$$

$Y_{i,t}$ is the outcome variable, $D_{i,g,t}$ is a dummy that takes the value of 1 if the

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[15] Wooldridge's (2023) approach does not allow for visually checking the parallel trend assumption, as it does not estimate pre-treatment coefficients when including not-yet treated units as controls. The estimation of pre-treatment coefficients is possible when using only never-treated units as controls.

[16] We use the *jwdid* STATA command to implement Wooldridge's (2023) approach.

observation i is in the treatment group g, on period t (and 0 otherwise). G is a set that indicates at what time treatment started for every observation, and T is the last period of the analysis. We include municipality fixed effects ξ_i and a year fixed effects ξ_t. The municipality-time error term is $\epsilon_{i,t}$ and is clustered at the district level.

$\theta_{g,t}$ are the coefficients of interest. They measure the ATE that the cohorts g experience at time t. As previously mentioned, our outcome variables are non-negative count data. We implement Wooldridge (2023) because his approach can be extended to non-linear models. In the non-linear case, Equation (1.5) becomes:

$$Y_{it} = H\left(\alpha + \sum_{g\in G}\sum_{t=g}^{T}\theta_{g,t}D_{i,g,t} + \xi_i + \xi_t\right)\upsilon_{i,t} \tag{1.6}$$

With $H(\cdot) = H(\exp)$[17]. We now denote the multiplicative error term $\upsilon_{i,t}$, to differentiate from the additive error term in Equation (1.5).

The approach is computationally demanding. We have 43 periods t and 26 different cohorts g. Therefore, we need to estimate one coefficient for every combination of t and g (every different $\theta_{g,t}$). The number of estimated parameters increases rapidly with the number of cohorts and time periods. Adding municipality time trends, as in our standard staggered difference-in-differences, would further increase the number of coefficients in our estimation. Ultimately, we do not have sufficient data to estimate a fully heterogeneous model that includes linear municipality-time trends, as in the TWFE case. Therefore, our non-linear EWTFE estimates include only time and municipality fixed effects.

After estimating the model, one can use the coefficients as direct estimates of the group and time-specific ATEs on the treated. However, one may also be interested in estimating other aggregated effects, for example, the effects across groups or periods, or the dynamic effects. We calculate dynamic treatment effects relative to the reference period and replicate our standard difference-in-differences analysis in which we include never-treated observations within 15 to 30 km of an access point.

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[17] If we assume $H(\cdot)$ to be the identity function, we go back to the linear ETWFE.

1.6 Main results - Standard DiD

1.6.1 Number of firms

We start by presenting the results for the number of firms. Figure 1.3 illustrates the accumulation of the effect over time in our full specification. Prior to the reference period, we do not observe differential trends, which mitigates concerns about the parallel trend assumption. The number of firms starts to slowly increase in period -3[18]. We observe an increase over time, with a slight plateau between periods 6 and 10, followed by continued increases up to 15 years after the opening of the access. We find a 30.34% long-term increase in the number of firms in specifications (2)[19]. Highway access increases the number of firms beyond what would be expected if treated municipalities continued along their pre-treatment trends.

The first part of Table 1.C.4 presents the cumulative effect of having an access less than 15 km away from the municipality's centroid compared to municipalities receiving similar access later in time. In our first specification, which includes municipality and time fixed effects, we do not find any statistically significant effect of the access. However, the estimates are positive and increasing over time. After introducing municipal-specific time trends in the specification (2), our estimates are much more precise, with coefficients that are quasi-identical to those in (1). Post-treatment estimates follow a similar pattern when we control for unit-specific time trends, but the estimates are substantially more precise. For the remainder of the paper, we use our full specification, and the reader can find all additional estimations and details in the Appendix.

The comparative advantage of municipalities with early access to nearby markets over those that have yet to receive similar access leads to an increase in the number of firms. This increase comes from reduced transportation costs resulting from the construction of new roads. Similar to the existing literature, we remain blind to the "growth versus reorganization" problem when using the number of firms as the outcome. We investigate the question by using the panel structure of our dataset, which enables us to track firms over time and distinguish between firm creations, deaths, and relocations.

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[18] The first statistically significant coefficient is in period +1, but the point estimates are already increasing before.

[19] The long-term effects measured following Davidson and MacKinnon (2004) are shown at the bottom of Table 1.C.4. The effect in percentage is calculated as follows: $100 \cdot \left(e^{0.265} - 1\right) = 30.34\%$

Figure 1.3: Cumulative effect before/after access (<15 km) - Number of firms

Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.4 in the Appendix provides all the details.

1.6.2 Creation, death, and relocation of firms

To investigate the "growth versus reorganization" question, we turn to our "flow" outcome variables. We estimate Equation (1.1) using the numbers of newly created, dying, and relocating firms as the outcome variables.

Figure 1.4 visually presents the effects for all flow variables. The top-left figure shows the effect on the number of newly created firms in the treatment group. We find a positive effect starting in period -3, and all subsequent periods show that highway construction induces large increases in firm creation. The increasing trend slows down after period 3. The long-term effect is 44.19%[20] as shown in Table 1.C.4 in the Appendix[21].

We also observe an increase in the number of closures after treatment, as shown in the top-right of Figure 1.4. We do not observe an effect prior to the reference

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[20] $100 \cdot \left(e^{0.366} - 1\right) = 44.19\%$

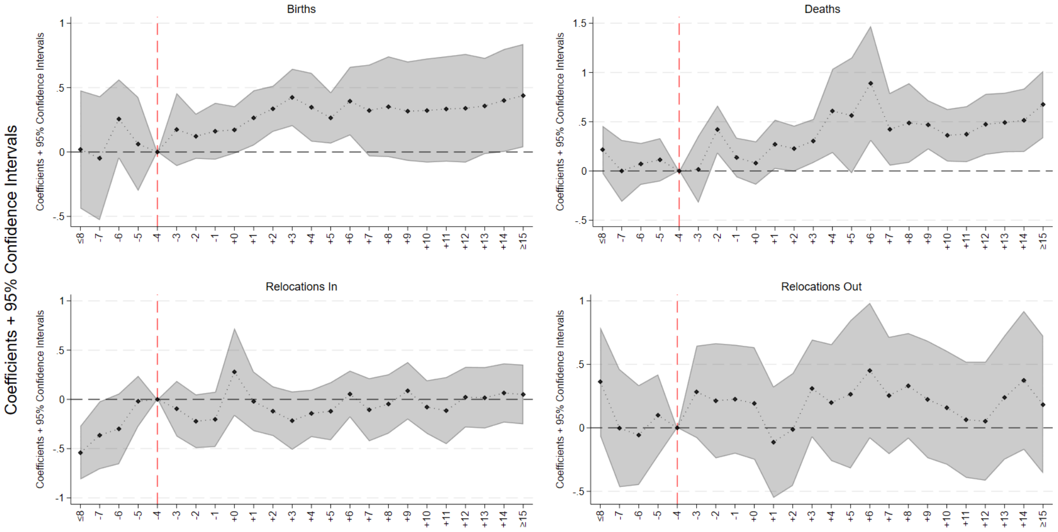
[21] Given an average of 1.46 newly created firms per municipality-year in the treatment group across all periods, this long-term coefficient corresponds to roughly 0.65 additional firm created per municipality per year. However, the mean is across all periods, so it is not an especially clean baseline for converting the estimated effects into a causal level change. This simple calculation serves as an illustration.

year, suggesting no violation of the parallel trends assumption. The cumulative effect starts to increase after period -2. The effect fluctuates more for firm closures, but again, the long-term effect shows a substantial increase in the number of firm deaths (92.51%)[22]. Highway access reduces market isolation, exposing incumbents to stronger competition due to new local entrants and from firms in urban centers.

The results for relocations in and out of treated municipalities are inconclusive, as shown in the lower part of Figure 1.4. This suggests that firm relocations are not a primary channel through which highways reshape the spatial distribution of economic activity. Finding no effect on relocations reinforces our baseline results, suggesting that SUTVA might not be a major threat to identification. More specifically, if firms are moving from untreated (even not-yet-treated) to treated municipalities, then the assumption would be violated. Here, our results suggest that the effect on the stock in treated municipalities comes from the creation and death of firms.

To sum up, the number of firms increases in municipalities that gain access earlier compared to those that gain access later. The increase in the stock of firms results from an increase in firm creation that exceeds the number of deaths. In brief, the development of highways in Switzerland appears to induce growth rather than reorganization of the economy in the treated municipalities. We investigate the potential channels behind these results in Section 1.6.3.

Figure 1.4: Cumulative effect before/after access (<15 km) - Flow variables



Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.4 in the Appendix provides all the details.

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[22] $100 \cdot \left(e^{0.655} - 1\right) = 92.51\%$

1.6.3 Spatial heterogeneity and adjustment mechanisms

We next examine the mechanisms through which highway access affects firm dynamics. In particular, we study how outcomes change with distance to the nearest highway access and with proximity to urban centers. We analyze the spatial decay of access effects by allowing the treatment to vary with distance from the highway network. We also test whether the benefits of new infrastructure attenuate with distance and whether some municipalities experience adverse effects depending on their proximity to urban centers, consistent with the presence of urban shadow effects.

This analysis allows us to distinguish between two competing forces emphasized in the literature. On the one hand, reduced transport costs can expand market access and foster firm creation. On the other hand, improved connectivity can intensify competitive pressure from urban centers, potentially leading to firm exit in nearby rural municipalities. By interacting highway access with distances to access points and cities, we directly test for the presence of urban shadow effects and assess how the balance between these forces varies across space. We then document heterogeneity in these responses across firm sizes to evaluate whether highway access differentially affects small and large firms.

Spatial decay

First, we show that the effects are spatially limited and vary with distance from the access point. Regarding the stock of firms, Table 1.1 suggests that the effect is strongest at distances of 5-10 km from a highway entry. The distance bins of 0-5 km and 10-15 km also experience an increase in the number of firms, but the estimates are less precise. The positive effect lasts only up to 15 km.

Column (2) shows that the impact on firm creation is positive in all distance bands, although smaller between 0-5 km and 20-25 km from the access. The largest effect occurs in the 5-10 km bin. Beyond 10km, the effect decreases with distance. Column (3) highlights the large increase in the death of firms concentrated near the highways, especially in the 0-5 km and 5-10 km distance bands. We find no conclusive results from examining the relocations of existing firms. The spatial decay shows no interpretable pattern in either relocation variable.

Urban shadows

Next, we estimate the effects of highway access as a function of distance to the nearest urban centers. To do so, we interact our access variable with a dummy variable

Table 1.1: Heterogeneity - Spatial decay

	Stock (1)		Births (2)		Deaths (3)		Relocation In (4)		Relocation Out (5)	
Long-term effect (0-5 km)	0.274	(0.205)	0.173	(0.316)	0.741**	(0.362)	-0.401	(0.565)	0.222	(0.560)
Long-term effect (5-10 km)	0.365*	(0.219)	0.675***	(0.230)	0.841**	(0.339)	-0.240	(0.261)	0.071	(0.365)
Long-term effect (10-15 km)	0.162	(0.149)	0.328*	(0.172)	0.271	(0.240)	0.103	(0.252)	0.133	(0.377)
Long-term effect (15-20 km)	0.011	(0.160)	0.206	(0.183)	-0.097	(0.294)	-0.341	(0.239)	-0.270	(0.392)
Long-term effect (20-25 km)	0.020	(0.111)	0.150	(0.181)	0.162	(0.292)	0.446	(0.358)	-0.204	(0.441)
# Observations	56412		55060		49579		44880		42699	
# Municipalities	1284		1259		1153		1020		993	

Notes: Include municipality fixed effects, time fixed effects, and municipal time trends. The sample includes all non-urban municipalities within 25 km of an access. We compare treated municipalities in each bin with not-yet-treated units in the same bin in a single regression. The standard errors, in parentheses, are clustered at the district level. ***p $<$ 0.01, **p $<$ 0.05, and *p $<$ 0.10.

indicating if the treated municipality is less than 20 km away from an urban center[23]. Table 1.2 shows that the increase in the number of firms is driven by the municipalities distant from the cities. This is suggested by the results in columns (2) and (3). The creation of firms also occurs in municipalities more than 20 km from urban centers. We do not observe any effect within 20 km of a city, suggesting that firms tend to avoid locating near urban centers. Regarding deaths, they are happening in both cases. The coefficient is larger closer to urban centers. Once again, results for relocation are inconclusive.

Table 1.2: Urban shadows - Distance to urban centers

	Stock (1)		Births (2)		Deaths (3)		Relocation In (4)		Relocation Out (5)	
Long-term effect										
distance $<$ 20 km	-0.013	(0.099)	0.049	(0.178)	0.662**	(0.328)	-0.205	(0.198)	-0.344	(0.415)
distance $\geq$ 20 km	0.295***	(0.105)	0.352*	(0.197)	0.285*	(0.161)	0.334	(0.223)	0.269	(0.200)
# Observations	45720		44698		40377		36916		34648	
# Municipalities	1041		1023		939		839		816	

Notes: The median distance between municipalities and the nearest urban center is 19.828 km, which we round to the nearest integer. Include municipality fixed effects, time fixed effects, and municipal time trends. The sample includes all non-urban municipalities within 15 km of an access. The standard errors, in parentheses, are clustered at the district level. ***p $<$ 0.01, **p $<$ 0.05, and *p $<$ 0.10.

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[23] The median distance between municipalities and the nearest urban center is 19.828 km, which we round to the nearest integer.

Those results are consistent with the presence of urban shadows. Highways provide a competitive advantage to rural municipalities with access compared to those without access. At the same time, high-speed roads reduce travel time between large cities and other connected municipalities. Decreasing transportation costs expand the market reach of firms in cities, which are usually more productive. Competition from firms in the city centers now reaches previously isolated markets. Table 1.2 suggests that firms close to the centers are dying way more, while no firms are getting created in those areas. The results are consistent with the presence of urban centers casting their shadows on nearby rural municipalities. For municipalities more than 20 km from the centers, we observe increases in both births and deaths. Deaths can result from both increasing local competition from firm creation and competition from city centers. We cannot disentangle the two channels, but the simultaneous increase in firm creations and the smaller increase in firm deaths suggest that the effect of competing with central firms is weaker the further municipalities are from cities.

We use the distance measure from Fretz et al. (2021). They measure distance using the road network as of 2012. However, the 2012 network might be affected by the highway construction. Therefore, we run the same analysis using Euclidean distance to the nearest urban center as an alternative measure. This distance measure omits important geographical features but is time-invariant and unaffected by highway construction. Table 1.C.10 shows comparable patterns.

Size of firms and distribution by size

To better understand how the aggregate increase in firm numbers arises, we examine how the effects of highway access vary across the firm size distribution. We use the number of directors on the board of directors and the nominal capital as proxies for firm size[24]. Our results, in Figures 1.5 and 1.6, seem to be driven by relatively smaller firms.

When we observe the effects across nominal capital percentiles, as measured in the national capital size distribution, we find effects among smaller firms. Figure 1.5 shows large increases in the number of firms in the bottom 50th and 50-75th percentiles. We do not find any effects in the different groups within the top 25% of the nominal capital distribution. When we use board size as a proxy, we also find that the increase in the number of firms is driven by small firms. Our results suggest positive long-term effects for the firm with one, two, and three board members. Highway access does not affect the number of firms with larger boards. However, the

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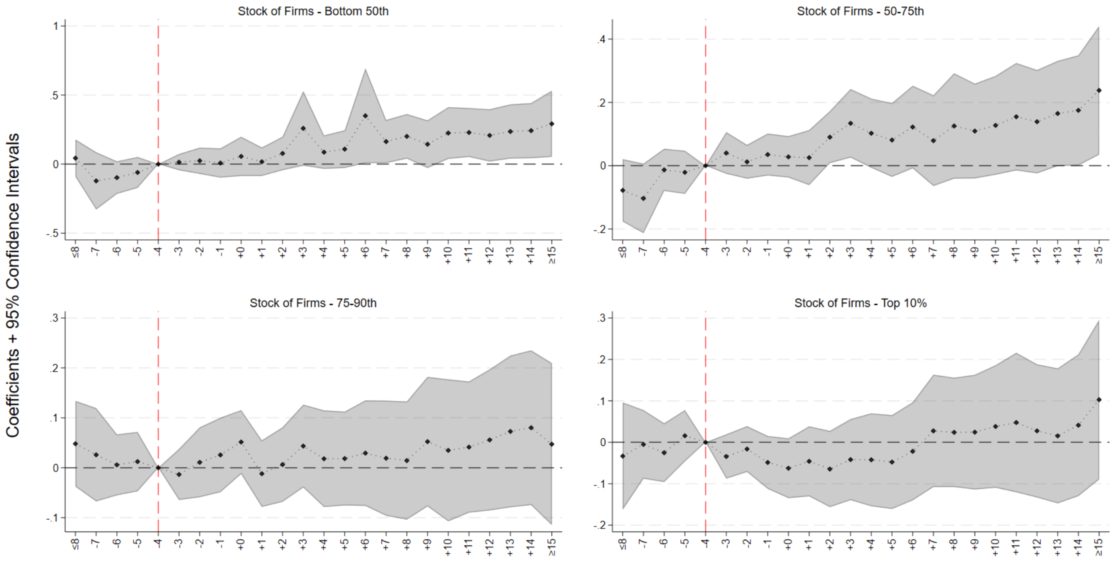
[24] Azar (2021) observes a positive relationship between firm size, measured by market capitalization, and board size.

results are far less conclusive given the unstable dynamic effects we measure.

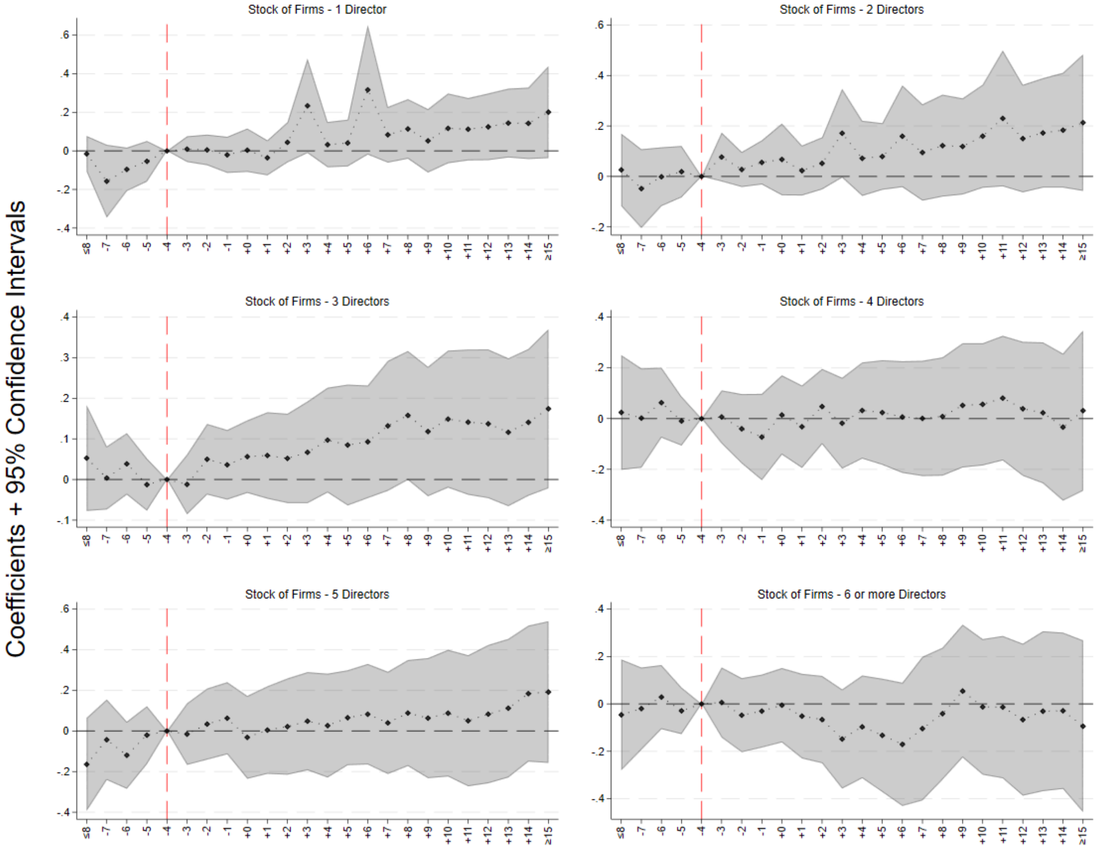
The effects are concentrated in the lower and middle parts of the firm size distribution, while the upper tail remains largely unaffected. The observed pattern in our baseline results is primarily due to an increase in the entry of smaller firms into local markets. Combined with the flow analysis, the firm size results indicate that highway access increases turnover among smaller firms. These findings imply that highway investments reshape local economies not only by increasing the number of firms, but also by altering the composition of the economy. This has direct implications for how infrastructure investments interact with entrepreneurship and local competition in rural areas.

Figure 1.5: Cumulative effect before/after access (<15 km) in the bottom 50th, 50th-75th, 75th-90th, and 90th percentile of national nominal capital size distribution



Notes: The figure presents our full specification results, which include year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.11 in the Appendix provides all the details.

Figure 1.6: Cumulative effect before/after access (<15 km) by board sizes



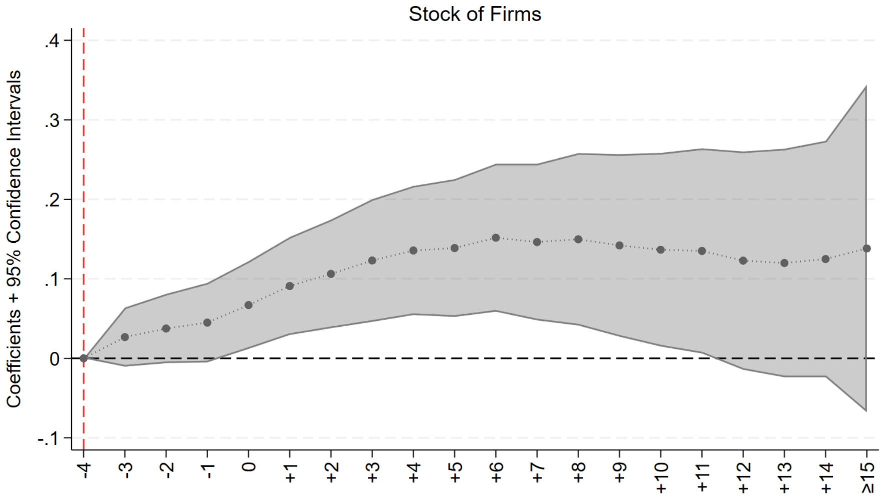
Notes: The figure presents our full specification results, which include year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.12 in the Appendix provides all the details.

1.7 Robustness checks

1.7.1 Heterogeneous difference-in-differences (Wooldridge 2021, 2023)

We begin our robustness checks by implementing the ETWFE proposed by Wooldridge (2021, 2023). Concretely, we allow treatment effects to differ across cohorts and over time. Given the data requirements, we are only able to replicate the TWFE regressions that include municipalities located between 15 km and 30 km away from highway accesses[25]. Therefore, we need to compare the results with those obtained in Table 1.C.8, column (1). The results for the number of firms are of comparable magnitude, as shown in Figure 1.7. This suggests that heterogeneity does not play a major role in our main results using standard TWFE designs.

Figure 1.7: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 30 km) - ETWFE



Notes: The figure presents our ETWFE results, which include year and unit fixed effects. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.9 in the Appendix provides all the details.

When using our flow variables, the ETWFE regression estimates a positive effect on firm creation in the years following highway construction. The increase lasts for 6 years after the road access is opened. For firm deaths, variance estimation failed to converge under ETWFE[26]. Columns (4) and (5) are noisy as usual, but it seems that some relocations outside of the treated municipalities occur when allowing for

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[25] As previously mentioned, we have 43 periods and 26 different cohorts, and we need to estimate one coefficient for every combination of periods and cohort (every different $\theta_{g,t}$).

[26] Point estimates are reported for completeness, but no inference should be drawn from them.

heterogeneity across time and cohorts. We cannot draw any conclusions from these results, given that we observe differential pre-trends in our baseline estimates and that Wooldridge (2023) do not compute pretrend coefficients.

Allowing heterogeneous treatment effects across cohorts does not affect post-treatment estimates. Given the unchanged estimates and the computational demands of the approach, we rely on the standard staggered TWFE for the remainder of the paper.

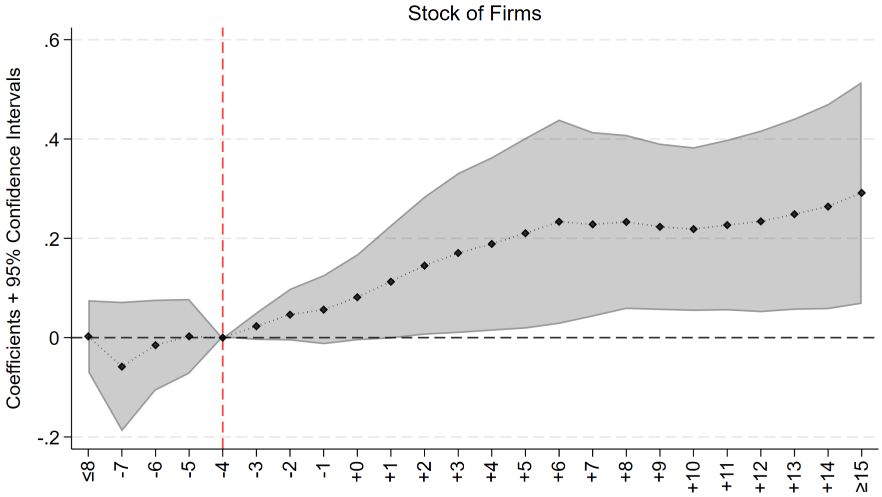
1.7.2 Inclusion of non-connected municipalities

In Figure 1.8, we also include municipalities with a distance ranging from 15 km to 30 km from a highway access in the control group. This means that, instead of having only treated municipalities in our sample, we also include municipalities that plausibly never received treatment. As our estimates in Table 1.1 suggest, the effects are concentrated within 15 km of the new access. The effects are essentially zero beyond.

Consistent with this spatial decay pattern, municipalities farther from highway access show slightly larger estimated effects. Relative to the baseline specification, the long-run effect rises from 0.265 to 0.305 when we add non-connected municipalities to the control group. The number of firms accumulates from period -3 up to period 6. The trend flattens between periods 8 and 12, before increasing again. This modest increase relative to the baseline is expected: municipalities beyond 15 km provide a cleaner counterfactual than “not-yet-treated” municipalities, which may be contaminated by the treatment.

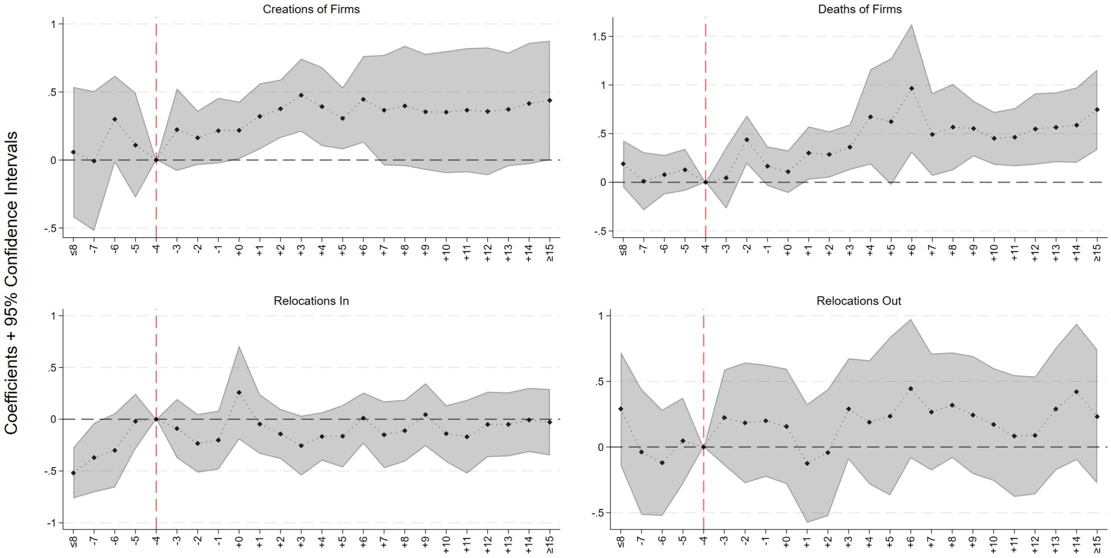
This interpretation is reinforced by Tables 1.C.6 and 1.C.7. Adding only non-connected municipalities located 15 to 20 km from an access to the control group almost does not change the coefficients relative to the main results. The long-term effects increase from 0.265 to 0.279. The increase is also marginal when we add municipalities 15 to 25 km away from highways, where we find a long-term effect of 0.287 (see Table 1.C.7). Regarding the "flow" variables, we observe more births and deaths when we add never-treated municipalities to the control group. However, we still do not find any effects for relocations. The coefficients are also comparable to those in the baseline. Once again, results on relocations are inconclusive.

Figure 1.8: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 30 km)



Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access, as well as municipalities located between 15 and 30 km of an access that have never been treated. Table 1.C.8 in the Appendix provides all the details.

Figure 1.9: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 30 km) - Flow variables



Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access, as well as municipalities located between 15 and 30 km of an access that have never been treated. Table 1.C.8 in the Appendix provides all the details.

1.7.3 Spillovers - Exclusion of "close controls"

A key concern in evaluating the local effects of infrastructure is whether observed changes in economic outcomes reflect net growth or reallocation of firms across space. In our context, such reallocation could occur if municipalities without highway access, but located near those with access, experience negative spillovers[27]. If this is the case, the observed positive effects of highways could partially reflect displacement rather than pure growth. Moreover, this would violate the no-spillover condition, which is required for our empirical identification.

To investigate this possibility, we re-estimate our main analysis after excluding from the control group municipalities that are geographically close to treated units but are not yet treated themselves (Debarsy and Le Gallo 2025). Specifically, we exclude municipalities within 5 km of any treated municipality, but keep those farther away. We expand the set of excluded municipalities to those within 10km in a more restrictive specification. We refer to those excluded municipalities as "*close controls*".

However, the estimated effects are remarkably stable, as shown in Figure 1.10. Excluding not-yet-treated “close controls” increases the cumulative effect on the number of firms only slightly after 15 periods, from 0.246 to 0.254. The long-run effects remain essentially unchanged. This pattern is inconsistent with negative spillovers on nearby untreated municipalities: if close controls were adversely affected, including them would increase the treatment effects. Excluding them would then tend to reduce estimated effects. On the contrary, the small increase we observe is consistent with the notion that nearby controls may be mildly exposed to the treatment (positive spillovers), attenuating baseline estimates. Therefore, dropping them slightly raises the treatment effects.

Similarly, as shown in Figure 1.11, the results for the flow variables remain comparable, although we observe a decrease in long-term births and deaths when dropping close controls. We also run the same analysis, including non-connected municipalities, but excluding close controls. Once again, we find small variations compared to results with close controls included, as shown in Tables 1.C.14 and 1.C.16 in the Appendix, suggesting no spillover from neighboring untreated municipalities.

Taken together, these exercises suggest that interference through spatial spillovers to nearby untreated municipalities is limited in our setting, and that our baseline estimates are not driven by negative spillovers onto neighboring municipalities.

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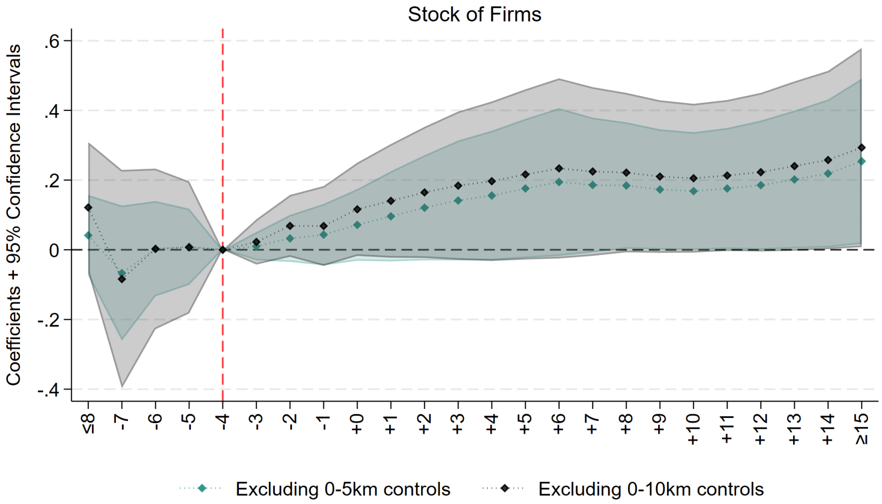
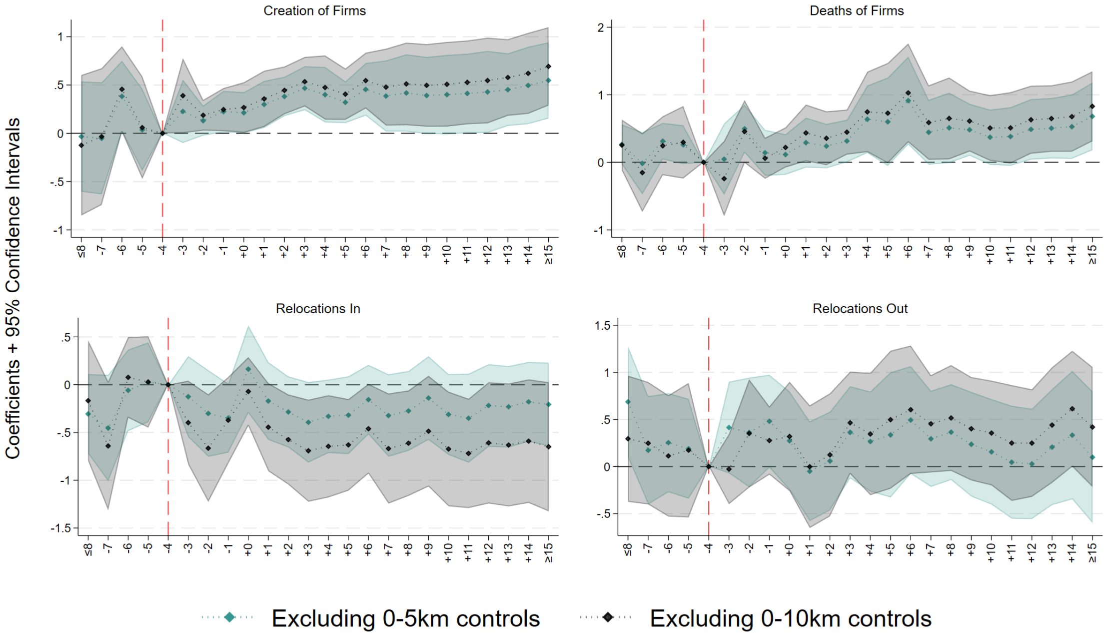
[27] They could also experience positive spillovers, meaning that part of the control group is partially treated and bias our main estimates towards zero. Dropping such control units increases treatment effects, but this could come at the price of comparability between treated and controls.

Figure 1.10: Cumulative effect before/after access (<15 km), excl. close controls (<5 & <10km)



Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access, but excludes not-yet-treated controls located within 5 km or 10km of any already treated municipalities. Table 1.C.13 and 1.C.15 in the Appendix provide all the details.

Figure 1.11: Cumulative effect before/after access (<15 km), excl. close controls (<5 & <10km) - Flow variables



Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access, but excludes not-yet-treated controls located within 5 km or 10km of any already treated municipalities. Table 1.C.13 and 1.C.15 in the Appendix provide all the details.

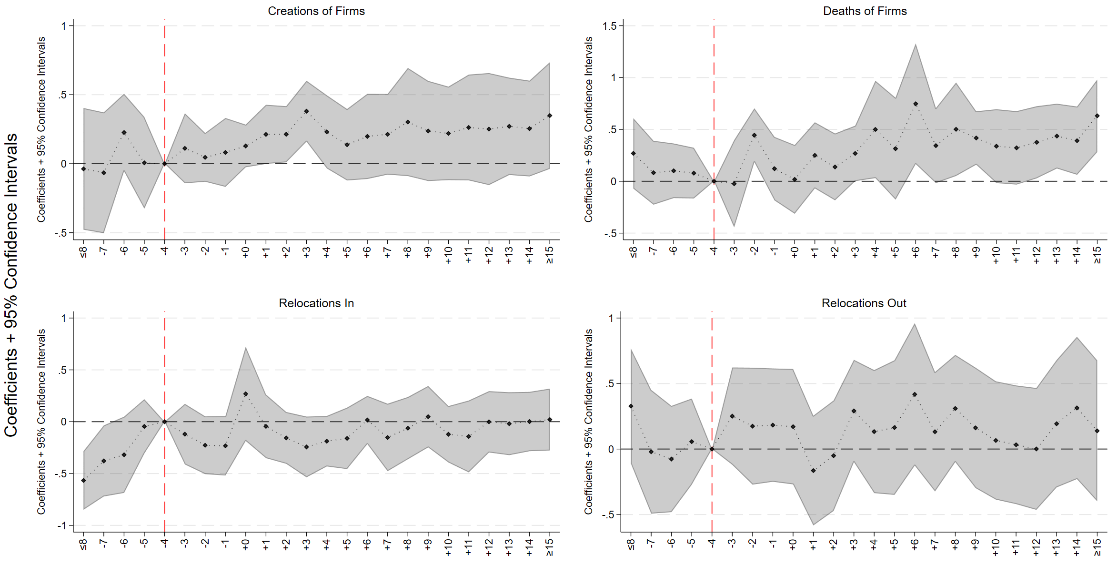
1.7.4 Estimation without single-year firms

Given that we construct the panel data using fuzzy matching across cross-sections and digitized firm names, we may have missed some matches. In reality, most newly created firms survive for more than one year. Thus, we want to rule out the possibility that these unmatched cases drive our results. To do so, we run the same regressions on a subsample excluding firms that exist only for 1 year. The results in Figure 1.12 and 1.13 are comparable to the baseline results. We find slightly smaller coefficients and long-term effects, but the patterns remain similar, suggesting the single-year firms do not drive the results.

Figure 1.12: Cumulative effect before/after access (<15 km) - w/o one-year firms

Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access, but excludes firms that appear in a single year. Table 1.C.17 in the Appendix provides all the details.

Figure 1.13: Cumulative effect before/after access (<15 km) - w/o one-year firms - Flow variables



Notes: The figure presents our full specification results estimating Equation (1.1). This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access, but excludes firms that appear in a single year. Table 1.C.17 in the Appendix provides all the details.

1.7.5 Net births and deaths estimation

As a further robustness check, we create "*net births*" and "*net deaths*" variables. We calculate the "*net births*" using the current number of created firms, from which we subtract the deaths from the previous period. For the "*net deaths*", we subtract the creations in the next period from the deaths in the current period. This is a way to address potential inflation in the number of creations when false negatives remain in the panel. If we miss some correct matches, we would measure additional deaths and births. We attempt to address this issue using these net outcome variables[28].

For the "*net birth*", the overall trend is very similar. The coefficients are larger, but also less precise. Potential false negatives in our matching procedure do not seem to affect our results on firm creation. Similarly, the "*net deaths*" estimates follow the same trend and differ only marginally from the estimations using the true death variable. Note that, using our "*net births*" and "*net deaths*" variables, we might have

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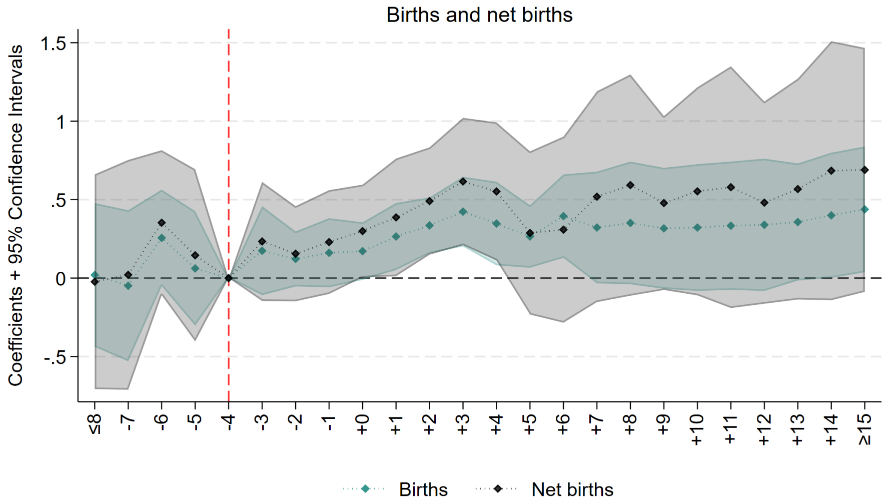
[28] Net births and net deaths can take negative values, which are incompatible with PQML estimation. We recode negative values to zero. Net births rarely take negative values, given the predominantly growing population of firms over the period. Only 10% of observations were recoded to zero, making this a minor concern. For net deaths, the growth trend implies more frequent negative values (around 25%), so these results should be interpreted with caution and are reported for completeness only.

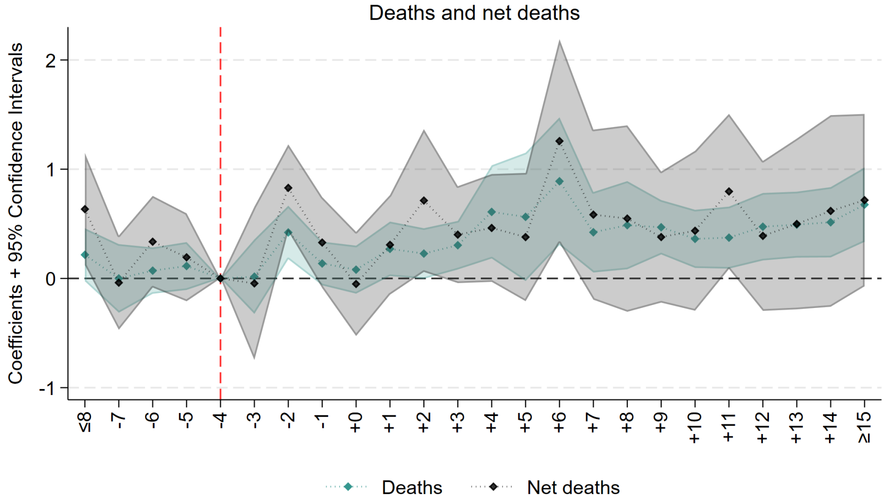
corrected for successive deaths and births that are perfectly right.

Figure 1.14: Cumulative effect before/after access (<15 km) - Net birth



Notes: The figure presents our full specification results estimating Equation (1.1). We plot both the results using the creation of firms and "*net birth*". This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.18 in the Appendix provides all the details.

Figure 1.15: Cumulative effect before/after access (<15 km) - Net death



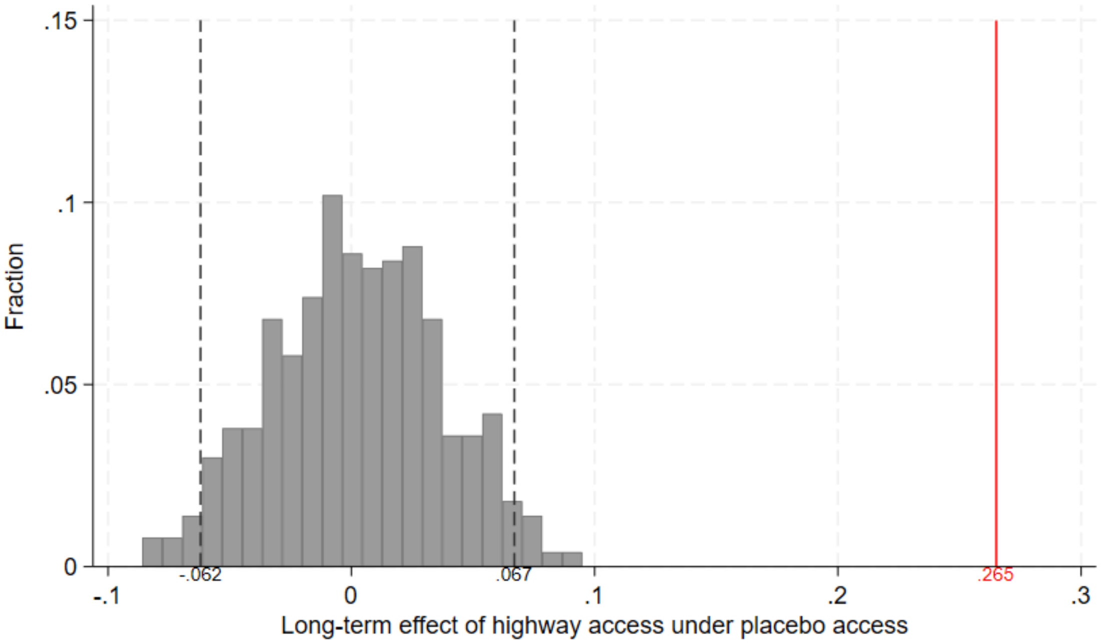
Notes: The figure presents our full specification results estimating Equation (1.1). We plot both the results using the death of firms and "*net death*". This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.18 in the Appendix provides all the details.

1.7.6 Placebos - Randomization of treatment timing

As a final robustness check, we conduct placebo tests in which we randomize the treatment date for municipalities within 15 km of an access. Regarding the number of firms, we use the full baseline specification and randomize treatment timing 1000 times. The dependent variable is the number of firms at the municipal level. The vertical red line shows the long-term coefficient from Table 1.C.4 specification (2). As Figure 1.16 shows, the placebo coefficients are much smaller compared to our estimated coefficient.

We proceed similarly with the flow variables for which we find an effect in the main analysis: firm births and deaths. We randomize the treatment date 500 times in this case. The resulting figures are in Appendix 1.C.3. They show that the long-term effect of the real treatment is substantially larger than the effects of the placebos. The placebo tests suggest that our results are not due to chance.

Figure 1.16: The long-term effect on the number of firms - Placebo tests



Notes: Resulting histogram from our placebo tests where we randomize the treatment 1000 times. We use the same regressions as in our main analysis, focusing on the long-term effects. The dotted vertical lines show the 95% confidence interval. The vertical axis shows the share of long-term effects falling into a certain range. The red line is the long-term effect on the number of firms from Table 1.C.4, specification (2).

1.8 Conclusion

This chapter studies how the staggered expansion of Switzerland's national highway network affected firm dynamics in non-urban municipalities between 1960 and 2003. Using newly digitized administrative records covering the universe of Swiss corporations, we construct a long municipal panel that tracks the number of firms, as well as firm entries, exits, and relocations over more than four decades. Exploiting variation in the timing of highway access openings, we estimate the dynamic and long-term effects of highway construction on firms.

We document a persistent increase in the number of firms in municipalities gaining highway access. These effects accumulate gradually after opening and persist in the long run. Importantly, the increase in the number of firms is driven by higher firm creation, which exceeds the simultaneous rise in firm closures. In contrast, we find no systematic evidence of firm relocations into or out of treated municipalities. Taken together, these results indicate that highway access generates net local firm growth that is not primarily driven by the relocation of existing firms. The results contribute to the long-standing "growth versus reorganization" question by showing that, in the Swiss context, highways increase economic activity.

By decomposing firm dynamics, we uncover important adjustment mechanisms. Highway access increases both firm entry and exit, pointing to intensified competition and selection. These responses vary systematically with proximity to urban centers. Municipalities closer to cities primarily experience higher firm exits, consistent with urban shadow effects, while more remote municipalities exhibit increases in both births and deaths. The analysis further shows that firm responses differ by size. Smaller firms experience higher turnover following highway access, while larger firms are less affected, suggesting that improved accessibility reshapes the composition of local firms through entries. Taken together, these findings indicate that highways do not simply increase the number of firms everywhere, but instead reorganize local economies along dimensions of distance to cities, urban hierarchy, and firm size.

A range of robustness checks reinforces these findings. First, excluding nearby untreated municipalities from the control group leaves the results essentially unchanged, suggesting that spatial spillovers do not drive the baseline estimates. The slight increase in treatment effects is consistent with mild contamination of the control group rather than negative spillovers. Second, allowing for heterogeneous cohort effects yields very similar post-treatment dynamics. Third, randomizing treatment timing substantially reduces the estimated long-run effects, indicating that the results are unlikely to be due to chance. Finally, alternative sample restrictions designed to address potential matching issues leave the main conclusions unaffected.

This chapter contributes to the literature in several ways. First, it provides causal evidence on the impact of transport infrastructure on firm dynamics using detailed firm-level data, allowing us to directly observe firm creations, closures, and relocations across Switzerland. Second, it documents sharp spatial decay in infrastructure effects and shows how improved connectivity interacts with urban proximity to shape local economic outcomes. Third, as relocation results are inconclusive, the evidence points to highway access primarily stimulating new firm entries rather than reallocating existing activity. Even in a country with extensive existing rail and road networks, reductions in transportation costs can lead to large and durable changes in firm dynamics in newly connected areas. While our focus is on Switzerland, the findings have broader implications for other countries considering infrastructure expansion or improvement.

Appendices

1.A Documents and data preparation

1.A.1 Raw data and panel construction

We digitize all available editions of the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften / Répertoire des administrateurs des sociétés anonymes suisses*" published between 1934 and 2003. These directories are organized around individual directors and are based on information from the cantonal commercial registries. For each year, they report the universe of corporations, together with information on directors, firm names, locations, and nominal capital (see Figure 1.A.1 for an example). We digitized these volumes and, in collaboration with Sugarcube Sàrl, converted the scanned material into a database. The resulting raw data contain approximately 4.6 million individual-level observations and 4.2 million firm-level entries, covering repeated cross-sections for the years 1934, 1943, 1960, 1962–1966, 1969, 1972, 1975, and 1979–2003. From these data, we compute firm counts at the municipal level and characterize local firm structure using size indicators such as nominal capital or board size.

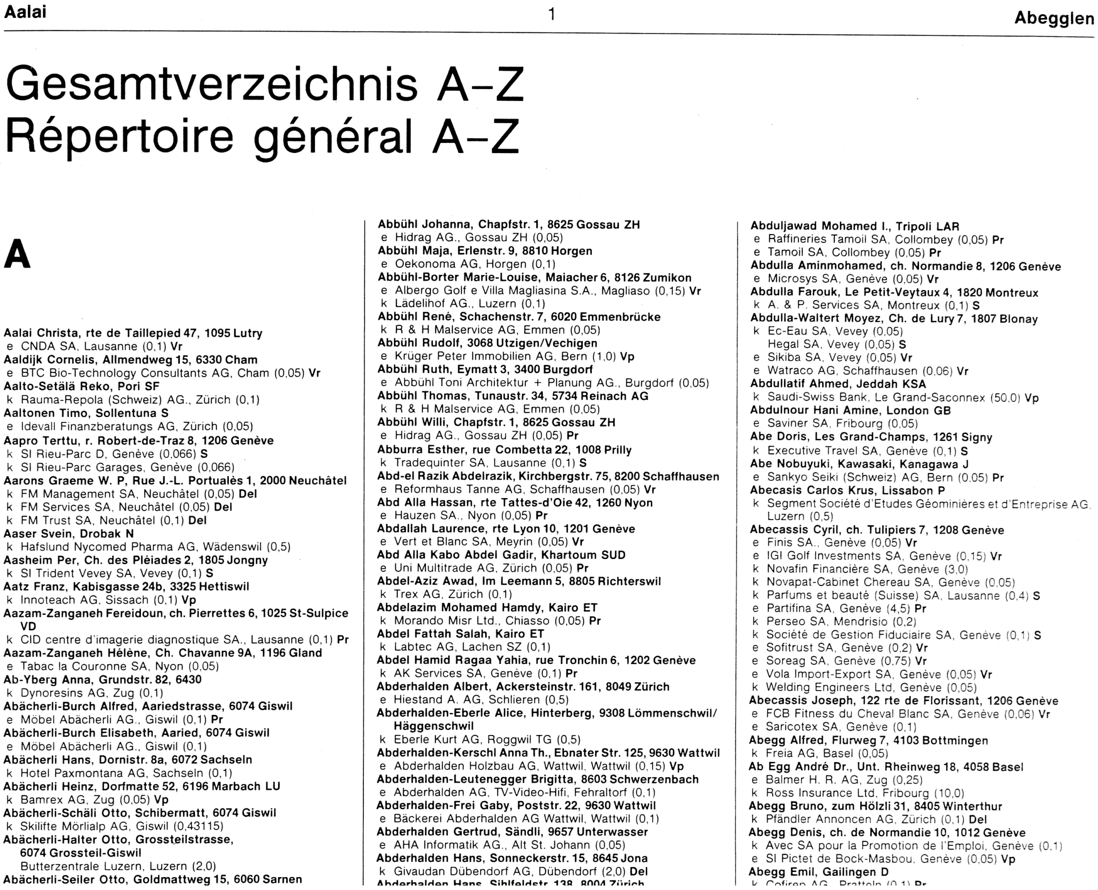
Because the directories are centered on individuals, firm information is not unique within a given year, as the same firm appears multiple times when several directors sit on its board. Constructing a firm-level panel, therefore, requires both deduplication within cross-sections and linkage of firms across years. We address this by implementing a multi-step matching procedure that progressively identifies unique firms within each year and then tracks them over time.

The matching strategy combines exact and approximate comparisons of firm names using a sequence of increasingly flexible rules. We begin with conservative string-distance matching approaches that rely on standardized firm names and strict similarity thresholds, allowing us to identify reliable matches while limiting false positives. These procedures are first applied within municipalities and across adjacent periods to establish a "high-confidence" set of firm links. Once this set is constructed and the number of remaining unmatched observations is reduced, we extend the matching using less restrictive string-distance thresholds.

In a final stage, we apply a token-based matching algorithm that is more flexible with respect to word order and naming conventions. This step is particularly important for linking firms across periods because naming practices change over time (e.g., different abbreviations, word orders, etc.). Matching at each stage is evaluated manually by junior research assistants. Geographical restrictions are gradually relaxed in order to allow for firm relocations across municipalities.

This sequential approach allows us to reduce the set of unmatched observations while maintaining a high level of confidence in firm linkages. The resulting firm-level panel contains approximately 430,000 distinct corporations, both publicly listed and private, observed over time. We aggregate this panel to the municipal level using harmonized municipal boundaries as of 2012. This choice ensures consistency with the municipal definitions Fretz et al. (2021) use, and allows us to combine our firm outcomes with their highway access measures.

Figure 1.A.1: Example : "*Répertoire des administrateurs des sociétés anonymes suisses*"



Aalai 1 **Abegglen**

Gesamtverzeichnis A–Z
Répertoire général A–Z

A

Aalai Christa, rte de Taillepied 47, 1095 Lutry
e CNDA SA, Lausanne (0,1) **Vr**
Aaldijk Cornelis, Allmendweg 15, 6330 Cham
e BTC Bio-Technology Consultants AG, Cham (0,05) **Vr**
Aalto-Setälä Reko, Pori SF
k Rauma-Repola (Schweiz) AG., Zürich (0,1)
Aaltonen Timo, Sollentuna S
e Idevall Finanzberatungs AG, Zürich (0,05)
Aapro Terttu, r. Robert-de-Traz 8, 1206 Genève
k SI Rieu-Parc D, Genève (0,066) **S**
k SI Rieu-Parc Garages, Genève (0,066)
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k FM Management SA, Neuchâtel (0,05) **Del**
k FM Services SA, Neuchâtel (0,05) **Del**
k FM Trust SA, Neuchâtel (0,1) **Del**
Aaser Svein, Drobak N
k Hafslund Nycomed Pharma AG, Wädenswil (0,5)
Aasheim Per, Ch. des Pléiades 2, 1805 Jongny
k SI Trident Vevey SA, Vevey (0,1) **S**
Aatz Franz, Kabisgasse 24b, 3325 Hettiswil
k Innoteach AG, Sissach (0,1) **Vp**
Aazam-Zanganeh Fereidoun, ch. Pierrettes 6, 1025 St-Sulpice VD
k CID centre d'imagerie diagnostique SA., Lausanne (0,1) **Pr**
Aazam-Zanganeh Hélène, Ch. Chavanne 9A, 1196 Gland
e Tabac la Couronne SA, Nyon (0,05)
Ab-Yberg Anna, Grundstr. 82, 6430
k Dynoresins AG, Zug (0,1)
Abächerli-Burch Alfred, Aariedstrasse, 6074 Giswil
e Möbel Abächerli AG., Giswil (0,1) **Pr**
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e Möbel Abächerli AG., Giswil (0,1)
Abächerli Hans, Dornistr. 8a, 6072 Sachseln
k Hotel Paxmontana AG, Sachseln (0,1)
Abächerli Heinz, Dorfmatte 52, 6196 Marbach LU
k Bamrex AG, Zug (0,05) **Vp**
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k Skilifte Mörlialp AG, Giswil (0,43115)
Abächerli-Halter Otto, Grossteilstrasse, 6074 Grossteil-Giswil
Butterzentrale Luzern, Luzern (2,0)
Abächerli-Seiler Otto, Goldmattweg 15, 6060 Sarnen
Abbühl Johanna, Chapfstr. 1, 8625 Gossau ZH
e Hidrag AG., Gossau ZH (0,05)
Abbühl Maja, Erlenstr. 9, 8810 Horgen
e Oekonoma AG, Horgen (0,1)
Abbühl-Borter Marie-Louise, Maiacher 6, 8126 Zumikon
e Albergo Golf e Villa Magliasina S.A., Magliaso (0,15) **Vr**
k Lädelihof AG., Luzern (0,1)
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k R & H Malservice AG, Emmen (0,05)
Abbühl Rudolf, 3068 Utzigen/Vechigen
e Krüger Peter Immobilien AG, Bern (1,0) **Vp**
Abbühl Ruth, Eymatt 3, 3400 Burgdorf
e Abbühl Toni Architektur + Planung AG., Burgdorf (0,05)
Abbühl Thomas, Tunaustr. 34, 5734 Reinach AG
k R & H Malservice AG, Emmen (0,05)
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Abburra Esther, rue Combetta 22, 1008 Prilly
k Tradequinter SA, Lausanne (0,1) **S**
Abd-el Razik Abdelrazik, Kirchbergstr. 75, 8200 Schaffhausen
e Reformhaus Tanne AG, Schaffhausen (0,05) **Vr**
Abd Alla Hassan, rte Tattes-d'Oie 42, 1260 Nyon
e Hauzen SA., Nyon (0,05) **Pr**
Abdallah Laurence, rte Lyon 10, 1201 Genève
e Vert et Blanc SA, Meyrin (0,05) **Vr**
Abd Alla Kabo Abdel Gadir, Khartoum SUD
e Uni Multitrade AG, Zürich (0,05) **Pr**
Abdel-Aziz Awad, Im Leemann 5, 8805 Richterswil
k Trex AG, Zürich (0,1)
Abdelazim Mohamed Hamdy, Kairo ET
k Morando Misr Ltd., Chiasso (0,05) **Pr**
Abdel Fattah Salah, Kairo ET
k Labtec AG, Lachen SZ (0,1)
Abdel Hamid Ragaa Yahia, rue Tronchin 6, 1202 Genève
k AK Services SA, Genève (0,1) **Pr**
Abderhalden Albert, Ackersteinstr. 161, 8049 Zürich
e Hiestand A. AG, Schlieren (0,5)
Abderhalden-Eberle Alice, Hinterberg, 9308 Lömmenschwil/ Häggenschwil
k Eberle Kurt AG, Roggwil TG (0,5)
Abderhalden-Kerschl Anna Th., Ebnater Str. 125, 9630 Wattwil
e Abderhalden Holzbau AG, Wattwil, Wattwil (0,15) **Vp**
Abderhalden-Leutenegger Brigitta, 8603 Schwerzenbach
e Abderhalden AG, TV-Video-Hifi, Fehraltorf (0,1)
Abderhalden-Frei Gaby, Poststr. 22, 9630 Wattwil
e Bäckerei Abderhalden AG Wattwil, Wattwil (0,1)
Abderhalden Gertrud, Sändli, 9657 Unterwasser
e AHA Informatik AG., Alt St. Johann (0,05)
Abderhalden Hans, Sonneckerstr. 15, 8645 Jona
k Givaudan Dübendorf AG, Dübendorf (2,0) **Del**
Abderhalden Hans, Sihlfeldstr. 138, 8004 Zürich
Abduljawad Mohamed I., Tripoli LAR
e Raffineries Tamoil SA, Collombey (0,05) **Pr**
e Tamoil SA, Collombey (0,05) **Pr**
Abdulla Aminmohamed, ch. Normandie 8, 1206 Genève
e Microsys SA, Genève (0,05) **Vr**
Abdulla Farouk, Le Petit-Veytaux 4, 1820 Montreux
k A. & P. Services SA, Montreux (0,1) **S**
Abdulla-Waltert Moyez, Ch. de Lury 7, 1807 Blonay
k Ec-Eau SA, Vevey (0,05)
Hegal SA, Vevey (0,05) **S**
e Sikiba SA, Vevey (0,05) **Vr**
e Watraco AG, Schaffhausen (0,06) **Vr**
Abdullatif Ahmed, Jeddah KSA
k Saudi-Swiss Bank, Le Grand-Saconnex (50,0) **Vp**
Abdulnour Hani Amine, London GB
e Saviner SA, Fribourg (0,05)
Abe Doris, Les Grand-Champs, 1261 Signy
k Executive Travel SA, Genève (0,1) **S**
Abe Nobuyuki, Kawasaki, Kanagawa J
e Sankyo Seiki (Schweiz) AG, Bern (0,05) **Pr**
Abecasis Carlos Krus, Lissabon P
k Segment Société d'Etudes Géominières et d'Entreprise AG, Luzern (0,5)
Abecassis Cyril, ch. Tulipiers 7, 1208 Genève
e Finis SA., Genève (0,05) **Vr**
e IGI Golf Investments SA, Genève (0,15) **Vr**
k Novafin Financière SA, Genève (3,0)
k Novapat-Cabinet Chereau SA, Genève (0,05)
k Parfums et beauté (Suisse) SA, Lausanne (0,4) **S**
e Partifina SA, Genève (4,5) **Pr**
k Perseo SA, Mendrisio (0,2)
k Société de Gestion Fiduciaire SA, Genève (0,1) **S**
e Sofitrust SA, Genève (0,2) **Vr**
e Soreag SA, Genève (0,75) **Vr**
e Vola Import-Export SA, Genève (0,05) **Vr**
k Welding Engineers Ltd, Genève (0,05)
Abecassis Joseph, 122 rte de Florissant, 1206 Genève
e FCB Fitness du Cheval Blanc SA, Genève (0,06) **Vr**
e Saricotex SA, Genève (0,1)
Abegg Alfred, Flurweg 7, 4103 Bottmingen
k Freia AG, Basel (0,05)
Ab Egg André Dr., Unt. Rheinweg 18, 4058 Basel
e Balmer H. R. AG, Zug (0,25)
k Ross Insurance Ltd, Fribourg (10,0)
Abegg Bruno, zum Hölzli 31, 8405 Winterthur
k Pfändler Annoncen AG, Zürich (0,1) **Del**
Abegg Denis, ch. de Normandie 10, 1012 Genève
k Avec SA pour la Promotion de l'Emploi, Genève (0,1)
e SI Pictet de Bock-Masbou, Genève (0,05) **Vp**
Abegg Emil, Gailingen D
k Cofiren AG., Pratteln (0,1) **Pr**

Notes: Example of a page in the publications showing the raw data before digitalization.

1.A.2 Dealing with missing years in the panel

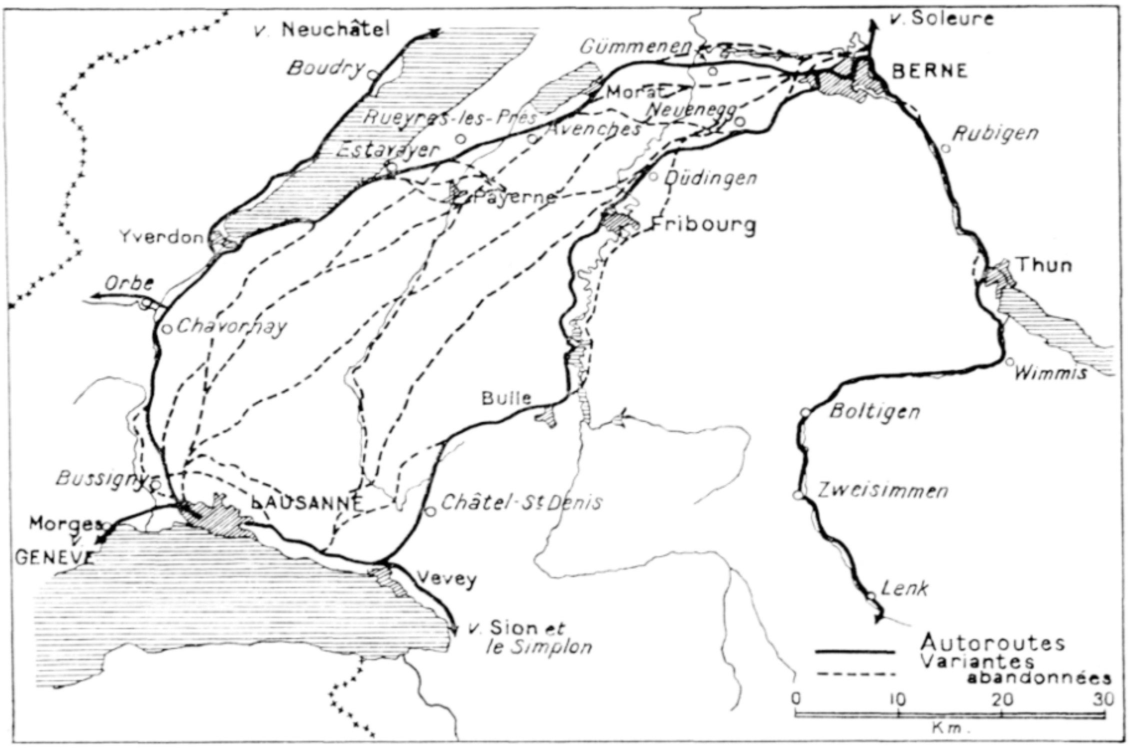
As mentioned several times, we have missing data in some years prior to the 1980s. Regarding the stock, we use a linear approximation because the number of consecutive missing years does not exceed 3. The number of firms is stable and, in almost all cases, steadily increases over time. Flow variables are more challenging because their current values are much less dependent on previous ones. If we observe no firm creations in the period immediately preceding a gap in the data and 10 newly created firms in the period following it, we have no information on when those 10 firms were created during the gap.

However, the number of created firms generally increases over time. We use this fact to determine a rule for attributing firm creations over missing years. As an example, we can use a gap with 10 newly created firms. We observe 10 firms created in period t after a three-year gap (the most frequently observed gap in our data). We start by dividing those 10 creations by 2. We then attribute 5 to period t, and 5 to period t-1. We repeat the same procedure and divide the 5 creations in period t-1 by 2, then attribute each half to periods t-1 and t-2. As we have rational numbers, we round up the number in t-1 and down the one in t-2. We continue with the same approach until the gaps are filled. By doing so, we preserve the exact number of created firms observed and attribute more creations to later periods. Note that firms created and closed during a gap are never observed in our data. The number of creations and deaths could be higher without gaps. The same procedure is applied to fill gaps in all our "*flow variables*".

1.A.3 Example of planning: Lausanne - Bern segment

One possible method for causally estimating the effects of highways would have been to use original plans as an instrument for later constructions, as in Duranton and Turner (2012) or Mahajan (2024), for example. We quickly realized that such plans did not exist consistently in the Swiss context. For example, we did find a map (see Figure 1.A.2) showing all the various routes considered for connecting Bern and Lausanne (Piveteau 1964). We were unable to determine when the final decision was made, and we did not find systematic or more granular information on the planned routes.

Figure 1.A.2: Example: All different options considered to connect Bern and Lausanne



Notes: The map shows a historical planning document showing the range of route options considered for the Bern–Lausanne highway corridor. Source: Piveteau (1964).

1.B Descriptive statistics

1.B.1 Summary statistics

Table 1.B.1 presents the summary statistics of the variables we use in this chapter. We present the means and standard deviations (in parentheses) for the entire sample across all years. We split the summary statistics by municipality type.

1.B.2 Stylized facts

We report the evolution of our main outcome variables by municipality type in Table 1.B.2. This table compares the average fraction of firm and worker variables in non-urban connected, non-urban non-connected, and urban municipalities at the beginning of our sample and after the highway construction.

Table 1.B.1: Summary Statistics

	All municipalities (1)	Non-urban connected municipalities (2)	Non-urban non-connected municipalities (3)	Urban centers (4)	Suburban municipalities (5)
Firms (SA/AG)	43.51	11.67	8.70	1965.03	40.07
	(366.39)	(56.03)	(19.64)	(2770.52)	(86.66)
Firms' birth	3.50	0.88	0.63	158.39	3.33
	(35.37)	(4.60)	(1.70)	(287.31)	(7.98)
Firms' death	2.15	0.50	0.33	103.40	1.93
	(22.86)	(3.45)	(1.13)	(184.17)	(5.91)
Firms relocating in	0.79	0.19	0.13	20.70	1.22
	(4.83)	(0.86)	(0.62)	(33.84)	(3.73)
Firms relocating out	0.77	0.17	0.11	29.37	0.95
	(6.85)	(1.03)	(0.60)	(53.82)	(3.20)
Total nominal capital	484.34	5.99	8.38	44059.43	41.30
	(1.0e+05)	(55.25)	(563.14)	(1.0e+06)	(1975.67)
Share of Firms in 50th-25th percentile (capital)	0.27	0.27	0.30	0.24	0.25
	(0.26)	(0.29)	(0.29)	(0.10)	(0.21)
Share of Firms in 25th-10th percentile (capital)	0.14	0.13	0.15	0.12	0.13
	(0.19)	(0.21)	(0.21)	(0.04)	(0.15)
Share of Firms in 10th-1th percentile (capital)	0.08	0.08	0.09	0.09	0.08
	(0.14)	(0.15)	(0.16)	(0.03)	(0.12)
Share of Firms in top 1% (capital)	0.01	0.01	0.01	0.01	0.01
	(0.04)	(0.05)	(0.05)	(0.01)	(0.03)
Total number of directors	91.76	24.74	21.31	4191.28	93.09
	(739.28)	(95.11)	(45.83)	(5771.66)	(178.43)
Share 1 director	0.37	0.35	0.32	0.45	0.41
	(0.27)	(0.29)	(0.29)	(0.12)	(0.23)
Share 2 directors	0.28	0.29	0.29	0.23	0.26
	(0.24)	(0.27)	(0.27)	(0.06)	(0.19)
Share 3 directors	0.22	0.23	0.23	0.20	0.22
	(0.22)	(0.25)	(0.25)	(0.04)	(0.17)
Share 4 directors	0.06	0.06	0.07	0.06	0.06
	(0.13)	(0.14)	(0.16)	(0.02)	(0.10)
Share 5 directors	0.07	0.07	0.10	0.06	0.05
	(0.14)	(0.16)	(0.18)	(0.02)	(0.10)
Distance to closest urban center	21.36	22.19	28.99	0.00	15.15
	(13.80)	(11.75)	(15.43)	(0.00)	(10.23)
As the crow flies distance to urban center	15.14	15.67	20.07	23.27	10.49
	(9.50)	(8.11)	(10.28)	(12.37)	(7.40)
Railway station	0.39	0.33	0.35	1.00	0.46
	(0.49)	(0.47)	(0.48)	(0.00)	(0.50)
# Municipalities	2361	1082	387	27	865

Notes: Sources of the data are: Population data (Swiss Federal Statistical Office 2000a, Swiss Federal Office of Transport 2016). Type of municipalities defined by the Swiss Federal Statistical Office (2005). Distance to the closest urban center is computed using the road network as of 2012 from Fretz et al. (2021). Railway station dummy variable in 2017 (Swiss Federal Office of Transport 2017). All firms, nominal capital, and directors' data are digitalized from the "*Répertoire des administrateurs des sociétés anonymes suisses*" based on the Commercial Registry of Switzerland.

Table 1.B.2: Stylized Facts

	# of Firms	Creations	Deaths	Relocations In	Relocations Out	Employment Total	Employment in 2nd sector	Employment in 3rd sector
Non-urban connected municipalities (N=1082)								
Before (1960-1970)	0.08	0.10	0.07	0.09	0.07	0.12	0.14	0.10
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
After (1993-2003)	0.12	0.10	0.10	0.10	0.09	0.13	0.18	0.11
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Ratio After/Before	1.45	1.02	1.43	1.10	1.29	1.06	1.30	1.10
	(0.03)	(0.05)	(0.10)	(0.10)	(0.12)	(0.01)	(0.01)	(0.01)
Non-urban non-connected municipalities (N=387)								
Before (1960-1970)	0.03	0.03	0.02	0.02	0.02	0.05	0.06	0.04
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
After (1993-2003)	0.04	0.03	0.03	0.02	0.02	0.05	0.06	0.04
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ratio After/Before	1.32	1.09	1.26	1.09	1.22	0.84	0.99	0.91
	(0.01)	(0.07)	(0.13)	(0.19)	(0.21)	(0.01)	(0.01)	(0.01)
Urban centers (N=27)								
Before (1960-1970)	0.68	0.65	0.72	0.46	0.65	0.47	0.39	0.60
	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)
After (1993-2003)	0.43	0.45	0.49	0.28	0.37	0.39	0.25	0.45
	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)
Ratio After/Before	0.63	0.70	0.68	0.61	0.57	0.83	0.64	0.76
	(0.01)	(0.01)	(0.02)	(0.04)	(0.03)	(0.00)	(0.01)	(0.01)
Suburban municipalities (N=865)								
Before (1960-1970)	0.21	0.23	0.19	0.43	0.26	0.35	0.41	0.26
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
After (1993-2003)	0.41	0.41	0.39	0.59	0.51	0.44	0.51	0.40
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Ratio After/Before	1.96	1.83	1.99	1.38	1.96	1.23	1.24	1.52
	(0.04)	(0.04)	(0.06)	(0.04)	(0.11)	(0.01)	(0.01)	(0.03)
Urban municipalities - Urban centers & Suburban municipalities (N=892)								
Before (1960-1970)	0.89	0.87	0.91	0.89	0.91	0.82	0.80	0.86
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
After (1993-2003)	0.84	0.87	0.87	0.87	0.88	0.83	0.76	0.86
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Ratio After/Before	0.95	0.99	0.96	0.98	0.97	1.00	0.95	0.99
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)

Notes: "Before" covers the period 1960–1970; "After" covers 1993–2003. The ratio After/Before measures the relative change between the two windows. Standard errors of the mean are in parentheses. Municipality types (non-urban connected, non-urban non-connected, urban centres, suburban) are defined by the Swiss Federal Statistical Office (2005). Connected municipalities have an access point within 15 km of the municipal centroid.

Mean equality testing

Although we can visually assess the parallel trends assumption in our dynamic estimates, we compare variable values for the period 1960-1962 across groups, depending on the timing of the treatment. Since we use eventually treated municipalities as controls, we want to test whether early treated municipalities are comparable to later treated ones.

Fretz et al. (2021) run a similar exercise using their data, but they can use earlier pre-treatment periods for their mean equality testing. One can refer to their Table in Appendix 1.C.3 for the same test using their variables, for which they find "[...] *no statistical differences in the mean population, number of taxpayers, income composition of municipalities, workplace- and residence-based employment, nor, to a lesser extent in railway access between municipalities*". Here, we focus on the firms and employment variables, as they are not in Fretz et al. (2021). We do not have pretreatment characteristics exclusively, so we use variables from the 1960-1962 period, before the main openings. We test whether the values in Columns (2) to (5) are similar. We also find no differences using our firms' variables. We find slight differences in total employment, but they remain relatively small and should not have driven differences in the timing of the treatment on their own. Overall, municipalities with different treatment timing do not statistically differ from each other.

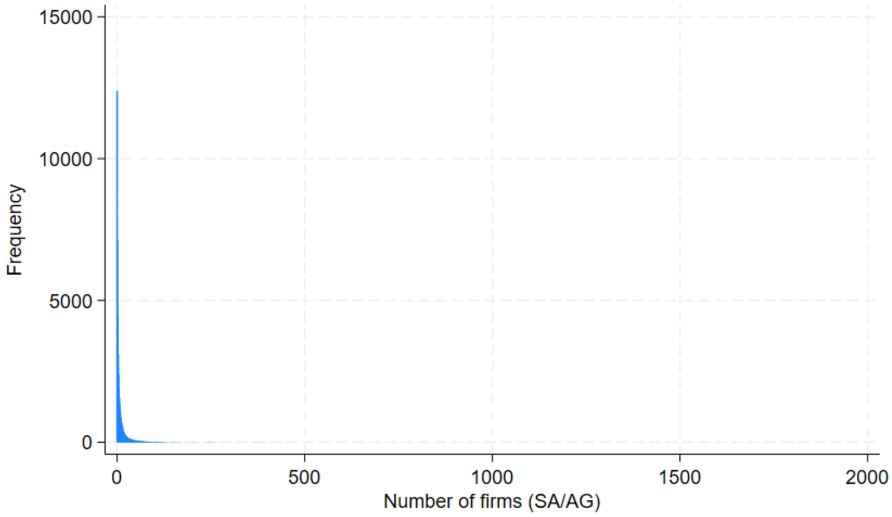
Table 1.B.3: Mean test equality between treatment periods

	Mean values for variables in period 1960-1962 for treated municipalities					
	All opening years (1)	Access 1963-69 (2)	Access 1970-79 (3)	Access 1980-89 (4)	Access after 1990 (5)	Test equality (p-value) (2) to (5)
# Firms (in 1,000)	2.45	3.48	1.73	2.13	1.60	0.47
	(16.92)	(27.24)	(4.51)	(4.55)	(5.29)	
# Firms (SA/AG) growth rate	9.34	8.41	9.35	11.69	9.34	0.82
	(30.05)	(28.37)	(27.98)	(38.89)	(26.09)	
Firms' birth	0.69	0.99	0.48	0.60	0.42	0.38
	(6.34)	(10.17)	(1.79)	(1.86)	(1.99)	
Firms' death	0.06	0.08	0.04	0.05	0.03	0.64
	(0.57)	(0.88)	(0.23)	(0.24)	(0.20)	
Firms relocating in	0.04	0.05	0.03	0.02	0.03	0.75
	(0.50)	(0.79)	(0.16)	(0.15)	(0.19)	
Firms relocating out	0.02	0.04	0.01	0.02	0.02	0.37
	(0.30)	(0.47)	(0.11)	(0.15)	(0.15)	
Total employment 2nd and 3rd sectors (in 1,000)	0.24	0.27	0.22	0.22	0.17	0.17
	(0.47)	(0.58)	(0.39)	(0.41)	(0.33)	
Share of employment in 2nd sector	0.61	0.62	0.60	0.59	0.61	0.56
	(0.22)	(0.23)	(0.22)	(0.21)	(0.24)	
Share of employment in 3rd sector	0.39	0.38	0.40	0.41	0.40	0.61
	(0.22)	(0.24)	(0.22)	(0.21)	(0.24)	
# Municipalities	1082	397	342	194	76	

Notes: The table reports means of firm and employment variables for the pre-treatment period 1960–1962, separately for all treated municipalities (column 1) and by highway access cohort (columns 2–5). Standard deviations are in parentheses. Column 6 reports the p-value of an F-test of equality of means across cohorts (2) to (5). The sample is restricted to non-urban municipalities that eventually receive highway access within 15 km of their centroid.

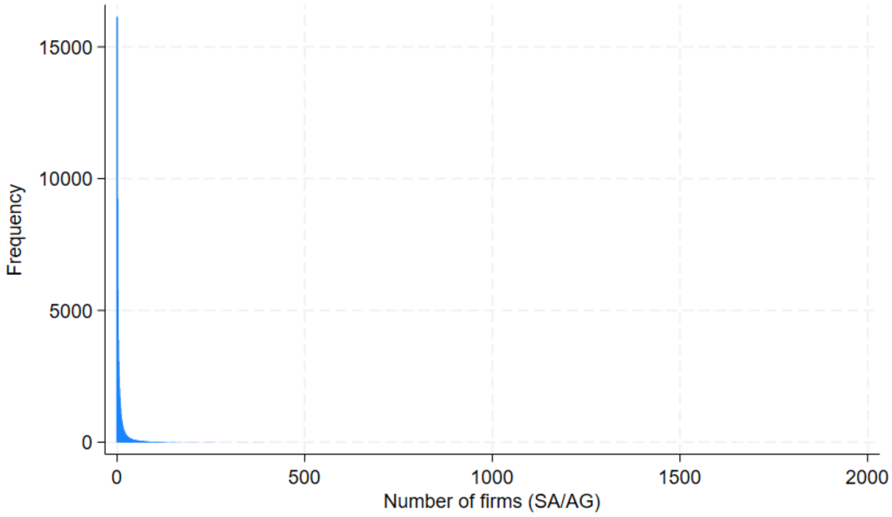
1.B.3 Frequency distributions

Figure 1.B.3: Frequency distribution of the number of firms - Non-urban connected municipalities



Notes: This graphic shows the frequency distribution in the subsample of municipalities with an access to a highway less than 15 km away. It also excludes urban centers and suburban municipalities. In Table 1.C.8, this corresponds to (2), i.e., the non-urban connected municipalities.

Figure 1.B.4: Frequency distribution of the number of firms - Non-urban municipalities



Notes: This graphic shows the frequency distribution in the subsample of municipalities with an access to a highway less than 30 km away. It also excludes urban centers and suburban municipalities. In Table 1.C.8, this corresponds to (2) and (3).

1.C Additional results

1.C.1 Main analysis

Note that the sample size and the number of municipalities differ across specifications. The variation across our outcome variables arises because some municipalities had no births, deaths, or relocations during the periods we analyze. Relocations are less frequent than deaths, which are themselves less frequent than births. The drop in observations when adding municipality-specific time trends is typically because those trends create separation or singletons in PQML, so *ppmlhdfe* automatically removes the affected observations to ensure convergence.

Table 1.C.4: Cumulative effect before/after access (<15 km)

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.119	(0.146)	0.030	(0.037)	0.020	(0.234)	0.217*	(0.124)	-0.541***	(0.139)	0.364*	(0.220)
-7	-0.093	(0.064)	-0.055	(0.063)	-0.049	(0.245)	0.000	(0.160)	-0.364**	(0.175)	-0.002	(0.237)
-6	-0.072	(0.053)	-0.014	(0.044)	0.256	(0.157)	0.072	(0.108)	-0.299	(0.183)	-0.056	(0.200)
-5	-0.057	(0.042)	-0.001	(0.036)	0.062	(0.186)	0.114	(0.112)	-0.019	(0.132)	0.099	(0.164)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.021	(0.014)	0.019	(0.014)	0.175	(0.145)	0.016	(0.174)	-0.094	(0.144)	0.284	(0.185)
-2	0.038	(0.029)	0.038	(0.025)	0.122	(0.089)	0.421***	(0.125)	-0.222	(0.139)	0.214	(0.231)
-1	0.047	(0.040)	0.043	(0.033)	0.161	(0.112)	0.137	(0.102)	-0.203	(0.143)	0.226	(0.218)
+0	0.069	(0.049)	0.063	(0.041)	0.172*	(0.094)	0.080	(0.112)	0.279	(0.228)	0.192	(0.226)
+1	0.099	(0.064)	0.090*	(0.054)	0.265**	(0.109)	0.271**	(0.127)	-0.020	(0.154)	-0.113	(0.224)
+2	0.119	(0.077)	0.119*	(0.066)	0.336***	(0.091)	0.227*	(0.118)	-0.119	(0.128)	-0.012	(0.226)
+3	0.140	(0.090)	0.140*	(0.075)	0.424***	(0.113)	0.304***	(0.113)	-0.216	(0.151)	0.310	(0.196)
+4	0.157	(0.101)	0.155*	(0.081)	0.348**	(0.136)	0.609***	(0.217)	-0.142	(0.122)	0.199	(0.234)
+5	0.168	(0.113)	0.173*	(0.089)	0.265***	(0.102)	0.563*	(0.300)	-0.120	(0.150)	0.264	(0.298)
+6	0.189	(0.124)	0.192**	(0.094)	0.395***	(0.136)	0.890***	(0.298)	0.055	(0.121)	0.451*	(0.272)
+7	0.183	(0.129)	0.185**	(0.083)	0.322*	(0.182)	0.423**	(0.188)	-0.106	(0.163)	0.255	(0.235)
+8	0.187	(0.139)	0.186**	(0.076)	0.351*	(0.199)	0.487**	(0.206)	-0.046	(0.153)	0.331	(0.212)
+9	0.175	(0.149)	0.174**	(0.072)	0.317	(0.197)	0.469***	(0.127)	0.088	(0.149)	0.224	(0.236)
+10	0.171	(0.159)	0.168**	(0.070)	0.322	(0.206)	0.362***	(0.136)	-0.078	(0.138)	0.158	(0.229)
+11	0.177	(0.170)	0.175**	(0.073)	0.334	(0.208)	0.373***	(0.145)	-0.114	(0.173)	0.064	(0.233)
+12	0.178	(0.181)	0.183**	(0.079)	0.340	(0.215)	0.473***	(0.157)	0.023	(0.156)	0.053	(0.239)
+13	0.192	(0.189)	0.198**	(0.084)	0.357*	(0.190)	0.492***	(0.154)	0.016	(0.159)	0.239	(0.249)
+14	0.205	(0.199)	0.214**	(0.091)	0.400**	(0.203)	0.514***	(0.164)	0.065	(0.153)	0.374	(0.279)
≥15	0.219	(0.222)	0.246**	(0.102)	0.438**	(0.204)	0.676***	(0.174)	0.050	(0.154)	0.183	(0.277)
Long-term effect	0.289	(0.285)	0.265**	(0.110)	0.366**	(0.159)	0.655***	(0.203)	0.149	(0.180)	0.191	(0.276)
# Observations	45848		45720		44696		40377		36916		35088	
# Municipalities	1042		1041		1023		939		839		816	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	No		Yes		Yes		Yes		Yes		Yes	

Notes: The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

1.C.2 Main analysis - Flow variables, all specifications

Table 1.C.5 provides the complete results for our flow variables. It also includes the regressions without municipal time trends.

Table 1.C.5: Cumulative effect before/after access (<15 km) - Flow variables, all specifications

	Births				Deaths				Relocations In				Relocations Out			
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
≤8	-0.381	(0.251)	0.020	(0.234)	-0.068	(0.244)	0.217*	(0.124)	-0.552*	(0.331)	-0.541***	(0.139)	0.144	(0.312)	0.364*	(0.220)
-7	-0.266	(0.164)	-0.049	(0.245)	-0.051	(0.155)	0.000	(0.160)	-0.458*	(0.253)	-0.364**	(0.175)	-0.133	(0.288)	-0.002	(0.237)
-6	-0.234	(0.166)	0.256	(0.157)	-0.075	(0.141)	0.072	(0.108)	-0.498*	(0.279)	-0.299	(0.183)	-0.273	(0.255)	-0.056	(0.200)
-5	-0.293**	(0.145)	0.062	(0.186)	-0.032	(0.141)	0.114	(0.112)	-0.148	(0.215)	-0.019	(0.132)	-0.091	(0.193)	0.099	(0.164)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	-0.198	(0.131)	0.175	(0.145)	0.073	(0.122)	0.016	(0.174)	-0.269	(0.197)	-0.094	(0.144)	0.189	(0.202)	0.284	(0.185)
-2	-0.209**	(0.083)	0.122	(0.089)	0.339***	(0.117)	0.421***	(0.125)	-0.380**	(0.171)	-0.222	(0.139)	0.083	(0.200)	0.214	(0.231)
-1	-0.123	(0.088)	0.161	(0.112)	0.125	(0.105)	0.137	(0.102)	-0.328**	(0.164)	-0.203	(0.143)	0.161	(0.199)	0.226	(0.218)
+0	-0.131	(0.089)	0.172*	(0.094)	0.064	(0.111)	0.080	(0.112)	0.171	(0.229)	0.279	(0.228)	0.141	(0.188)	0.192	(0.226)
+1	-0.061	(0.070)	0.265**	(0.109)	0.198*	(0.103)	0.271**	(0.127)	-0.193	(0.164)	-0.020	(0.154)	-0.178	(0.160)	-0.113	(0.224)
+2	-0.023	(0.105)	0.336***	(0.091)	0.256**	(0.106)	0.227*	(0.118)	-0.268**	(0.134)	-0.119	(0.128)	0.003	(0.168)	-0.012	(0.226)
+3	0.057	(0.150)	0.424***	(0.113)	0.217*	(0.116)	0.304***	(0.113)	-0.423**	(0.180)	-0.216	(0.151)	0.189	(0.184)	0.310	(0.196)
+4	-0.029	(0.088)	0.348**	(0.136)	0.534***	(0.197)	0.609***	(0.217)	-0.333**	(0.133)	-0.142	(0.122)	0.139	(0.171)	0.199	(0.234)
+5	-0.063	(0.080)	0.265***	(0.102)	0.625**	(0.311)	0.563*	(0.300)	-0.205	(0.137)	-0.120	(0.150)	0.308	(0.224)	0.264	(0.298)
+6	0.001	(0.107)	0.395***	(0.136)	0.948**	(0.396)	0.890***	(0.298)	-0.132	(0.132)	0.055	(0.121)	0.411	(0.271)	0.451*	(0.272)
+7	-0.027	(0.129)	0.322*	(0.182)	0.328*	(0.172)	0.423**	(0.188)	-0.249	(0.155)	-0.106	(0.163)	0.250	(0.215)	0.255	(0.235)
+8	0.038	(0.162)	0.351*	(0.199)	0.440**	(0.177)	0.487**	(0.206)	-0.103	(0.133)	-0.046	(0.153)	0.391*	(0.217)	0.331	(0.212)
+9	-0.076	(0.132)	0.317	(0.197)	0.354**	(0.156)	0.469***	(0.127)	-0.086	(0.150)	0.088	(0.149)	0.161	(0.183)	0.224	(0.236)
+10	-0.025	(0.150)	0.322	(0.206)	0.407***	(0.157)	0.362***	(0.136)	-0.157	(0.130)	-0.078	(0.138)	0.237	(0.212)	0.158	(0.229)
+11	0.005	(0.179)	0.334	(0.208)	0.452***	(0.153)	0.373***	(0.145)	-0.167	(0.171)	-0.114	(0.173)	0.219	(0.191)	0.064	(0.233)
+12	-0.057	(0.161)	0.340	(0.215)	0.385**	(0.181)	0.473***	(0.157)	-0.104	(0.142)	0.023	(0.156)	0.062	(0.182)	0.053	(0.239)
+13	-0.044	(0.147)	0.357*	(0.190)	0.416**	(0.195)	0.492***	(0.154)	-0.132	(0.153)	0.016	(0.159)	0.274	(0.188)	0.239	(0.249)
+14	0.011	(0.168)	0.400**	(0.203)	0.441**	(0.211)	0.514***	(0.164)	-0.043	(0.146)	0.065	(0.153)	0.403*	(0.213)	0.374	(0.279)
≥15	0.022	(0.183)	0.438**	(0.204)	0.616**	(0.274)	0.676***	(0.174)	-0.017	(0.158)	0.050	(0.154)	0.322	(0.253)	0.183	(0.277)
Long-term effect	0.224	(0.256)	0.366**	(0.159)	0.664*	(0.385)	0.655***	(0.203)	0.318	(0.297)	0.149	(0.180)	0.346	(0.382)	0.191	(0.276)
# Observations	45100		44696		40377		40377		36916		36916		35088		35088	
# Municipalities	1025		1023		939		939		839		839		816		816	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	No		Yes		No		Yes		No		Yes		No		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

1.C.3 Robustness checks and extensions

Inclusion of non-connected municipalities

Table 1.C.6: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 20 km)

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.052	(0.104)	0.018	(0.037)	0.052	(0.239)	0.206*	(0.123)	-0.522***	(0.127)	0.324	(0.219)
-7	-0.078	(0.056)	-0.055	(0.065)	-0.020	(0.256)	0.009	(0.156)	-0.371**	(0.174)	-0.025	(0.242)
-6	-0.062	(0.047)	-0.014	(0.046)	0.285*	(0.160)	0.066	(0.107)	-0.302*	(0.181)	-0.099	(0.202)
-5	-0.052	(0.040)	0.002	(0.038)	0.094	(0.192)	0.124	(0.112)	-0.024	(0.132)	0.063	(0.165)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.016	(0.012)	0.021	(0.014)	0.210	(0.151)	0.019	(0.175)	-0.087	(0.148)	0.223	(0.187)
-2	0.028	(0.023)	0.042	(0.026)	0.149	(0.096)	0.431***	(0.122)	-0.232	(0.142)	0.189	(0.230)
-1	0.031	(0.031)	0.050	(0.034)	0.195*	(0.118)	0.138	(0.100)	-0.204	(0.143)	0.194	(0.214)
+0	0.048	(0.037)	0.073*	(0.043)	0.201*	(0.103)	0.088	(0.113)	0.261	(0.233)	0.157	(0.220)
+1	0.073	(0.049)	0.102*	(0.056)	0.298***	(0.114)	0.287**	(0.129)	-0.029	(0.146)	-0.129	(0.221)
+2	0.087	(0.060)	0.132*	(0.068)	0.362***	(0.101)	0.241**	(0.118)	-0.143	(0.124)	-0.066	(0.242)
+3	0.103	(0.070)	0.156**	(0.079)	0.455***	(0.125)	0.326***	(0.115)	-0.248*	(0.149)	0.273	(0.192)
+4	0.113	(0.076)	0.173**	(0.085)	0.375***	(0.142)	0.637***	(0.229)	-0.159	(0.119)	0.165	(0.234)
+5	0.119	(0.085)	0.193**	(0.093)	0.286***	(0.107)	0.581*	(0.313)	-0.161	(0.152)	0.211	(0.299)
+6	0.135	(0.091)	0.214**	(0.100)	0.425***	(0.149)	0.920***	(0.314)	0.022	(0.126)	0.423	(0.266)
+7	0.123	(0.089)	0.208**	(0.089)	0.347*	(0.197)	0.453**	(0.197)	-0.139	(0.166)	0.237	(0.226)
+8	0.120	(0.093)	0.211**	(0.083)	0.380*	(0.215)	0.517**	(0.211)	-0.086	(0.153)	0.296	(0.201)
+9	0.102	(0.098)	0.200**	(0.079)	0.336	(0.207)	0.507***	(0.129)	0.053	(0.156)	0.209	(0.228)
+10	0.089	(0.106)	0.194**	(0.077)	0.335	(0.217)	0.399***	(0.126)	-0.123	(0.141)	0.140	(0.218)
+11	0.089	(0.113)	0.201**	(0.081)	0.347	(0.221)	0.411***	(0.138)	-0.163	(0.180)	0.041	(0.228)
+12	0.082	(0.121)	0.208**	(0.086)	0.346	(0.229)	0.501***	(0.167)	-0.040	(0.164)	0.039	(0.226)
+13	0.087	(0.124)	0.222**	(0.091)	0.362*	(0.203)	0.518***	(0.162)	-0.037	(0.161)	0.236	(0.237)
+14	0.093	(0.130)	0.237**	(0.098)	0.403*	(0.218)	0.539***	(0.175)	0.005	(0.162)	0.368	(0.265)
≥15	0.041	(0.130)	0.264**	(0.108)	0.430**	(0.214)	0.695***	(0.188)	-0.017	(0.164)	0.173	(0.261)
Long-term effect	0.088	(0.174)	0.279**	(0.113)	0.344**	(0.162)	0.661***	(0.211)	0.096	(0.192)	0.191	(0.260)
# Observations	53504		53332		51989		46913		42636		40635	
# Municipalities	1216		1214		1189		1091		969		945	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	No		Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. We include rural municipalities with an access between 15 km and 20 km in the control group. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 1.C.7: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 25 km)

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.063	(0.090)	0.013	(0.037)	0.055	(0.237)	0.206*	(0.124)	-0.522***	(0.126)	0.289	(0.223)
-7	-0.080	(0.053)	-0.057	(0.065)	-0.017	(0.255)	0.009	(0.150)	-0.380**	(0.172)	-0.047	(0.241)
-6	-0.063	(0.045)	-0.014	(0.046)	0.291*	(0.157)	0.076	(0.104)	-0.307*	(0.183)	-0.121	(0.203)
-5	-0.052	(0.039)	0.002	(0.037)	0.093	(0.191)	0.127	(0.109)	-0.033	(0.134)	0.043	(0.166)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.017	(0.011)	0.021	(0.014)	0.207	(0.149)	0.029	(0.161)	-0.101	(0.149)	0.218	(0.186)
-2	0.030	(0.021)	0.043	(0.026)	0.148	(0.095)	0.428***	(0.128)	-0.250*	(0.147)	0.181	(0.233)
-1	0.034	(0.028)	0.052	(0.035)	0.199*	(0.117)	0.152	(0.103)	-0.220	(0.146)	0.195	(0.217)
+0	0.052	(0.034)	0.075*	(0.044)	0.200**	(0.100)	0.096	(0.112)	0.242	(0.236)	0.156	(0.224)
+1	0.078*	(0.045)	0.105*	(0.057)	0.300***	(0.115)	0.281**	(0.140)	-0.053	(0.147)	-0.115	(0.226)
+2	0.092*	(0.055)	0.136*	(0.070)	0.360***	(0.101)	0.259**	(0.123)	-0.157	(0.124)	-0.046	(0.246)
+3	0.110*	(0.065)	0.160**	(0.081)	0.457***	(0.129)	0.334***	(0.117)	-0.269*	(0.148)	0.290	(0.197)
+4	0.121*	(0.070)	0.177**	(0.087)	0.375***	(0.142)	0.642***	(0.246)	-0.185	(0.118)	0.186	(0.239)
+5	0.127	(0.078)	0.198**	(0.096)	0.288***	(0.109)	0.591*	(0.329)	-0.186	(0.158)	0.234	(0.304)
+6	0.144*	(0.083)	0.220**	(0.103)	0.425***	(0.153)	0.930***	(0.332)	-0.005	(0.127)	0.446*	(0.269)
+7	0.133*	(0.077)	0.214**	(0.092)	0.345*	(0.198)	0.459**	(0.212)	-0.166	(0.166)	0.269	(0.230)
+8	0.132*	(0.078)	0.217**	(0.086)	0.377*	(0.217)	0.531**	(0.224)	-0.121	(0.155)	0.328	(0.206)
+9	0.114	(0.082)	0.207**	(0.082)	0.334	(0.209)	0.513***	(0.141)	0.024	(0.157)	0.240	(0.232)
+10	0.103	(0.089)	0.202**	(0.081)	0.332	(0.220)	0.408***	(0.137)	-0.159	(0.143)	0.174	(0.223)
+11	0.104	(0.096)	0.209**	(0.084)	0.346	(0.223)	0.421***	(0.153)	-0.192	(0.187)	0.081	(0.238)
+12	0.098	(0.102)	0.216**	(0.090)	0.340	(0.230)	0.510***	(0.180)	-0.070	(0.163)	0.086	(0.232)
+13	0.105	(0.104)	0.230**	(0.095)	0.353*	(0.203)	0.527***	(0.176)	-0.068	(0.160)	0.292	(0.242)
+14	0.111	(0.109)	0.245**	(0.102)	0.398*	(0.218)	0.547***	(0.190)	-0.025	(0.161)	0.420	(0.269)
≥15	0.070	(0.108)	0.273**	(0.110)	0.424**	(0.215)	0.706***	(0.201)	-0.047	(0.167)	0.235	(0.266)
Long-term effect	0.122	(0.140)	0.287**	(0.115)	0.338**	(0.162)	0.664***	(0.221)	0.074	(0.196)	0.260	(0.259)
# Observations	56584		56412		55067		49579		44880		42699	
# Municipalities	1286		1284		1259		1153		1020		993	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	No		Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. We include rural municipalities with an access between 15 km and 25 km in the control group. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 1.C.8: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 30 km)

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.068	(0.085)	0.003	(0.037)	0.058	(0.244)	0.190	(0.122)	-0.519***	(0.125)	0.291	(0.222)
-7	-0.080	(0.051)	-0.058	(0.067)	-0.008	(0.262)	0.011	(0.152)	-0.371**	(0.171)	-0.038	(0.244)
-6	-0.064	(0.043)	-0.015	(0.047)	0.300*	(0.163)	0.078	(0.103)	-0.301	(0.183)	-0.120	(0.206)
-5	-0.052	(0.038)	0.003	(0.039)	0.109	(0.197)	0.128	(0.110)	-0.020	(0.136)	0.047	(0.168)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.018	(0.011)	0.023	(0.014)	0.223	(0.155)	0.045	(0.161)	-0.090	(0.146)	0.224	(0.186)
-2	0.032	(0.021)	0.046*	(0.027)	0.164	(0.101)	0.439***	(0.127)	-0.233	(0.144)	0.184	(0.234)
-1	0.037	(0.028)	0.056	(0.036)	0.215*	(0.123)	0.166	(0.103)	-0.202	(0.145)	0.200	(0.217)
+0	0.056*	(0.033)	0.081*	(0.044)	0.218**	(0.107)	0.109	(0.111)	0.258	(0.231)	0.157	(0.224)
+1	0.083*	(0.044)	0.113*	(0.058)	0.321***	(0.123)	0.301**	(0.139)	-0.047	(0.147)	-0.125	(0.231)
+2	0.098*	(0.054)	0.145**	(0.071)	0.376***	(0.109)	0.286**	(0.121)	-0.142	(0.123)	-0.042	(0.247)
+3	0.116*	(0.064)	0.170**	(0.082)	0.476***	(0.137)	0.362***	(0.119)	-0.255*	(0.147)	0.290	(0.197)
+4	0.128*	(0.069)	0.189**	(0.089)	0.393***	(0.148)	0.673***	(0.250)	-0.167	(0.119)	0.189	(0.241)
+5	0.135*	(0.076)	0.210**	(0.098)	0.307***	(0.116)	0.624*	(0.332)	-0.164	(0.153)	0.234	(0.308)
+6	0.153*	(0.081)	0.233**	(0.105)	0.446***	(0.162)	0.966***	(0.338)	0.011	(0.126)	0.445	(0.271)
+7	0.143*	(0.073)	0.228**	(0.095)	0.366*	(0.207)	0.492**	(0.217)	-0.150	(0.164)	0.267	(0.227)
+8	0.142*	(0.074)	0.233***	(0.090)	0.397*	(0.225)	0.567**	(0.226)	-0.110	(0.152)	0.318	(0.205)
+9	0.126	(0.078)	0.223***	(0.086)	0.354	(0.217)	0.553***	(0.145)	0.044	(0.155)	0.244	(0.229)
+10	0.115	(0.084)	0.219***	(0.084)	0.352	(0.229)	0.451***	(0.138)	-0.139	(0.140)	0.171	(0.219)
+11	0.117	(0.091)	0.227***	(0.088)	0.366	(0.232)	0.463***	(0.152)	-0.169	(0.182)	0.083	(0.236)
+12	0.111	(0.097)	0.234**	(0.093)	0.358	(0.239)	0.548***	(0.187)	-0.050	(0.161)	0.088	(0.229)
+13	0.118	(0.098)	0.249**	(0.098)	0.372*	(0.213)	0.565***	(0.183)	-0.050	(0.158)	0.289	(0.238)
+14	0.125	(0.103)	0.264**	(0.106)	0.415*	(0.227)	0.588***	(0.197)	-0.007	(0.158)	0.421	(0.265)
≥15	0.087	(0.102)	0.292**	(0.114)	0.439**	(0.223)	0.747***	(0.211)	-0.029	(0.163)	0.232	(0.260)
Long-term effect	0.142	(0.129)	0.305***	(0.118)	0.346**	(0.166)	0.701***	(0.229)	0.089	(0.191)	0.252	(0.255)
# Observations	58916		58744		57351		51729		46244		44032	
# Municipalities	1339		1337		1311		1203		1051		1024	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	No		Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10. We include rural municipalities with an access between 15 km and 30 km in the control group.

Heterogeneous difference-in-differences

Table 1.C.9: Cumulative effect before/after access (<15 km), including non-connected municipalities (15 km to 30 km) - ETWFE

	Number of Firms (1)		Birth (2)		Death (3)		Relocation In (4)		Relocation Out (5)	
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.027	(0.019)	0.004	(0.066)	0.448	(.)	-0,029	(0.335)	0.096	(0.257)
-2	0.037*	(0.022)	0.042	(0.063)	0.547	(.)	0.036	(0.201)	0.319	(0.234)
-1	0.045*	(0.025)	0.048	(0.065)	0.069	(.)	0.215	(0.238)	0.282	(0.206)
0	0.067**	(0.028)	0.087	(0.055)	0.211	(.)	0.335*	(0.175)	0.184	(0.230)
+1	0.091***	(0.031)	0.199***	(0.067)	0.218	(.)	0.414**	(0.164)	0.150	(0.188)
+2	0.106***	(0.035)	0.121**	(0.061)	0.133	(.)	0.305	(0.187)	0.146	(0.216)
+3	0.123***	(0.039)	0.172***	(0.064)	0.194	(.)	-0.080	(0.180)	0.373**	(0.185)
+4	0.136***	(0.041)	0.185**	(0.075)	0.642	(.)	0.113	(0.174)	0.289**	(0.147)
+5	0.139***	(0.044)	0.109	(0.070)	0.226	(.)	0.232	(0.167)	0.044	(0.287)
+6	0.152***	(0.047)	0.132*	(0.076)	0.726	(.)	0.129	(0.204)	0.299	(0.239)
+7	0.146***	(0.050)	0.071	(0.076)	0.552	(.)	-0.116	(0.239)	0.209	(0.331)
+8	0.150***	(0.055)	0.106	(0.085)	0.248	(.)	-0.046	(0.202)	0.350*	(0.186)
+9	0.142**	(0.059)	0.131	(0.081)	0.349	(.)	0.188	(0.171)	0.426**	(0.179)
+10	0.137**	(0.062)	0.017	(0.090)	0.550	(.)	0.141	(0.176)	0.380**	(0.155)
+11	0.135**	(0.066)	0.072	(0.101)	0.333	(.)	-0.212	(0.271)	0.290**	(0.136)
+12	0.123*	(0.070)	0.076	(0.102)	0.575	(.)	-0.140	(0.242)	0.146	(0.156)
+13	0.120	(0.073)	0.025	(0.105)	0.509	(.)	0.121	(0.189)	0.322*	(0.172)
+14	0.125*	(0.076)	0.088	(0.111)	0.442	(.)	0.258*	(0.155)	0.481**	(0.199)
≥15	0.138	(0.105)	0.109	(0.146)	0.685	(.)	0.119	(0.210)	0.300	(0.219)
Observations	51788		50802		51763		37960		35183	
Municipalities	1177		1157		1223		922		894	
Year FE	Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The treatment group includes all non-urban municipalities within 15 km of an access. The control group includes not-yet-treated and never-treated observations (15 km to 30 km of an access). Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Urban Shadows - Euclidean distances

Table 1.C.10: Urban shadows - Distance to urban centers (Euclidean distances)

	Stock (1)		Births (2)		Deaths (3)		Relocation In (4)		Relocation Out (5)	
Long-term effect										
distance $<$ 15 km	0.009	(0.103)	0.011	(0.164)	0.513	(0.330)	-0.144	(0.213)	-0.358	(0.412)
distance $\geq$ 15 km	0.291***	(0.105)	0.372*	(0.166)	0.370**	(0.230)	0.315	(0.230)	0.311	(0.190)
# Observations	45720		44700		40377		36433		34611	
# Municipalities	1041		1023		939		839		816	

Notes: The median distance between municipalities and the nearest urban center is 15.120 km, which we round to the nearest integer. Include municipality fixed effects, time fixed effects, and municipal time trends. The sample includes all non-urban municipalities within 15 km of an access. The standard errors, in parentheses, are clustered at the district level. ***p $<$ 0.01, **p $<$ 0.05, and *p $<$ 0.10.

Size of firms and distribution by size

Table 1.C.11: Cumulative effect before/after access (<15 km) in the bottom 50th, 50th-75th, 75th-90th, and 90th percentile of national nominal capital size distribution

	Bottom 50th (1)		50-75th (2)		75-90th (3)		Top 10% (4)		90-99th (5)		Top 1% (6)	
≤-8	0.044	(0.069)	-0.078	(0.050)	0.048	(0.044)	-0.033	(0.066)	-0.072	(0.068)	0.573**	(0.292)
-7	-0.121	(0.106)	-0.103*	(0.056)	0.026	(0.048)	-0.005	(0.042)	-0.028	(0.042)	0.317*	(0.173)
-6	-0.098	(0.060)	-0.013	(0.034)	0.006	(0.031)	-0.025	(0.036)	-0.045	(0.036)	0.140	(0.154)
-5	-0.060	(0.057)	-0.021	(0.035)	0.012	(0.030)	0.016	(0.032)	0.001	(0.032)	0.154	(0.161)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.014	(0.030)	0.040	(0.034)	-0.013	(0.026)	-0.034	(0.027)	-0.042	(0.028)	-0.093	(0.244)
-2	0.024	(0.048)	0.012	(0.027)	0.011	(0.036)	-0.016	(0.028)	-0.019	(0.028)	-0.088	(0.168)
-1	0.008	(0.054)	0.035	(0.034)	0.026	(0.038)	-0.049	(0.033)	-0.061*	(0.031)	0.048	(0.170)
+0	0.056	(0.072)	0.028	(0.033)	0.052	(0.033)	-0.063*	(0.037)	-0.076**	(0.037)	0.069	(0.195)
+1	0.017	(0.052)	0.026	(0.044)	-0.012	(0.034)	-0.046	(0.043)	-0.052	(0.046)	-0.097	(0.222)
+2	0.077	(0.062)	0.090**	(0.042)	0.006	(0.038)	-0.065	(0.047)	-0.069	(0.047)	-0.122	(0.235)
+3	0.259*	(0.138)	0.134**	(0.055)	0.043	(0.042)	-0.042	(0.050)	-0.057	(0.048)	-0.028	(0.243)
+4	0.087	(0.062)	0.103*	(0.056)	0.018	(0.049)	-0.042	(0.057)	-0.057	(0.057)	-0.030	(0.268)
+5	0.109	(0.069)	0.081	(0.059)	0.018	(0.048)	-0.048	(0.058)	-0.056	(0.059)	-0.116	(0.291)
+6	0.350**	(0.175)	0.122*	(0.067)	0.029	(0.054)	-0.022	(0.060)	-0.031	(0.061)	-0.163	(0.298)
+7	0.164**	(0.080)	0.079	(0.073)	0.019	(0.059)	0.028	(0.069)	0.014	(0.070)	-0.066	(0.318)
+8	0.201**	(0.082)	0.126	(0.085)	0.014	(0.060)	0.024	(0.067)	0.025	(0.069)	-0.306	(0.325)
+9	0.144	(0.088)	0.110	(0.076)	0.052	(0.066)	0.025	(0.071)	0.019	(0.071)	-0.222	(0.365)
+10	0.225**	(0.096)	0.128	(0.080)	0.035	(0.073)	0.038	(0.075)	0.038	(0.076)	-0.336	(0.351)
+11	0.229**	(0.090)	0.155*	(0.087)	0.041	(0.067)	0.048	(0.086)	0.038	(0.084)	-0.188	(0.359)
+12	0.208**	(0.096)	0.139*	(0.083)	0.056	(0.072)	0.028	(0.082)	0.022	(0.083)	-0.254	(0.398)
+13	0.236**	(0.100)	0.165*	(0.085)	0.073	(0.078)	0.016	(0.083)	0.009	(0.083)	-0.280	(0.418)
+14	0.242**	(0.102)	0.175**	(0.089)	0.080	(0.079)	0.041	(0.087)	0.038	(0.087)	-0.304	(0.413)
≥+15	0.292**	(0.122)	0.238**	(0.104)	0.047	(0.083)	0.103	(0.098)	0.103	(0.096)	-0.335	(0.465)
Long-term effect	0.362***	(0.135)	0.255***	(0.097)	0.045	(0.084)	0.099	(0.101)	0.105	(0.099)	-0.315	(0.431)
# Observations	34403		30254		24577		19695		19185		5542	
# Municipalities	1013		890		723		582		567		163	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes		Yes		Yes	

Notes: The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 1.C.12: Cumulative effect before/after access (<15 km) by board sizes

	1 board member (1)		2 board members (2)		3 board members (3)		4 board members (4)		5 board members (5)		6+ board members (6)	
≤-8	-0.015	(0.047)	0.026	(0.073)	0.053	(0.066)	0.024	(0.115)	-0.164	(0.117)	-0.046	(0.119)
-7	-0.157	(0.096)	-0.048	(0.080)	0.004	(0.040)	0.002	(0.100)	-0.043	(0.101)	-0.020	(0.088)
-6	-0.096*	(0.057)	-0.001	(0.059)	0.039	(0.039)	0.063	(0.070)	-0.120	(0.084)	0.029	(0.069)
-5	-0.054	(0.054)	0.019	(0.052)	-0.013	(0.033)	-0.010	(0.049)	-0.021	(0.073)	-0.029	(0.050)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.009	(0.034)	0.077	(0.049)	-0.012	(0.038)	0.007	(0.053)	-0.015	(0.077)	0.006	(0.075)
-2	0.005	(0.040)	0.027	(0.035)	0.050	(0.044)	-0.040	(0.070)	0.034	(0.089)	-0.048	(0.080)
-1	-0.021	(0.048)	0.056	(0.045)	0.036	(0.044)	-0.073	(0.087)	0.063	(0.091)	-0.030	(0.079)
+0	0.004	(0.057)	0.067	(0.072)	0.056	(0.046)	0.014	(0.080)	-0.032	(0.104)	-0.005	(0.080)
+1	-0.036	(0.046)	0.023	(0.050)	0.059	(0.054)	-0.032	(0.083)	0.004	(0.110)	-0.051	(0.091)
+2	0.044	(0.053)	0.052	(0.053)	0.052	(0.056)	0.048	(0.076)	0.022	(0.121)	-0.066	(0.094)
+3	0.234*	(0.125)	0.171*	(0.090)	0.067	(0.064)	-0.018	(0.091)	0.048	(0.123)	-0.148	(0.107)
+4	0.032	(0.060)	0.072	(0.076)	0.097	(0.066)	0.032	(0.097)	0.026	(0.130)	-0.097	(0.111)
+5	0.041	(0.062)	0.079	(0.067)	0.085	(0.076)	0.024	(0.105)	0.065	(0.119)	-0.133	(0.122)
+6	0.317*	(0.171)	0.159	(0.103)	0.093	(0.071)	0.006	(0.112)	0.083	(0.126)	-0.171	(0.133)
+7	0.083	(0.074)	0.095	(0.097)	0.132	(0.082)	0.001	(0.116)	0.040	(0.128)	-0.104	(0.155)
+8	0.114	(0.079)	0.122	(0.103)	0.158*	(0.081)	0.008	(0.119)	0.088	(0.133)	-0.041	(0.142)
+9	0.052	(0.084)	0.119	(0.097)	0.118	(0.081)	0.052	(0.125)	0.063	(0.151)	0.054	(0.143)
+10	0.117	(0.092)	0.160	(0.104)	0.149*	(0.086)	0.056	(0.123)	0.088	(0.159)	-0.012	(0.146)
+11	0.112	(0.082)	0.230*	(0.137)	0.141	(0.091)	0.081	(0.125)	0.050	(0.165)	-0.013	(0.153)
+12	0.125	(0.088)	0.150	(0.109)	0.137	(0.093)	0.039	(0.135)	0.083	(0.174)	-0.066	(0.164)
+13	0.144	(0.091)	0.172	(0.111)	0.116	(0.093)	0.023	(0.142)	0.112	(0.174)	-0.031	(0.172)
+14	0.143	(0.095)	0.183	(0.116)	0.141	(0.092)	-0.034	(0.148)	0.184	(0.171)	-0.029	(0.168)
≥+15	0.201*	(0.122)	0.213	(0.138)	0.174*	(0.100)	0.031	(0.161)	0.192	(0.178)	-0.094	(0.185)
Long-term effect	0.265**	(0.119)	0.219*	(0.126)	0.174*	(0.094)	0.021	(0.160)	0.210	(0.187)	-0.098	(0.189)
# Observations	33312		32291		31009		24596		20450		15991	
# Municipalities	981		950		913		726		603		471	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes		Yes		Yes	

Notes: The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Spillovers - Exclusion of "close-controls"

Table 1.C.13: Cumulative effect before/after access (<15 km), excluding municipalities close to treated units (<5 km)

	Number of Firms		Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)	
≤8	0.041	(0.059)	-0.034	(0.291)	0.265*	(0.151)	-0.306	(0.213)	0.688**	(0.302)
-7	-0.067	(0.099)	-0.052	(0.296)	-0.016	(0.235)	-0.454	(0.284)	0.173	(0.293)
-6	0.003	(0.070)	0.383**	(0.188)	0.313**	(0.138)	-0.060	(0.217)	0.254	(0.268)
-5	0.009	(0.056)	0.036	(0.217)	0.260*	(0.147)	0.025	(0.213)	0.189	(0.270)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.010	(0.021)	0.227	(0.167)	0.047	(0.270)	-0.125	(0.216)	0.415*	(0.248)
-2	0.033	(0.034)	0.131*	(0.079)	0.497***	(0.182)	-0.301	(0.231)	0.362	(0.297)
-1	0.043	(0.045)	0.225**	(0.109)	0.141	(0.173)	-0.350*	(0.184)	0.481*	(0.252)
+0	0.071	(0.052)	0.213**	(0.108)	0.115	(0.153)	0.163	(0.234)	0.274	(0.267)
+1	0.096	(0.066)	0.299**	(0.124)	0.292	(0.188)	-0.171	(0.208)	-0.050	(0.270)
+2	0.121	(0.077)	0.381***	(0.104)	0.240	(0.167)	-0.285	(0.190)	0.059	(0.267)
+3	0.141	(0.088)	0.467***	(0.116)	0.316**	(0.160)	-0.394*	(0.215)	0.364	(0.250)
+4	0.156	(0.095)	0.400***	(0.146)	0.638**	(0.255)	-0.331*	(0.196)	0.265	(0.271)
+5	0.176*	(0.102)	0.321***	(0.110)	0.600*	(0.334)	-0.319	(0.208)	0.336	(0.339)
+6	0.194*	(0.108)	0.455***	(0.139)	0.914***	(0.332)	-0.156	(0.186)	0.494*	(0.292)
+7	0.186*	(0.099)	0.386**	(0.188)	0.445*	(0.245)	-0.323	(0.220)	0.295	(0.260)
+8	0.184**	(0.093)	0.418**	(0.204)	0.510*	(0.266)	-0.275	(0.213)	0.365	(0.259)
+9	0.173**	(0.088)	0.392*	(0.203)	0.482**	(0.196)	-0.140	(0.223)	0.237	(0.284)
+10	0.168*	(0.086)	0.400*	(0.210)	0.372*	(0.208)	-0.312	(0.216)	0.156	(0.286)
+11	0.176**	(0.089)	0.413**	(0.210)	0.382*	(0.221)	-0.351	(0.237)	0.046	(0.306)
+12	0.185**	(0.094)	0.428**	(0.217)	0.488**	(0.228)	-0.218	(0.221)	0.028	(0.299)
+13	0.202**	(0.101)	0.452**	(0.192)	0.505**	(0.228)	-0.231	(0.217)	0.207	(0.314)
+14	0.219**	(0.108)	0.495**	(0.204)	0.527**	(0.242)	-0.180	(0.213)	0.334	(0.348)
≥15	0.254**	(0.121)	0.548***	(0.201)	0.680***	(0.255)	-0.206	(0.223)	0.099	(0.357)
Long-term effect	0.268*	(0.142)	0.443***	(0.162)	0.558*	(0.293)	-0.143	(0.241)	-0.040	(0.438)
# Observations	42559		41772		37237		34320		32345	
# Municipalities	1039		1010		921		825		794	
Year FE	Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. We exclude control municipalities located within 5 km of the nearest treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 1.C.14: Cumulative effect before/after access (<15 km), including never treated between 15 km and 30 km, excluding municipalities close to treated units (<5 km)

	Number of Firms (1)		Births (2)		Deaths (3)		Relocations In (4)		Relocations Out (5)	
≤8	0.010	(0.056)	-0.013	(0.296)	0.223	(0.150)	-0.317	(0.208)	0.569*	(0.322)
-7	-0.073	(0.102)	-0.026	(0.307)	-0.011	(0.219)	-0.470*	(0.280)	0.115	(0.312)
-6	-0.000	(0.072)	0.415**	(0.187)	0.301**	(0.131)	-0.058	(0.220)	0.181	(0.279)
-5	0.010	(0.058)	0.067	(0.223)	0.262*	(0.142)	0.023	(0.217)	0.110	(0.276)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.015	(0.020)	0.262	(0.171)	0.094	(0.241)	-0.128	(0.212)	0.340	(0.256)
-2	0.043	(0.036)	0.164*	(0.086)	0.528***	(0.185)	-0.316	(0.235)	0.339	(0.307)
-1	0.059	(0.047)	0.275**	(0.114)	0.174	(0.166)	-0.340*	(0.180)	0.437*	(0.253)
+0	0.097*	(0.056)	0.254**	(0.116)	0.160	(0.151)	0.169	(0.228)	0.237	(0.277)
+1	0.127*	(0.070)	0.349***	(0.133)	0.339*	(0.201)	-0.169	(0.198)	-0.055	(0.287)
+2	0.155*	(0.082)	0.418***	(0.113)	0.325*	(0.172)	-0.275	(0.182)	0.029	(0.304)
+3	0.180*	(0.094)	0.518***	(0.131)	0.390**	(0.158)	-0.384*	(0.203)	0.361	(0.258)
+4	0.197*	(0.102)	0.440***	(0.152)	0.722**	(0.285)	-0.317*	(0.186)	0.281	(0.288)
+5	0.222**	(0.109)	0.364***	(0.119)	0.685*	(0.367)	-0.302	(0.196)	0.350	(0.359)
+6	0.244**	(0.116)	0.505***	(0.157)	1.013***	(0.368)	-0.143	(0.175)	0.528*	(0.300)
+7	0.239**	(0.107)	0.431**	(0.204)	0.544**	(0.263)	-0.308	(0.210)	0.344	(0.256)
+8	0.241**	(0.101)	0.465**	(0.221)	0.616**	(0.281)	-0.271	(0.198)	0.402	(0.258)
+9	0.232**	(0.096)	0.432**	(0.217)	0.594***	(0.199)	-0.117	(0.211)	0.298	(0.279)
+10	0.229**	(0.094)	0.437*	(0.226)	0.493**	(0.204)	-0.298	(0.199)	0.216	(0.277)
+11	0.238**	(0.096)	0.453**	(0.226)	0.505**	(0.222)	-0.327	(0.225)	0.120	(0.310)
+12	0.248**	(0.102)	0.458**	(0.232)	0.603**	(0.237)	-0.202	(0.206)	0.113	(0.292)
+13	0.265**	(0.108)	0.479**	(0.206)	0.621***	(0.237)	-0.206	(0.202)	0.307	(0.303)
+14	0.282**	(0.115)	0.526**	(0.219)	0.646**	(0.252)	-0.159	(0.199)	0.436	(0.337)
≥15	0.318**	(0.126)	0.572***	(0.214)	0.807***	(0.264)	-0.185	(0.210)	0.216	(0.343)
Long-term effect	0.328**	(0.144)	0.460***	(0.167)	0.675**	(0.306)	-0.109	(0.228)	0.096	(0.418)
# Observations	50000		49121		43525		38907		36863	
# Municipalities	1267		1234		1094		944		915	
Year FE	Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. We include rural (never-connected) municipalities with an access between 15 km and 30 km in the control group, but we exclude control municipalities located within 5 km of the nearest treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 1.C.15: Cumulative effect before/after access (<15 km), excluding municipalities close to treated units (<10 km)

	Number of Firms (1)		Births (2)		Deaths (3)		Relocations In (4)		Relocations Out (5)	
≤8	0.121	(0.096)	-0.124	(0.371)	0.256	(0.192)	-0.167	(0.322)	0.296	(0.342)
-7	-0.084	(0.160)	-0.034	(0.361)	-0.152	(0.297)	-0.641*	(0.343)	0.248	(0.332)
-6	0.002	(0.118)	0.456**	(0.227)	0.246	(0.222)	0.077	(0.215)	0.113	(0.329)
-5	0.007	(0.097)	0.059	(0.271)	0.296	(0.273)	0.029	(0.244)	0.174	(0.364)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.023	(0.033)	0.390**	(0.198)	-0.241	(0.285)	-0.397*	(0.224)	-0.027	(0.190)
-2	0.069	(0.045)	0.187**	(0.081)	0.457*	(0.237)	-0.664**	(0.287)	0.352	(0.293)
-1	0.068	(0.058)	0.246**	(0.113)	0.060	(0.155)	-0.371	(0.231)	0.277	(0.184)
+0	0.116*	(0.068)	0.267**	(0.133)	0.221	(0.151)	-0.071	(0.183)	0.320	(0.296)
+1	0.140*	(0.083)	0.357**	(0.148)	0.436**	(0.214)	-0.444*	(0.235)	-0.002	(0.332)
+2	0.165*	(0.096)	0.445***	(0.126)	0.356*	(0.203)	-0.575**	(0.241)	0.125	(0.333)
+3	0.184*	(0.108)	0.535***	(0.131)	0.447***	(0.170)	-0.691**	(0.272)	0.465*	(0.277)
+4	0.197*	(0.117)	0.474***	(0.169)	0.747**	(0.303)	-0.645**	(0.273)	0.346	(0.331)
+5	0.216*	(0.125)	0.405***	(0.135)	0.728*	(0.379)	-0.630***	(0.244)	0.498	(0.374)
+6	0.234*	(0.132)	0.546***	(0.147)	1.029***	(0.373)	-0.460*	(0.239)	0.604*	(0.347)
+7	0.225*	(0.123)	0.478**	(0.203)	0.589**	(0.281)	-0.669**	(0.294)	0.454*	(0.263)
+8	0.221*	(0.117)	0.512**	(0.218)	0.650**	(0.309)	-0.612**	(0.281)	0.516*	(0.287)
+9	0.210*	(0.112)	0.496**	(0.218)	0.611***	(0.230)	-0.486*	(0.296)	0.402	(0.280)
+10	0.205*	(0.109)	0.508**	(0.223)	0.508**	(0.247)	-0.673**	(0.306)	0.356	(0.284)
+11	0.213*	(0.110)	0.527**	(0.221)	0.510*	(0.269)	-0.719**	(0.292)	0.250	(0.314)
+12	0.223*	(0.116)	0.547**	(0.226)	0.631**	(0.257)	-0.609*	(0.323)	0.249	(0.291)
+13	0.240*	(0.124)	0.578***	(0.202)	0.648***	(0.251)	-0.631*	(0.328)	0.441	(0.313)
+14	0.258**	(0.131)	0.621***	(0.214)	0.677**	(0.265)	-0.591*	(0.330)	0.614*	(0.314)
≥15	0.293**	(0.146)	0.693***	(0.207)	0.829***	(0.264)	-0.650*	(0.345)	0.420	(0.325)
Long-term effect	0.311	(0.201)	0.549***	(0.176)	0.761**	(0.349)	-0.592*	(0.353)	0.340	(0.313)
# Observations	39975		39210		34920		32251		30348	
# Municipalities	1037		1005		915		815		785	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. We exclude control municipalities located within 10 km of the nearest treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 1.C.16: Cumulative effect before/after access (<15 km), including never treated between 15 km and 30 km, excluding municipalities close to treated units (<10 km)

	Number of Firms		Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)	
≤8	0.105	(0.095)	-0.132	(0.372)	0.219	(0.185)	-0.193	(0.325)	0.290	(0.371)
-7	-0.088	(0.160)	-0.030	(0.364)	-0.158	(0.280)	-0.662*	(0.342)	0.197	(0.359)
-6	0.000	(0.117)	0.467**	(0.220)	0.219	(0.214)	0.071	(0.222)	0.070	(0.349)
-5	0.007	(0.096)	0.066	(0.271)	0.246	(0.272)	-0.003	(0.253)	0.109	(0.374)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.025	(0.033)	0.401**	(0.193)	-0.232	(0.265)	-0.406*	(0.211)	-0.106	(0.201)
-2	0.074	(0.045)	0.206**	(0.081)	0.469*	(0.240)	-0.700**	(0.298)	0.265	(0.323)
-1	0.078	(0.059)	0.274**	(0.112)	0.062	(0.157)	-0.349	(0.231)	0.185	(0.195)
+0	0.132*	(0.069)	0.287**	(0.132)	0.224	(0.161)	-0.078	(0.184)	0.212	(0.314)
+1	0.158*	(0.084)	0.381***	(0.148)	0.454**	(0.229)	-0.455*	(0.237)	-0.108	(0.356)
+2	0.184*	(0.097)	0.465***	(0.126)	0.388*	(0.214)	-0.572**	(0.238)	0.011	(0.358)
+3	0.205*	(0.110)	0.561***	(0.134)	0.472***	(0.176)	-0.688**	(0.270)	0.343	(0.295)
+4	0.219*	(0.119)	0.499***	(0.166)	0.781**	(0.323)	-0.636**	(0.271)	0.220	(0.360)
+5	0.241*	(0.127)	0.433***	(0.134)	0.759*	(0.402)	-0.623**	(0.248)	0.365	(0.405)
+6	0.259*	(0.134)	0.577***	(0.152)	1.067***	(0.394)	-0.450*	(0.235)	0.473	(0.368)
+7	0.252**	(0.125)	0.510**	(0.203)	0.631**	(0.296)	-0.659**	(0.288)	0.326	(0.280)
+8	0.251**	(0.118)	0.543**	(0.218)	0.691**	(0.326)	-0.603**	(0.279)	0.375	(0.307)
+9	0.241**	(0.113)	0.529**	(0.218)	0.660***	(0.241)	-0.472	(0.289)	0.272	(0.298)
+10	0.237**	(0.110)	0.539**	(0.225)	0.561**	(0.258)	-0.655**	(0.299)	0.227	(0.302)
+11	0.246**	(0.112)	0.559**	(0.223)	0.565**	(0.283)	-0.701**	(0.289)	0.111	(0.341)
+12	0.256**	(0.117)	0.579**	(0.229)	0.681**	(0.267)	-0.587*	(0.316)	0.119	(0.310)
+13	0.274**	(0.125)	0.611***	(0.205)	0.702***	(0.264)	-0.611*	(0.320)	0.311	(0.332)
+14	0.292**	(0.131)	0.655***	(0.217)	0.732***	(0.278)	-0.572*	(0.321)	0.481	(0.332)
≥15	0.329**	(0.147)	0.725***	(0.209)	0.890***	(0.276)	-0.627*	(0.337)	0.283	(0.343)
Long-term effect	0.345*	(0.201)	0.578***	(0.179)	0.834**	(0.356)	-0.558	(0.345)	0.237	(0.322)
# Observations	43087		42287		37142		33533		31844	
# Municipalities	1212		1174		1014		868		846	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. We include rural (never-connected) municipalities with an access between 15 km and 30 km in the control group, but we exclude control municipalities located within 10 km of the nearest treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

Estimation without single-year firms

Table 1.C.17: Cumulative effect before/after access (<15 km) - w/o one-year firms

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.119	(0.146)	0.030	(0.038)	-0.037	(0.225)	0.269	(0.173)	-0.566***	(0.144)	0.328	(0.224)
-7	-0.089	(0.064)	-0.050	(0.061)	-0.066	(0.223)	0.082	(0.157)	-0.378**	(0.175)	-0.021	(0.241)
-6	-0.069	(0.053)	-0.011	(0.044)	0.226	(0.143)	0.101	(0.134)	-0.319*	(0.187)	-0.076	(0.207)
-5	-0.055	(0.041)	-0.000	(0.036)	0.006	(0.169)	0.079	(0.125)	-0.045	(0.134)	0.057	(0.168)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.020	(0.014)	0.018	(0.014)	0.111	(0.129)	-0.024	(0.215)	-0.120	(0.149)	0.251	(0.189)
-2	0.037	(0.029)	0.035	(0.024)	0.046	(0.090)	0.444***	(0.132)	-0.227	(0.142)	0.175	(0.228)
-1	0.047	(0.039)	0.040	(0.032)	0.082	(0.127)	0.121	(0.156)	-0.232	(0.146)	0.183	(0.221)
+0	0.068	(0.048)	0.060	(0.040)	0.129	(0.079)	0.018	(0.169)	0.269	(0.231)	0.171	(0.225)
+1	0.098	(0.063)	0.087*	(0.053)	0.213*	(0.109)	0.250	(0.162)	-0.044	(0.156)	-0.164	(0.214)
+2	0.116	(0.077)	0.113*	(0.064)	0.214**	(0.104)	0.138	(0.164)	-0.156	(0.127)	-0.050	(0.215)
+3	0.137	(0.090)	0.134*	(0.074)	0.381***	(0.112)	0.269**	(0.136)	-0.243	(0.149)	0.291	(0.199)
+4	0.147	(0.097)	0.144*	(0.076)	0.231*	(0.135)	0.500**	(0.239)	-0.188	(0.124)	0.133	(0.239)
+5	0.152	(0.107)	0.156**	(0.079)	0.137	(0.132)	0.314	(0.251)	-0.160	(0.151)	0.164	(0.262)
+6	0.166	(0.117)	0.168**	(0.081)	0.198	(0.157)	0.748**	(0.297)	0.017	(0.118)	0.418	(0.277)
+7	0.168	(0.125)	0.167**	(0.073)	0.213	(0.149)	0.344*	(0.185)	-0.152	(0.166)	0.132	(0.232)
+8	0.176	(0.137)	0.172**	(0.070)	0.302	(0.200)	0.502**	(0.230)	-0.062	(0.153)	0.310	(0.209)
+9	0.169	(0.148)	0.165**	(0.069)	0.237	(0.185)	0.418***	(0.131)	0.049	(0.151)	0.162	(0.234)
+10	0.168	(0.158)	0.163**	(0.070)	0.220	(0.173)	0.338*	(0.182)	-0.121	(0.139)	0.066	(0.230)
+11	0.173	(0.168)	0.169**	(0.073)	0.263	(0.195)	0.322*	(0.181)	-0.142	(0.177)	0.033	(0.231)
+12	0.173	(0.180)	0.175**	(0.079)	0.251	(0.207)	0.376**	(0.177)	-0.001	(0.151)	0.001	(0.237)
+13	0.185	(0.188)	0.189**	(0.083)	0.271	(0.180)	0.436***	(0.159)	-0.019	(0.155)	0.193	(0.248)
+14	0.197	(0.198)	0.203**	(0.090)	0.255	(0.177)	0.391**	(0.168)	0.002	(0.146)	0.314	(0.277)
≥15	0.214	(0.221)	0.236**	(0.101)	0.349*	(0.197)	0.631***	(0.178)	0.021	(0.152)	0.139	(0.274)
Long-term effect	0.282	(0.284)	0.253**	(0.108)	0.289*	(0.155)	0.607***	(0.193)	0.129	(0.182)	0.166	(0.274)
# Observations	45672		45672		43391		39560		36916		35088	
# Municipalities	1038		1038		1014		920		839		816	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	No		Yes		Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10. We exclude observations appearing in a single period.

Net births and deaths estimation

Table 1.C.18: Cumulative effect before/after access (<15 km) - Net births and deaths

	Births (1)		Net Births (2)		Deaths (3)		Net Deaths (4)	
≤8	0.020	(0.234)	-0.023	(0.349)	0.217*	(0.124)	0.635**	(0.262)
-7	-0.049	(0.245)	0.021	(0.373)	0.000	(0.160)	-0.039	(0.220)
-6	0.256	(0.157)	0.353	(0.236)	0.072	(0.108)	0.336	(0.214)
-5	0.062	(0.186)	0.145	(0.280)	0.114	(0.112)	0.194	(0.205)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.175	(0.145)	0.234	(0.194)	0.016	(0.174)	-0.045	(0.358)
-2	0.122	(0.089)	0.155	(0.155)	0.421***	(0.125)	0.829***	(0.203)
-1	0.161	(0.112)	0.230	(0.169)	0.137	(0.102)	0.329	(0.212)
+0	0.172*	(0.094)	0.300**	(0.151)	0.080	(0.112)	-0.051	(0.243)
+1	0.265**	(0.109)	0.387**	(0.191)	0.271**	(0.127)	0.308	(0.233)
+2	0.336***	(0.091)	0.491***	(0.175)	0.227*	(0.118)	0.713**	(0.333)
+3	0.424***	(0.113)	0.616***	(0.207)	0.304***	(0.113)	0.401*	(0.226)
+4	0.348**	(0.136)	0.552**	(0.224)	0.609***	(0.217)	0.462*	(0.252)
+5	0.265***	(0.102)	0.288	(0.265)	0.563*	(0.300)	0.379	(0.299)
+6	0.395***	(0.136)	0.309	(0.302)	0.890***	(0.298)	1.258***	(0.476)
+7	0.322*	(0.182)	0.519	(0.343)	0.423**	(0.188)	0.584	(0.397)
+8	0.351*	(0.199)	0.593*	(0.360)	0.487**	(0.206)	0.548	(0.435)
+9	0.317	(0.197)	0.478*	(0.283)	0.469***	(0.127)	0.379	(0.306)
+10	0.322	(0.206)	0.553	(0.338)	0.362***	(0.136)	0.437	(0.374)
+11	0.334	(0.208)	0.579	(0.393)	0.373***	(0.145)	0.796**	(0.362)
+12	0.340	(0.215)	0.481	(0.329)	0.473***	(0.157)	0.390	(0.350)
+13	0.357*	(0.190)	0.567	(0.359)	0.492***	(0.154)	0.499	(0.399)
+14	0.400**	(0.203)	0.684	(0.421)	0.514***	(0.164)	0.618	(0.447)
≥15	0.438**	(0.204)	0.690*	(0.397)	0.676***	(0.174)	0.717*	(0.402)
Long-term effect	0.366**	(0.159)	0.536*	(0.324)	0.655***	(0.203)	0.703*	(0.368)
# Observations	44696		44624		40377		37351	
# Municipalities	1023		1022		939		911	
Year FE	Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes	

Notes: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

Urban sprawl - Firms

To complement our main findings, we conduct an additional analysis including all municipalities. We face endogeneity problems in those regressions because we include both centers and suburban municipalities that planners usually target because of their economic importance.

Table 1.C.19 shows how the road construction in treated municipalities affects firm dynamics, depending on the distance to the city center. For example, the first line of the table illustrates the effect of an urban center getting connected early compared to the other centers that connect later. The more interesting results are those for municipalities outside of centers. Similar to the trends shown in Table 1.B.2, we find that the centers saw a decrease in economic activity. Fewer firms are being created, and fewer are moving in or out of the centers. Overall, they see a decline in the number of firms. Between 1 and 20 km from the centers, we begin to see positive effects on firm creation. The coefficients are positive for the stock of firms and the number of firm deaths. Relocation coefficients remain negative. We find similar results for municipalities located within 20 to 40 km of the centers. There are massive changes in municipalities more than 40 km away, as we observe increases in all variables simultaneously.

Table 1.C.19: Urban sprawl - firm outcomes

	# of Firms	**Creations**	**Deaths**	**Relocations In**	**Relocations Out**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Long-term effect					
Center	-0.132	-0.629***	-0.001	-1.073***	-0.704***
	(0.109)	(0.210)	(0.173)	(0.217)	(0.172)
1-20 km	0.144	0.295*	0.259	-0.104	-0.100
	(0.107)	(0.170)	(0.220)	(0.144)	(0.185)
21-40 km	0.136	0.262*	0.357	0.230	-0.310
	(0.119)	(0.154)	(0.250)	(0.268)	(0.454)
> 40 km	0.794***	1.105***	1.849***	0.733*	0.570***
	(0.146)	(0.223)	(0.281)	(0.389)	(0.215)
# Observations	81976	80965	75465	72424	69617
# Municipalities	1865	1847	1755	1646	1619

Notes: All specifications include municipality fixed effects, time fixed effects, and municipality-specific time trends. The sample includes all municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

Railways

We also interact our access variables with a variable indicating if the municipality has a railway station. By doing so, we examine whether the effects of highways differ between municipalities with and without a railway station. The increase in the number of firms is larger in municipalities without a railway station. We find a positive effect in municipalities with railway stations, although it is not statistically significant. This is unsurprising, as we municipalities with railways are already better connected via another mode of transportation.

When we look at the flows behind this stock effect, we can see that the effects on firm creation are larger in municipalities with a train station. Again, we do find a positive but statistically insignificant effect in municipalities without any railway station. Regarding deaths, we have the opposite result. Firms are closing more often in municipalities without a train station.

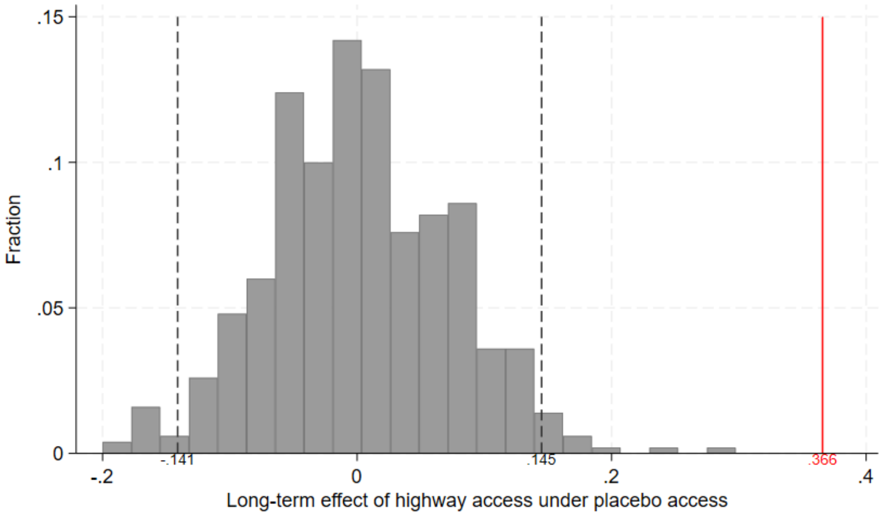
Table 1.C.20: Heterogeneity - Railways

	Stock (1)		Births (2)		Deaths (3)		Relocation In (4)		Relocation Out (5)	
Long-term effect										
without railway station	0.335***	(0.094)	0.271	(0.239)	1.091***	(0.244)	0.337	(0.384)	0.213	(0.434)
with railway station	0.180	(0.132)	0.380*	(0.203)	0.360	(0.278)	-0.019	(0.254)	0.170	(0.276)
# Observations	45720		44698		40377		36916		34648	
# Municipalities	1041		1023		939		839		816	

Notes: Include municipality fixed effects, time fixed effects, and municipal time trends. The sample includes all non-urban municipalities within 15 km of an access. The standard errors, in parentheses, are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

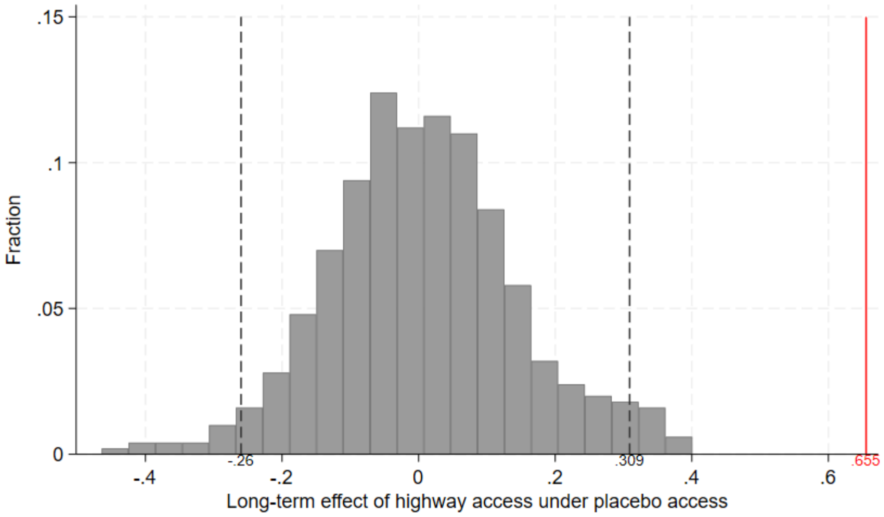
Placebos - Births & deaths

Figure 1.C.5: The long-term effect on the creation of firms - Placebo tests



Notes: Resulting histogram from our placebo tests where we randomize the treatment 1000 times. We use the same regressions as in our main analysis, focusing on the long-term effects. The dotted vertical lines show the 95% confidence interval. The vertical axis shows the share of long-term effects falling into a certain range. The red line is the long-term effect on the number of created firms from Table 1.C.4, specification (3).

Figure 1.C.6: The long-term effect on the death of firms - Placebo tests

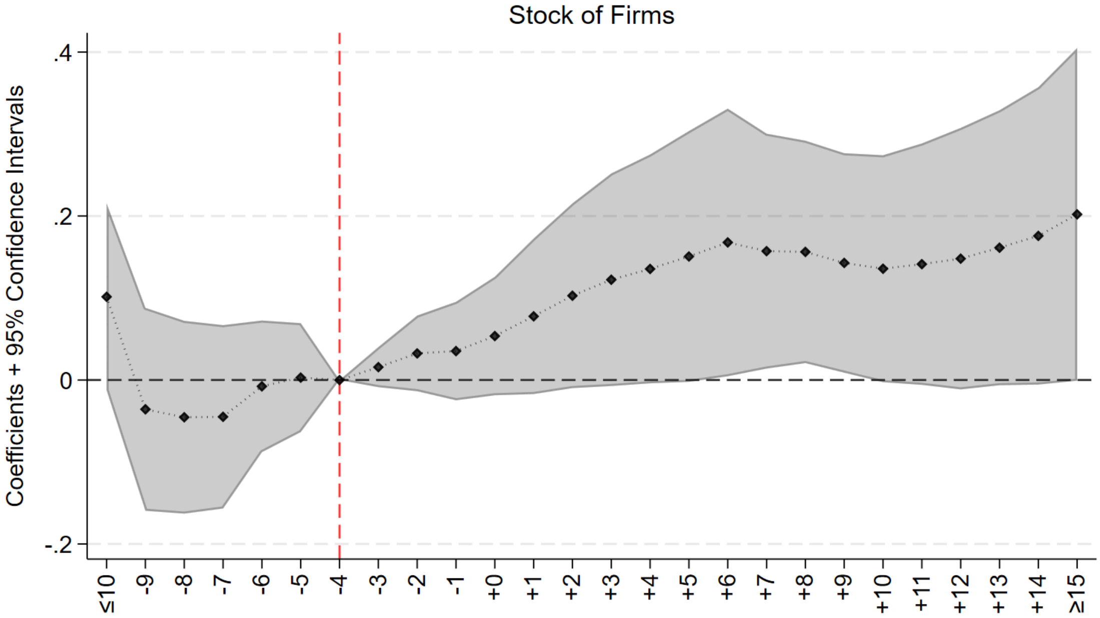


Notes: Resulting histogram from our placebo tests where we randomize the treatment 500 times. We use the same regressions as in our main analysis, focusing on the long-term effects. The dotted vertical lines show the 95% confidence interval. The vertical axis shows the share of long-term effects falling into a certain range. The red line is the long-term effect on the number of disappearing firms from Table 1.C.4, specification (4).

Longer pre-treatment period

One possible concern with the pretrends in our baseline results is that periods before -7 could continue to diverge from 0. To test if that is the case, we increase the number of pre-treatment periods to 9 before binning earlier periods. This comes at the cost of precision, but allows us to observe if coefficients in -8 and -9 continue to diverge. As shown in Figure 1.C.7, this is not the case.

Figure 1.C.7: Cumulative effect before/after access (<15 km) - Extended pretreatment period



Notes: The figure presents our full specification results estimating Equation (1.1). We extend the pretreatment periods by 1 year to see whether the coefficients at -9 continue the downward trend we observed in the baseline results. This specification includes year and unit fixed effects, as well as municipal-specific time trends. Standard errors are clustered at the district level. The sample includes all non-urban municipalities within 15 km of an access. Table 1.C.4 in the Appendix provides all the details.

Chapter 2

Place-Based Policy, Firm Dynamics, Local and Spatial Effects: Evidence from Swiss Firm Data, 1960-1993*

2.1 Introduction

Place-based and industrial policies are once again at the center of economic policy debates worldwide. Governments invest unprecedented sums to boost struggling industries, incentivize innovation, and address persistent regional disparities. Most notably, the U.S. administration under President Biden has made place-based industrial policy a central pillar of its economic strategy. The federal government allocated roughly 80 billion dollars through programs such as the CHIPS and Science Act, the Inflation Reduction Act (IRA), the American Rescue Plan Act (ARP), and the Infrastructure Investment and Jobs Act (IIJA) (McGahey 2023).

Place-based policies aim to shape the geographic distribution of economic activity by directing investment toward disadvantaged areas. However, the effectiveness and unintended consequences of such interventions remain unclear. Particularly regarding who benefits, how persistent the effects are, and the channels through which these policies operate. Switzerland provides a useful setting to study these issues. In response to the oil shock and structural crises of the 1970s, the Swiss government introduced two major regional policy interventions: the 1974 Law on Infrastructure Investments in Mountain Areas (also known as LIM) and the 1978 Federal Decree on the Promotion of Economically Endangered Areas (also known as Lex Bonny or simply Bonny). These policies targeted economically vulnerable municipalities. The government offered generous packages of infrastructure investments, subsidized loans, guarantees, and tax breaks. Despite their historical importance, rigorous evidence on their long-run effects, especially regarding firm dynamics, remains scarce.

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*I am grateful for the many comments and feedback from Mark Schelker. I also thank Emilie Dousse, Daniel Rocha, and Julie Uldry for their fantastic and careful work in the data collection and data cleaning process. This project benefited from constructive comments and helpful feedback from participants at the Cookie Seminar at the University of Fribourg.

This chapter studies the long-term effects of Lex Bonny eligibility on firm activity at the municipal level. We examine the number of firms, as well as firm creation, exit, and relocation. The Lex Bonny was explicitly designed to stimulate local economic activity by attracting firms to disadvantaged regions through investment subsidies and tax relief, under the expectation that firm location decisions would translate into job creation and economic growth. Using firm-level data, we can directly assess whether the policy achieved its primary objective: increasing the presence of firms in eligible municipalities. While the main focus is on firms, we complement the analysis by investigating the effects on workers and taxpayers.

We address several questions. Do subsidies increase the number of firms in targeted regions? Does the policy reorganize existing economic activity across space? Do positive spillovers arise in nearby municipalities? To answer these questions, we construct a municipal panel dataset covering the period from the 1960s to 1993. The dataset combines historical and administrative records on the universe of Swiss corporations (hereafter, firms), workers, taxpayers, and municipal characteristics. We exploit spatial variation in policy eligibility and estimate several difference-in-differences (DiD) specifications within a spatial framework. Because treatment is defined as municipal eligibility rather than firm participation, our estimates measure intention-to-treat effects[2]

Our empirical strategy combines several DiD approaches. First, we estimate a dynamic event-study that compares Bonny municipalities with municipalities covered by neither Bonny nor LIM. Second, we apply a concentric-ring design that progressively excludes nearby control municipalities. Third, we estimate spatial DiD models that explicitly account for spillovers. We adopt this sequence because place-based policies can violate key assumptions of standard DiD designs. Moving from a standard DiD to designs that account for spillovers allows us to recover interpretable dynamic effects while balancing violations of the stable unit treatment value assumption (henceforth SUTVA) against the loss of comparable units.

Most studies evaluate place-based policies through employment or wage outcomes. This chapter instead focuses on firm dynamics and spatial reallocation, particularly over long horizons. We contribute to the literature in several ways. First, we examine firm entry, exit, and relocation rather than only labor-market outcomes. By doing so, we assess whether the policy's main goal of stimulating firm activity is achieved. Second, we address spatial spillovers and displacement by combining dynamic DiD

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[2] Strictly speaking, municipalities are covered by the policy rather than directly treated, since the policy targets firms within eligible municipalities (or willing to move in those municipalities). For simplicity, we sometimes refer to eligible municipalities as treated municipalities (and vice versa) throughout the chapter.

estimates with distance-based and donut-style specifications. Third, we show that firm responses concentrate among small firms. Finally, we combine firm outcomes with data on workers and taxpayers. This allows us to test whether increases in firm activity translate into improvements in local labor markets. Overall, the analysis provides a more nuanced assessment of whether place-based policies generate net local gains, spatial spillovers, or primarily reallocate economic activity.

Our results show that Lex Bonny had a large, positive, and persistent effect on the number of firms in eligible municipalities. Using dynamic DiD estimates, we document a sustained increase in the number of firms following policy implementation, with no evidence of differential pre-trends. Decomposing these effects reveals that the increase is driven primarily by higher firm entry, while firm exits and relocations play a more limited role. Spatial DiD estimates further indicate that these effects are strong in Bonny municipalities and diffuse to nearby untreated ones. We find some evidence of positive spillovers to nearby untreated areas, and some patterns are consistent with local reallocation. Taken together, the results suggest that the Lex Bonny succeeded in stimulating firm creation in targeted areas, but that its economic impacts remained spatially concentrated.

Evidence on workers and taxpayers provides additional insight. Firm dynamics respond strongly to the policy, but other outcomes show weaker effects. The increase in the number of firms does not translate into higher local employment. We also find heterogeneous effects across the tax distribution. The number of taxpayers increases for the bottom 50% of the distribution, whereas it decreases for the top 50%. In addition, most firm responses come from small firms, which typically generate few jobs locally. Together, these patterns suggest that Lex Bonny primarily reshaped the geography and composition of firms in eligible municipalities, but did not substantially increase employment or attract high-income taxpayers. However, we find evidence of spillovers to municipalities outside the Lex Bonny coverage area.

The remainder of the chapter proceeds as follows. Section 2.2 provides historical background on Switzerland's regional policy landscape, detailing the design and implementation of the Lex Bonny. Section 2.3 outlines the theoretical mechanisms through which place-based policies may affect firm dynamics, commuting patterns, and demographic change, and reviews the related literature. Section 2.4 describes the data sources, variable construction, treatment definitions, and the descriptive statistics. Section 2.5 presents our empirical strategies, combining various DiD approaches. Section 2.6 reports the results, while Section 2.7 provides robustness checks. Section 2.8 discusses the implications of our findings for the overall policy evaluation of Lex Bonny, before section 2.9 concludes.

2.2 Background and institutional context

2.2.1 Early regional policy

Switzerland's earlier regional policies were more sector-specific. In the late 19th century, the federal government first intervened to help the agricultural sector. The Federal Agriculture Act of 1893 provided price supports, subsidies, and protective tariffs. Much later, the Federal Hotel Credit Act of 1966 targeted the tourism sector by offering low-interest loans. These measures, while important for local economies, remained tied to specific sectors and did not aim at broad industrial diversification or economic growth. The approach changed with the place-based policies in the 1970s.

By the early 1970s, several structural shocks had created uneven regional pressures. The collapse of the watchmaking industry, rising unemployment in mono-industrial areas, and the oil shock hit some regions particularly hard, while others were less affected. In response, the Swiss federal government shifted toward territorially targeted policies aiming at structural transformation and economic diversification of struggling areas. This shift led to the implementation of two major policies: the 1974 Law on Infrastructure Investments in Mountain Areas (LIM) and the 1978 Federal Decree on the Promotion of Economically Endangered Areas (Lex Bonny). Unlike earlier measures, these policies combined multiple instruments, ranging from infrastructure and public goods to direct firm-level incentives and tax relief, within a broader regional development strategy.

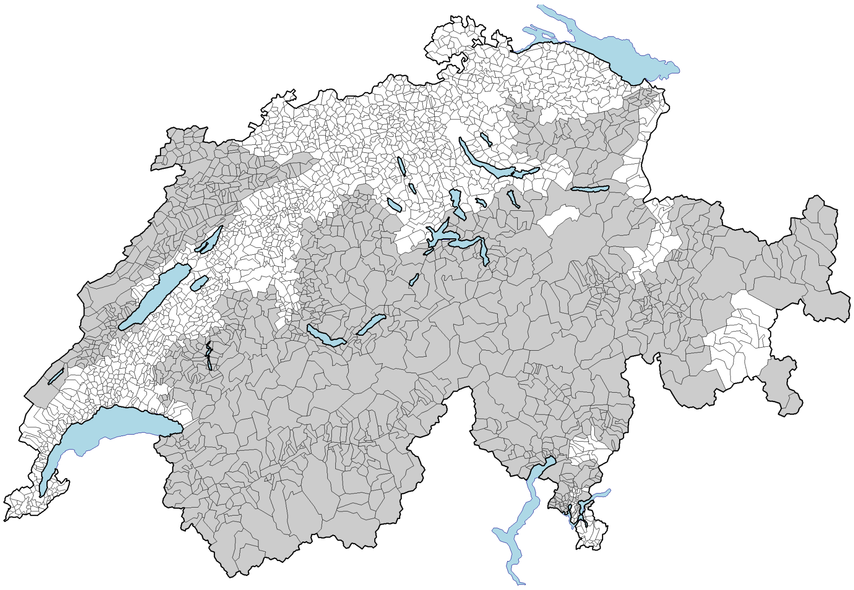
2.2.2 The 1974 Federal Law on Aid for Infrastructure Investments in Mountain Areas

The Federal Law on Aid for Infrastructure Investments in Mountain Areas, adopted in December 1974, was the first large-scale regional policy implemented in Switzerland. Many mountain municipalities faced poor accessibility, limited public services, and a narrow economic base with mostly low-productivity activities. These challenges were exacerbated by migration to urban areas, which further reduced the resources available for local development.

LIM aimed to strengthen the economic potential and living conditions of mountain regions through modern infrastructure. The law aimed to improve local amenities and create the conditions needed to both retain residents and attract new economic activity. To achieve those goals, municipalities voluntarily formed regions for which an economic development program would be planned.

The law defined its scope geographically, targeting municipalities classified as mountain areas based on altitude, topography, and economic characteristics[3]. The Confederation funded projects only if cantons and municipalities also contributed financially. Unlike Lex Bonny, LIM did not target specific sectors or individual firms. Instead, it aimed to benefit all firms and residents in eligible areas.

Figure 2.1: Map of the municipalities covered by LIM



Notes: The light grey areas highlight the municipalities eligible for LIM subsidies. The map shows the state in 1993. The number and coverage of LIM regions did not change after 1987. Figure 2.A.3 in the Appendix shows official LIM coverage maps for comparison.

The LIM operated through co-financed public investment projects. The Confederation provided subsidized loans, guarantees, and, in some cases, direct grants for eligible infrastructure. Projects included transport networks, water and energy systems, telecommunications, health and education facilities, and tourism infrastructure. The number of LIM regions did not change between 1987 and the program's abolition in 2007. Their territorial boundaries have undergone only minor modifications due to municipal mergers (Swiss Federal Statistical Office 2000a). Figure 2.1 shows the geographical repartition of the municipalities included in LIM regions at the end of

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[3] LIM regions have certain criteria to fulfill: 1) Regions must be voluntarily formed on the basis of close functional relationships. In addition to economic interdependence, the intensity of existing exchanges and cooperations, as well as cultural and institutional affinities, are also taken into account. 2) To be included in a LIM region, municipalities must have rates below 80% of the national average for at least two of the following three indicators: population growth, age structure, and income levels. 3) At least 20% of the region's population must live at an altitude of more than 1,000 meters. 4) Initially, regions were required to have a minimum population of 20,000 inhabitants. This was relaxed later to allow the creation of smaller regions in terms of population.

our sample in 1993.

Government reports often portray the policy as successful. Yet they usually base that assessment on the number of subsidized projects rather than on economic outcomes. This measure is not very informative about the policy's actual impact. By 2004, the 54 LIM regions had received about CHF 2.3 billion in subsidized loans to finance investment projects. In total, they launched roughly 8,500 projects with an estimated value of CHF 19.3 billion (Swiss Federal Statistical Office 2000a).

2.2.3 The 1978 Federal Decree on the Promotion of Economically Endangered Areas (Lex Bonny)

The Federal Decree of 6 October 1978 was enacted after the mid-1970s recession. Several Swiss regions, particularly mono-industrial, experienced sudden job losses of 20% or more, persistent unemployment, and out-migration. The Federal Decree encourages the implementation of privately financed projects to create and maintain jobs in underperforming regions by granting financial assistance and tax relief to firms. The policy also aimed to diversify the economic activities in regions that suffered heavily from the recession. The idea was to improve the resilience of those regions to negative exogenous shocks.

The policy pursued these objectives through a combination of subsidized finance and corporate tax relief. The main financial tool consisted of federal guarantees covering up to one-third of the total investment cost, subject to the firm providing sufficient own capital, commercial bank participation at market terms, and shared risk coverage by the canton. In some cases, these guarantees could be complemented by interest rate subsidies of up to one-quarter of the prevailing market rate on the guaranteed portion, also co-financed by the canton. In parallel, projects receiving such support could qualify for direct federal corporate tax reductions, provided the canton granted relief equivalent to or greater than that.

Eligibility was restricted to industrial and artisanal firms located in designated economically endangered regions. Designation as an "economically endangered" municipality required that more than 20% of secondary-sector employment be concentrated in a single industrial branch, combined with at least two of the following conditions: (i) a decline in resident population exceeding 1% over several years; (ii) a comparable decline in the active population; (iii) an unemployment rate exceeding 1% for full-time workers; and (iv) an unemployment rate exceeding 3% for part-time workers. Within these areas, priority was given to projects that expanded or modernized production facilities, introduced new products or processes, or established new activities in industries underrepresented in the local economy.

This focus on new or expanding firms meant that a small number of incumbents were eligible for direct support, raising questions about potential crowding-out effects on the other firms. However, among the projects effectively financed, slightly more than half involved innovation or diversification investments by existing firms. Another feature of the policy is that the subsidies were not tied to any obligation to hire local workers[4] This feature contrasts with the policy's stated objective. The first article of the decree explicitly stated that the goal was to "*encourage private sector projects aimed at creating and maintaining jobs in the regions*."

Together, the regions covered by the decree represent around 11% of Switzerland's jobs and resident population. In 1979, when the policy was implemented, 459 municipalities were covered by the program. Those municipalities are shown in Figure 2.2. Any firms willing to invest in those locations could apply for subsidies under the Lex Bonny. Eleven cantons, namely Vaud, Neuchâtel, Bern, Jura, Solothurn, Basel-Land, Sankt-Gallen, Glarus, Ticino, Fribourg, and Thurgau, have developed projects under the decree. In 1994, a revision to the Decree changed the criteria for selecting the covered municipalities[5]. To avoid mixing two policies with different instruments and coverage, we restrict the analysis to the initial policy and exclude periods after the 1994 revision.

The policy targeted individual firms rather than municipalities themselves. Therefore, municipalities, their residents, and most of the existing firms do not benefit directly from the policy. Incumbent firms might even suffer from this new subsidized competition. An early report addressed to the Parliament even openly stated that the policy was "[...] *not a question of maintaining jobs in failing companies, but of creating them in healthy companies*" (Swiss Federal Council 1978). Because of this stronger and subsidized competition, the Lex Bonny could adversely affect incumbent firms.

No causal evaluation exists for the first phase of Lex Bonny. Contemporary reports instead focused on the number of supported projects and jobs created through subsidies (Economic Affairs and Taxation Committee of the National Council 1994). By February 1994, authorities had supported 531 private-sector projects under the decree. These projects represented total investments of about CHF 2.5 billion. About one quarter of the supported projects involved new foreign establishments, and one

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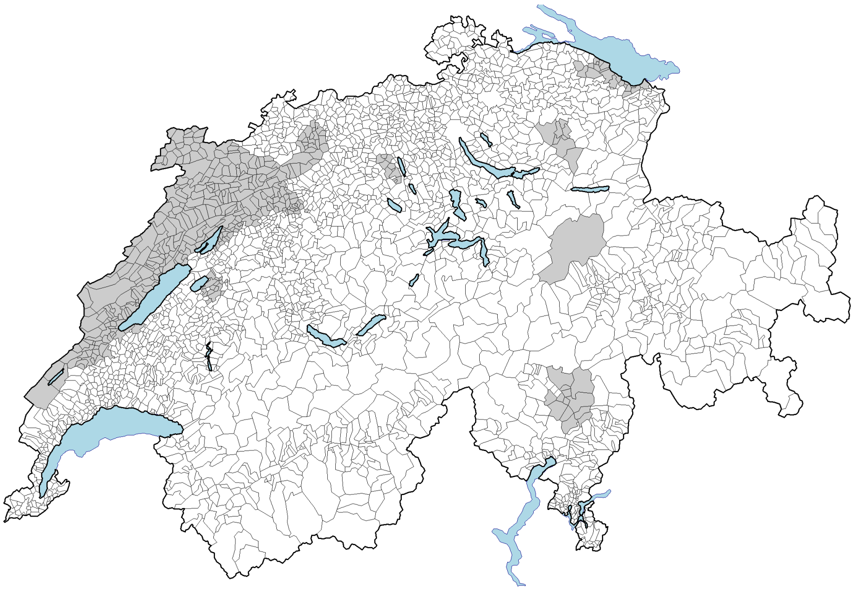
[4] A report by Swiss Federal Audit Office (2014) later revealed that the proportion of individuals and workers taxed at source, as a share of total employees, ranged from 20% to 60% in firms benefiting from the policy. Politicians argued that the policy mostly benefited foreign workers.

[5] These include an unemployment rate exceeding the national average by one tenth during the previous three years, an unfavorable employment trend, and poor development prospects.

Figure 2.2: Map of the municipalities covered by Lex Bonny from 1979 to 1993



Notes: The light grey areas highlight the municipalities where firms are eligible for Lex Bonny subsidies according to our own data. The map shows the state in 1979. The area covered did not change until late in 1994. Figure 2.A.2 in the Appendix shows official maps of the Bonny coverage for comparison

fifth involved the creation of new domestic firms. The remaining projects, slightly more than half, financed innovation or diversification investments by existing firms.

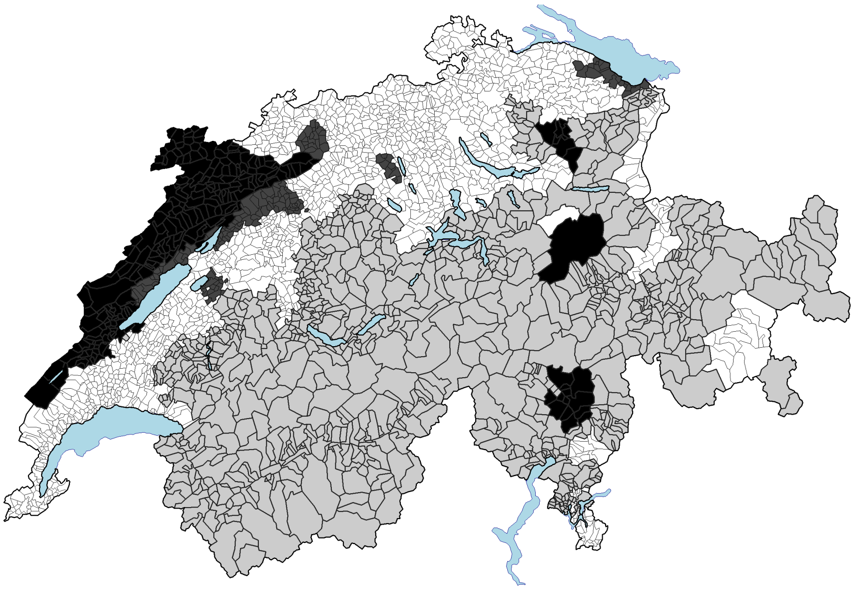
2.2.4 Geographical overlap

The two policies do not always target the same municipalities. Some municipalities are eligible for neither policy, some for only one, and others for both. Figure 2.3 shows the geographic distribution of these treatment categories and the overlap between the two programs.

However, we could not recover the full timing of LIM eligibility. We know which municipalities were eventually covered by LIM, but we cannot identify the year in which they first became eligible. This limitation matters for identification. To avoid contaminating Lex Bonny's estimates, we exclude all LIM-eligible municipalities from the main analysis. Identification relies on comparisons within the set of municipalities that were never covered by LIM.

Given these treatment definitions, municipalities fall into four mutually exclusive categories. In 1993, about 6.28% were eligible only for Bonny. 53.86% were neither LIM- nor Bonny-eligible. The remainder (around 40% of municipalities) were either eligible for LIM only, or for both LIM and Bonny.

Figure 2.3: Map of the municipalities covered by Lex Bonny and LIM in 1993



Notes: The map shows the treatment status in 1993, once all LIM regions are defined. The area covered by Lex Bonny did not change until 1994, while the LIM regions were unchanged until 2007. The light grey areas highlight the municipalities eligible for LIM subsidies. The dark grey municipalities are included in the Lex Bonny coverage. The dark municipalities are treated by both policies, and the white municipalities are excluded from both.

2.3 Theoretical framework and related literature

2.3.1 Place-based policies empirical evidence

Early evaluations of place-based policies rely largely on correlational evidence and often deliver mixed or weak results. Negative associations between policy interventions and local economic performance are common in the early empirical literature.[6] More recent studies exploit quasi-experimental variation and generally find that spatially targeted policies affect local economic activity. However, the size and persistence of these effects vary substantially across programs and contexts. For example, Greenstone et al. (2010) study large plant openings in U.S. counties and document substantial productivity spillovers to incumbent plants. In a series of papers on European Union regional policy, Becker et al. (2010, 2012, 2013, 2018) find positive effects on economic growth and show that these effects vary across implementation periods.

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[6] Neumark and Simpson (2015) provide a detailed review of early empirical contributions. As Juhász et al. (2024) emphasizes, a central limitation of this early literature is that many estimates do not rely on research designs that credibly identify causal effects.

A key insight for interpreting firm-level responses is that changes in the number of firms can arise through distinct margins. Net increases in the number of firms may reflect higher entry, lower exit, and/or relocations, and these margins can respond differently to incentives. Recent evidence suggests that induced entry can be particularly important: Lu et al. (2019) show that new entrants account for a large share of the positive effects of place-based policies in China, including gains in employment, wages, productivity, and investment. At the same time, improved firm outcomes do not necessarily translate into local job creation. For example, Brachert et al. (2019) find that productivity gains associated with a German place-based policy do not lead to higher wages or employment.

In the literature, the focus is generally on job creation and employment in the treated regions, as in Papke (1993, 1994), Greenbaum and Engberg (2004), Neumark and Kolko (2010), Gobillon et al. (2012), or de Castris and Pellegrini (2012). Several studies also document long-run effects of place-based interventions. Garin and Rothbaum (2024) study the local long-term effects of US war-industry production following WW2. Locations with new large factories enjoyed long-term positive effects on manufacturing employment. Children who grow up near these factories later display higher economic mobility. Access to well-paid manufacturing jobs appears to contribute to these long-run gains. Other studies document similar persistent effects (Kline and Moretti 2013, Incoronato and Lattanzio 2024).

However, poorly designed programs can lead to the misallocation of resources to less productive firms. Regardless, there are several reasons why the government might want to implement place-based industrial policies. One important rationale is that these policies may trigger agglomeration economies and help regions become self-sustaining. Evidence suggests that such interventions can generate persistent agglomeration effects and long-run growth, as documented for the United States (Kline and Moretti 2013, Garin and Rothbaum 2024), Finland (Mitrunen 2024), and Italy (Incoronato and Lattanzio 2024). Yet these gains are not guaranteed. Programs that favor particular industries may create excessive specialization, and subsidies to unproductive firms can reduce overall economic efficiency (Glaeser and Gottlieb 2008).

A central issue for interpretation is that place-based interventions may reallocate activity across space rather than generate true economic growth. von Ehrlich and Seidel (2018) document relocation of economic activity in their evaluation of EU regional policy. More generally, spillovers may be positive or negative. Agglomeration effects and stronger local demand may benefit nearby areas, while firm and worker relocation may harm them. Empirical evidence often shows that spillovers are highly localized and decay rapidly with distance, although some interventions generate positive effects in neighboring regions (von Ehrlich and Seidel 2018, Einiö and Overman 2020, Gallé et al. 2024).

Several studies examine policies that are closely related to our setting. Mayer et al. (2017) find that French enterprise zones increase firm entry rates and also raise employment and wages. Criscuolo et al. (2019) exploit externally determined EU eligibility rules to evaluate regional aid in the United Kingdom and find that the policy increased jobs and reduced unemployment, with effects operating primarily through small firms. Interestingly, the effects persist long after the EU's regional policy ended, whereas in other studies the effects vanish after the programs end (Barone et al. 2016, Di Cataldo 2017, Becker et al. 2018).

Although many papers examine similar outcomes, the policies themselves differ widely. Contexts and program designs play a major role in the success or failure of place-based policies. They require careful design and targeting as emphasized in Austin et al. (2018) and von Ehrlich and Overman (2020). Of course, different policy designs yield different outcomes, as shown by Blouri and von Ehrlich (2020) and Canova and Pappa (2025). Evidence from India illustrates this point clearly. Görg and Mulyukova (2024) find no productivity growth in special economic zones and document differences between publicly and privately owned zones.

These insights motivate our empirical strategy. We focus on firm dynamics at the Swiss municipal level, decompose stock effects into creations, deaths, and relocations, and explicitly assess neighboring effects using a spatial DiD design.

2.3.2 Economic mechanisms & hypothesis

From an efficiency perspective, altering the initial location decisions of economic activities might be costly at the national level. Firms may simply relocate, receive subsidies, and produce the same goods they would have produced elsewhere without subsidies. Nevertheless, local governments actively seek to attract firms to their jurisdictions, as this would increase tax revenues. They often offer substantial subsidies in the hope of generating new tax revenues and local multiplier effects (Moretti 2010). These incentives may include "*[...] tax breaks, low-cost or free land, the issuance of tax-exempt bonds, training funds, the construction of roads, and other infrastructure investments*" (Greenstone et al. 2010). In theory, firms choose locations that maximize expected profits. Hence, relocation and investment decisions depend on the expected gains generated by government incentives.

Despite these aggregate efficiency concerns, national governments still regularly implement place-based policies. Policymakers typically justify these interventions on redistributive grounds. National policies usually aim to reduce persistent spatial inequalities (Bartik 2020). Place-based policies may increase the attractiveness of regions that benefit less from agglomeration forces when governments seek to reduce regional disparities. However, these policies may also generate efficiency losses. Gaubert (2018) shows that place-based policies can produce negative aggregate effects. As Kline and Moretti (2013) argue, such policies may be locally efficient and reduce nationwide inequality, but at the expense of reducing agglomeration economies elsewhere. In practice, the policies are often justified on redistributive grounds, given the profound negative social and political consequences in regions with persistent disadvantage (Austin et al. 2018, von Ehrlich and Overman 2020).

This chapter evaluates the effects of Lex Bonny on local economic activity. We focus on firm dynamics and analyze the number of firms as well as firm entry, exit, and relocation. The policy encourages firms to open new establishments, expand existing activities, innovate, or relocate to eligible regions. Firms constitute the primary target of the intervention. Policymakers expect that the arrival of new firms will attract additional economic activity and potentially generate local industry clusters (Swiss Federal Council 2000). If the policy works as intended, treated municipalities should experience higher firm activity than untreated municipalities.

We can summarize the expected effects of the Lex Bonny as follows: First, Lex Bonny should increase the total number of firms in treated municipalities. Second, the policy should raise firm entry by lowering investment costs for new establishments. This increase in economic activity can intensify agglomeration economies and attract additional firms. At the same time, stronger competition from subsidized entrants may increase the number of incumbents exiting. Subsidies may also attract firms from other municipalities, generating relocation rather than new economic activity. By observing entry, exit, and relocation separately, we can distinguish between genuine local expansion and spatial reallocation.

The effects on neighboring municipalities are less clear. On the one hand, increased activity in treated municipalities may generate positive spillovers through agglomeration effects or stronger local demand. On the other hand, nearby firms may face subsidized competitors. The net effect depends on whether agglomeration benefits outweigh competitive pressures. Spillover effects may also decline with distance from treated municipalities. Previous evidence suggests that, when present, spillovers from place-based policies are often highly localized (von Ehrlich and Seidel 2018, Einiö and Overman 2020).

The effects on workers and taxpayers are also ambiguous. Lex Bonny does not require firms to hire local workers, and workers can commute across municipal borders. Firm entry may therefore increase employment at the workplace without necessarily increasing employment among local residents. Similarly, changes in the local tax base depend on whether new jobs benefit residents or commuters. Although a full welfare analysis lies beyond the scope of this chapter, the results for workers and taxpayers help identify who captures gains and whether the effects differ across the education and income distributions.

2.4 Data

2.4.1 Outcomes

Firms

Our firm data comes from the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*". We digitize all available editions from 1934 to 2003 and extract the universe of Swiss corporations. Each firm-level observation reports the firm's name and location, as well as its board size and nominal capital. Using these publications, we construct yearly cross-sections of the firm universe for 1934, 1943, 1960, 1962-1966, 1969, 1972, 1975, and 1979-2003. We then aggregate the data to the municipal level and build a panel of firm counts.[7]

Because we track firms across cross-sections, we can compute municipal-level measures of firm entry, exit, and relocation. We define firm entry (exit) as the first (last) appearance of a firm in the panel. We define relocation as a change in the municipality of a firm's headquarters between two periods. We exclude 1934 because this edition covers only a subsample of firms. We also exclude 1943 because it lies too far from the next available cross-section in 1960. Our working dataset therefore covers 1960, 1962–1966, 1969, 1972, 1975, and 1979–2003. Appendix 1.A in Chapter 1 details how we assign firm identifiers across periods and how we handle gaps between cross-sections.

We also use board size and nominal capital as proxies for firm size. This allows us to test whether the policy affects firms of different sizes differently. Azar (2021) documents a positive correlation between board size and firm size measured by market

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[7] We aggregate the data using harmonized municipal boundaries as of 2012. This avoids issues arising from municipal mergers and allows us to combine the firm variables with the workers, taxpayers, and geographic variables from Fretz et al. (2021). When a merger involves both covered and uncovered municipalities, we classify the merged municipality as treated under Lex Bonny. This concerns very few cases.

capitalization. For each period, we compute the national distribution of both variables and assign firms to percentile groups. We then count the number of firms in each group at the municipal level. As a result, we obtain count variables for all firm outcomes by position in the national distribution of board size and nominal capital.

Workers & taxpayers

We complement the firm data with municipal-level employment data from the Business Census for 1965, 1975, 1985, and 1995 (Swiss Federal Statistical Office 1955). These data report the number of employees working in each municipality. They cover all jobs in the secondary and tertiary sectors, which are the sectors most likely to respond to the policy.

We complement the Business Census data with both residence- and workplace-based employee numbers for 1969, 1979, and 1989. These data allow us to use the number of employees in a municipality by education level (Swiss Federal Office of Transport 2016). The residence-based variables count the number of employed residents living in a municipality, regardless of where they work. The workplace-based variables count the number of employees working in a municipality, irrespective of where they live.

Finally, we use the count and share of taxpayers in percentile groups at the municipal level. We use the tax data from Fretz et al. (2021), who collect them from the Swiss federal income tax statistics (Swiss Federal Tax Administration 2016).

2.4.2 Treatment variable - Lex Bonny eligibility

Our treatment variable is a dummy indicating whether a municipality is eligible for the Lex Bonny. We compile lists of coverage announcements for the policy (see Appendix 2.A.2), which allow us to identify eligible and ineligible municipalities. We focus on the first wave of the program, spanning from 1979 to the end of 1994[8]. During this period, municipal eligibility is unchanged. Treatment status changes only with the implementation of the program. We also exclude municipalities eligible for LIM subsidies, as well as those eligible under Lex Bonny. This restriction allows us to isolate the effect of Lex Bonny relative to municipalities that remain untreated.

We georeference the municipalities and use their centroids to measure Euclidean Distances to the closest border of the treatment units. We use these distances to

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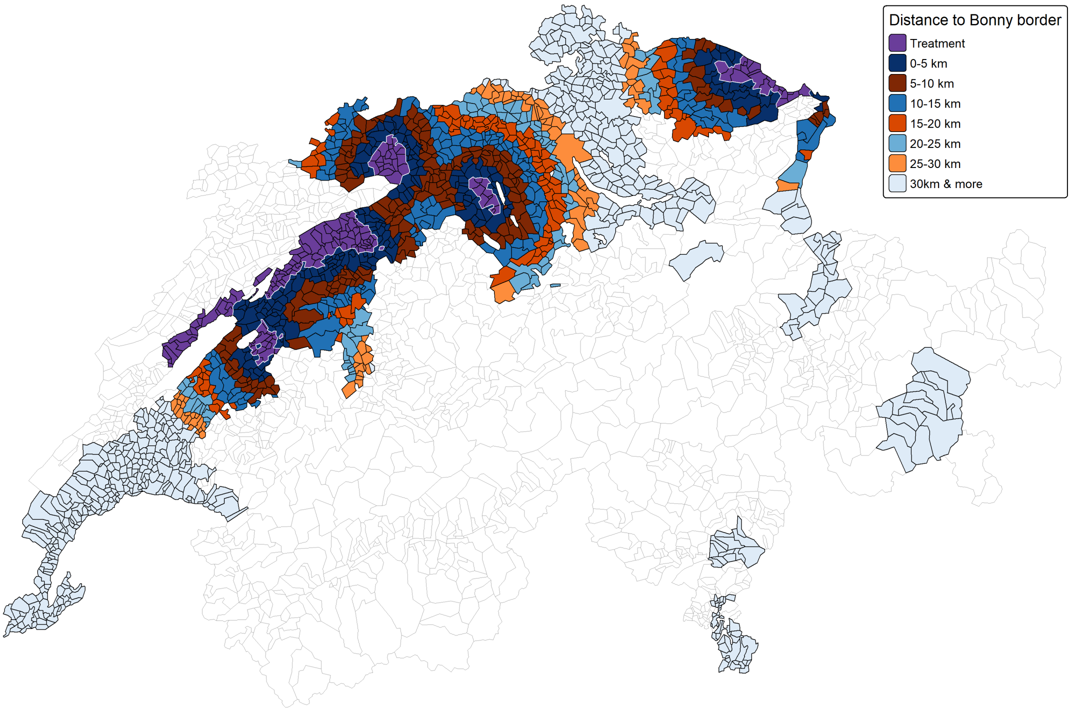
[8] We restrict our analysis sample to 1960 to 1993 to avoid anticipation effects potentially arising from the November 1994 revisions

construct the bins for the spatial DiD analysis. Figure 2.4 shows the geographical repartition of the treatment group and the distance bins used in the empirical analysis.

Figure 2.4: Treatment group and 5km bins control groups



Notes: The map shows the locations of the treated, potentially exposed but untreated, and untreated municipalities. The treatment group consists of municipalities covered only by the Lex Bonny.

2.4.3 Municipal characteristics & LIM coverage

Municipal characteristics

We include several municipal characteristics as controls in our analysis. These variables constitute the pre-treatment conditions, which we interact with a post-treatment indicator in the full specification.

First, we include demographic data, including population size, the number of residents aged 0 to 20, and the number of residents aged 65 or older. Second, we collect several geographic characteristics. We measure the distance to the nearest urban center, to the nearest lake shore, and data on highway accessibility from Fretz et al. (2021), as well as a dummy indicating the presence of a railway station in the municipality[9]

LIM coverage

Several sources list LIM-treated units, but we could not consistently recover the exact dates of LIM region creation. To avoid confounding Lex Bonny effects with LIM support, we exclude all municipalities eligible under the LIM from both the treatment and control groups. We use Swiss Federal Statistical Office (2005) to define the set of LIM municipalities to exclude.

2.4.4 Summary statistics

Table 2.1 reports summary statistics for treated and untreated municipalities over the whole sample period. On the one hand, treated municipalities have fewer firms, fewer firm creations, and fewer firms in-migrations. On the other hand, fewer firms exit or relocate from the treatment municipalities. Eligible municipalities differ from the average municipality along several dimensions. In particular, they tend to have smaller populations and lower economic activity, yet are closer to urban centers and better connected to transport infrastructure. These differences reflect the non-random targeting of Lex Bonny and motivate an empirical strategy that controls for time-invariant municipal characteristics and pre-treatment characteristics.

Differences in labor-market variables are less pronounced. The number of workplace-based employees differs, but education shares are almost identical. The Business Census also shows that treated municipalities have a higher share of employment in the secondary sector, while untreated municipalities have more jobs overall.

Table 2.2 confirms these differences over the pre-treatment period 1969–1978. Treated municipalities have lower firm counts and flows, but they are closer to highways and urban centers. These gaps increase when the control group is restricted to municipalities farther from the treated areas. However, DiD identification does not require equal baseline levels, but it requires parallel trends. Section 2.6 presents visual examinations of the parallel trends.

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[9] Our dataset also includes a variable indicating the class of municipalities as defined by the Federal Statistical Office (Swiss Federal Statistical Office 2005). The typology classifies municipalities as "*urban centers*", "*suburban municipalities*", or "*rural municipalities*".

Table 2.1: Summary Statistics

	All (1)		Treated (2)		Untreated (3)	
Firms (SA/AG)	36.32	(341.34)	18.84	(59.99)	51.51	(448.02)
Share of Firms in 50th-25th percentile (capital)	0.34	(0.27)	0.34	(0.26)	0.34	(0.26)
Share of Firms in 25th-10th percentile (capital)	0.14	(0.20)	0.17	(0.21)	0.14	(0.19)
Share of Firms in 10th-1th percentile (capital)	0.09	(0.15)	0.09	(0.15)	0.09	(0.15)
Share of Firms in top 1% (capital)	0.01	(0.05)	0.01	(0.04)	0.01	(0.04)
Share 1 owner	0.35	(0.28)	0.34	(0.26)	0.37	(0.27)
Share 2 owners	0.27	(0.25)	0.31	(0.25)	0.28	(0.25)
Share 3 owners	0.23	(0.24)	0.23	(0.21)	0.23	(0.23)
Share 4 owners	0.07	(0.14)	0.07	(0.14)	0.06	(0.13)
Share 5 owners	0.04	(0.12)	0.03	(0.07)	0.03	(0.09)
Share 6+ owners	0.04	(0.11)	0.02	(0.07)	0.02	(0.08)
Firm births	3.22	(35.79)	1.67	(6.69)	4.55	(47.27)
Firm deaths	1.58	(18.82)	0.72	(3.20)	2.26	(24.28)
Firms relocating in	0.60	(4.01)	0.35	(1.16)	0.86	(5.02)
Firms relocating out	0.64	(6.46)	0.33	(1.27)	0.94	(8.39)
Population (in 1,000)	2.55	(11.23)	2.29	(3.83)	3.29	(14.91)
Share of 0-20 years old	0.31	(0.06)	0.30	(0.05)	0.31	(0.05)
Share of 65+ years old	0.13	(0.04)	0.12	(0.03)	0.12	(0.04)
# Taxpayers (in 1,000)	0.78	(3.95)	0.72	(1.31)	1.05	(5.27)
Share in bottom-50% income	0.57	(0.12)	0.52	(0.08)	0.53	(0.11)
Share in 50-25% quartile	0.24	(0.05)	0.26	(0.04)	0.25	(0.04)
Share in top-25-10%	0.13	(0.04)	0.15	(0.03)	0.15	(0.04)
Share in top-10% decile	0.09	(0.06)	0.09	(0.04)	0.11	(0.07)
# Workplace-based workers (in 1,000)	1.26	(8.11)	1.10	(2.59)	1.68	(10.83)
Share of workplace-based workers with low education	0.42	(0.16)	0.39	(0.13)	0.38	(0.14)
Share of workplace-based workers with middle education	0.50	(0.13)	0.53	(0.10)	0.53	(0.11)
Share of workplace-based workers with high education	0.08	(0.06)	0.08	(0.05)	0.09	(0.06)
# Residence-based workers (in 1,000)	1.23	(5.38)	1.13	(1.87)	1.61	(7.14)
Share of residence-base workers with low education	0.38	(0.17)	0.35	(0.14)	0.34	(0.15)
Share of residence-base workers with middle education	0.52	(0.12)	0.55	(0.10)	0.54	(0.10)
Share of residence-base workers with high education	0.10	(0.07)	0.11	(0.06)	0.12	(0.08)
# of workers 2nd and 3rd sectors (in 1,000) - Business Census	1.11	(7.77)	0.96	(2.38)	1.51	(10.41)
Share of workers in 2nd sector	0.52	(0.23)	0.60	(0.23)	0.53	(0.22)
Share of workers in 3rd sector	0.49	(0.23)	0.40	(0.23)	0.47	(0.22)
Distance to closest national and planned highway [km]	12.14	(9.74)	9.90	(8.45)	10.07	(8.46)
Railway station	0.39	(0.49)	0.47	(0.50)	0.38	(0.48)
Distance to closest urban center	22.80	(16.14)	13.38	(7.46)	17.77	(11.17)
Distance to lake shore	16.87	(15.29)	12.93	(13.36)	14.16	(12.61)
# Municipalities	2484		156		1338	

Notes: Entries report means with standard deviations in parentheses. Column (1) includes all Swiss municipalities. The treated sample (column (2)) comprises all municipalities covered by the Lex Bonny, whereas the untreated sample (column (3)) includes those not covered by Bonny or LIM. The firm data covers all years between 1960 and 1993. Workplace- and residence-based data are available for 1969, 1979, and 1989. The number of workers, according to the Business Census, is available for 1965, 1975, and 1985. The number is limited to the 2nd and 3rd sectors. The distance to the nearest highway access is measured in road distance, whereas the other distances are computed as Euclidean distances.

Table 2.2: Group comparisons - Mean Test Equality

	Mean values for variables in period 1969-1978								
	All Municipalities (1)	Treated (2)	Untreated (3)	Untreated 0-10km (4)	Untreated 10-20km (5)	Untreated 20-30km (6)	Untreated 30km+ (7)	Test equality (2)-(3) (p-value)	Test equality (2)-(6) (p-value)
# Firms (in 1,000)	31.26	13.95	44.09	16.74	37.75	88.38	93.34	0.20	0.80
	(317.24)	(47.95)	(411.61)	(115.65)	(282.58)	(637.82)	(773.59)		
Firm births	2.30	0.89	3.17	1.34	2.47	7.30	5.46	0.14	0.83
	(24.31)	(2.44)	(30.06)	(10.87)	(17.22)	(56.07)	(43.93)		
Firm deaths	1.09	0.40	1.51	0.54	1.14	3.32	3.28	0.16	0.89
	(16.73)	(1.94)	(20.35)	(6.38)	(12.19)	(33.92)	(36.50)		
Firms relocating in	0.27	0.15	0.38	0.15	0.33	0.86	0.64	0.01	0.82
	(1.95)	(0.51)	(2.44)	(1.04)	(1.67)	(4.65)	(2.83)		
Firms relocating out	0.35	0.15	0.50	0.17	0.40	1.15	0.94	0.09	0.93
	(3.98)	(0.59)	(5.12)	(1.44)	(2.99)	(9.80)	(7.30)		
Population	2543.12	2302.90	3283.37	1745.60	3915.77	5659.33	3783.05	0.20	0.85
	(11470.13)	(3984.07)	(15226.93)	(4404.13)	(14980.46)	(28020.74)	(13997.26)		
Share of of 0-20 years old	0.33	0.32	0.33	0.33	0.33	0.32	0.31	0.00	0.00
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)		
Share of of 65+ years old	0.13	0.12	0.12	0.13	0.11	0.11	0.12	0.73	0.00
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)		
# Residence-based workers	1149.60	1106.92	1490.10	767.25	1779.39	2629.65	1714.03	0.13	0.00
	(5962.91)	(2042.10)	(7926.10)	(1983.19)	(7593.68)	(14935.48)	(7290.61)		
# Workplace-based workers	1190.61	1122.08	1554.12	801.30	1823.52	2765.03	1691.97	0.46	0.97
	(7965.36)	(2721.61)	(10616.05)	(2570.30)	(10138.58)	(19877.83)	(9325.89)		
Distance to closest national and planned highway [km]	15.49	10.90	10.10	11.73	9.08	7.67	10.33	0.56	0.00
	(12.52)	(9.80)	(8.14)	(8.36)	(7.34)	(6.59)	(9.86)		
Railway station	0.39	0.47	0.38	0.34	0.37	0.46	0.39	0.15	0.00
	(0.49)	(0.50)	(0.48)	(0.47)	(0.48)	(0.50)	(0.49)		
Distance to closest urban center	22.80	13.38	17.77	19.52	17.65	13.15	18.47	0.00	0.00
	(16.14)	(7.46)	(11.17)	(9.54)	(9.29)	(7.41)	(20.00)		
Distance to lake shore	16.87	12.93	14.16	15.74	13.24	10.17	16.52	0.05	0.00
	(15.29)	(13.36)	(12.61)	(12.26)	(11.17)	(9.07)	(18.61)		
# Municipalities	2484	156	1338	561	392	228	157		

Notes: This table reports average pre-treatment municipal characteristics measured over the period 1969–1978. Entries are means, with standard deviations in parentheses. “Treated” municipalities are those eligible for Lex Bonny subsidies (and not covered by LIM); “Untreated” municipalities are those not eligible for either Bonny or LIM, partitioned into distance-to-treatment bins (0–10 km, 10–20 km, 20–30 km, and 30 km+), where distance is measured from each municipality to the closest treated-area border using municipal centroids. Columns (7)–(8) report p-values from two-sample mean equality tests.

2.5 Empirical methodology

2.5.1 Identification challenges and empirical strategy

Research on place-based policies faces several identification challenges. First, eligibility depends on specific characteristics. This is a case of confoundedness, or a lack of conditional independence (CIA), as treatment assignment variables are correlated with potential outcomes (Debarsy and Le Gallo 2025). Second, standard DiD estimates may be biased if treatment spillovers affect nearby control units. Third, excluding nearby controls might introduce bias by violating the parallel trends assumption. This is because control units far from the treatment region are usually worse counterfactuals (Butts 2021). Table 2.2 illustrates this concern. Differences between groups increase as the bins are farther from the treated municipalities.

We address these issues in several steps. We begin with a standard DiD approach, assuming SUTVA holds[10]. In this approach, we compare municipalities covered by

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[10] We also require no anticipation of the treatment and common trends across groups before treatment. Our empirical strategy allows us to visually assess whether pre-trends are parallel and whether there are effects before the policy implementation.

the Lex Bonny with untreated municipalities, those included neither in Bonny nor in LIM. We estimate the cumulative treatment effect using an event-study approach, allowing us to observe coefficients in pre-treatment periods. We also control for baseline differences between treated and control municipalities by interacting pre-treatment characteristics with a post-treatment indicator. This allows municipalities with different initial conditions to follow different post-treatment trajectories, reducing concerns that incomparability biases our DiD estimates (Altonji et al. 2005).

However, this baseline approach assumes no spillovers. Empirical evidence suggests that place-based policies often affect nearby untreated areas but are not far-reaching (von Ehrlich and Seidel 2018, Einiö and Overman 2020). Our second step focuses on treated municipalities but accounts for potential contamination from nearby controls. Specifically, we implement a concentric-ring analysis, following Neumark and Kolko (2010), Kline and Moretti (2014), and Lu et al. (2019). This approach excludes control municipalities located near treated areas, as these are most likely to experience spillovers. The trade-off is clear: excluding nearby controls reduces contamination but may also reduce comparability (Butts 2021). We therefore use this approach to assess the sensitivity of the estimated treatment effect to the exclusion of potentially exposed controls.

Finally, we implement the spatial DiD approach proposed by Butts (2021) and applied, for example, by Gallé et al. (2024). This approach uses untreated municipalities sufficiently far from treated areas as the reference group for both treated and untreated municipalities exposed to spillovers. The goal is to isolate a control group that is not contaminated by treatment, even at the cost of some comparability. Unlike the concentric-ring analysis, we directly estimate the policy effects on untreated municipalities that are exposed to spillovers, rather than excluding them.

Estimating the SDiD specification on nearby untreated (but exposed) municipalities serves two purposes. First, it provides an estimation of spatial spillovers. If untreated municipalities located near treated areas experience changes in outcomes following treatment, this indicates that the policy's effects extend beyond the defined borders. Second, this exercise allows one to distinguish between positive local spillovers and spatial reallocation. Positive effects in nearby untreated areas are consistent with agglomeration or demand spillovers, whereas negative effects would suggest displacement of economic activity away from neighboring locations. This distinction matters for assessing whether place-based policies generate net local gains or simply reorganize existing activity across space (Neumark and Simpson 2015).

2.5.2 Specifications

Standard DiD with dynamic effects

In the main analysis, we focus on municipalities in which firms are eligible for Lex Bonny subsidies. Identification comes from comparing municipalities eligible only for Lex Bonny with municipalities eligible for neither Bonny nor LIM. The coefficients capture the cumulative municipal-level effect of Bonny eligibility on firm outcomes.

We estimate the dynamic effects of the policy on the number, creation, death, and relocation of firms using the following specification:

$$Y_{i,t} = \exp\left(\sum_{\tau=-8}^{10} \beta_\tau \text{Treat}_{i,t+\tau} + \boldsymbol{\eta}(\mathbf{X}_i \cdot \text{Post}_t)' + \xi_i + \xi_t + \theta_i t\right) \cdot \epsilon_{i,t}, \tag{2.1}$$

where $Y_{i,t}$ denotes one of the firm outcomes: the number of firms, firm entry, firm exit, or relocations into or out of municipality i[11]. $\text{Treat}_{i,t+\tau}$ equals one if municipality i is eligible for Lex Bonny at time $t+\tau$, and β_τ captures the cumulative treatment effect τ periods relative to the policy implementation in 1979, with $\tau = -1$ as the reference period. We include municipality fixed effects ξ_i, year fixed effects ξ_t, and municipality time trends $\theta_i t$. We include municipality-specific linear trends to absorb differential pre-existing trajectories[12].

In the full specification, we follow Lu et al. (2019) and Gallé et al. (2024), and control for municipality baseline characteristics by interacting the vector of characteristics $\mathbf{X}$ with the post-treatment dummy $Post_t = 1$ for $t \geq 1979$. This vector includes pre-treatment controls for demographics, infrastructure access, employment, and tax base[13]. Similar estimates across specifications reduce concerns that differences in pre-treatment characteristics drive the results. We cluster the municipality-level error term, $\epsilon_{i,t}$, at the district level to allow for spatial and serial correlation across

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[11] All outcome variables are non-negative count data. Therefore, all our regressions are estimated via PQML regressions using Stata's *ppmlhdfe* command.

[12] Eligibility partly depends on sustained declines in population and active population over several years. Thus, municipalities may differ in underlying pre-treatment trajectories, motivating specifications that allow for differential municipal trends. We report results without these trends to show that the full specification estimates are not driven by trend specification. The specification using only fixed effects also show positive trends and long-term effects.

[13] For demographics, we control for the pretreatment population and the share of residents aged 0-20 and 65 or older. We also use the residence-based variable, which counts the number of employed residents living in a municipality, regardless of where their jobs are located. Similarly, we control for the workplace-based workers, which counts the number of employees working in a municipality, irrespective of where they reside. To capture infrastructure access, we control for distance to the nearest highway access, the presence of a railway station, and distances to key locations, namely the nearest urban center and the nearest lake shore. We use 1979 values as the pre-treatment baseline characteristics.

nearby municipalities. We compute the long-run effect following Davidson and MacKinnon (2004).

As a robustness check, we estimate several doughnut-style specifications that exclude control units within different distance bins from the treatment group. Nearby untreated municipalities may be indirectly affected by the policy through spatial spillovers or anticipation effects, potentially contaminating the control group and biasing the estimated treatment effects. By progressively removing nearby controls, the doughnut design assesses the sensitivity of the estimates to local contamination.

Spatial DiD

We complement the baseline DiD with a spatial DiD analysis to assess spillovers and the spatial decay of treatment effects across distance bins. We use municipalities located 25 to 30 km from the treatment border as the reference group. We use the following regression equation to non-parametrically estimate the spatial profile of the treatment effects:

$$Y_{i,t} = \exp\left(\sum_{d=0,d\neq 6}^{6} \beta_d (D_{d_i=d} \cdot Post_t) + \boldsymbol{\eta}(\mathbf{X}_i \cdot Post_t)' + \xi_i + \xi_t + \theta_i t\right) \cdot \epsilon_{i,t}, \tag{2.2}$$

where $Y_{i,t}$ is one of the firm outcome variables in municipality i in year t. The β_d is the treatment effect in bin d relative to the reference group[14]. We exclude municipalities located more than 30km away from the treatment borders, as they might not be comparable to treated areas.

Spatial DiD with dynamic effects

Equation (2.2) does not allow us to observe pre-treatment trends, although parallel trends are required for identification. To address this limitation, we estimate event-study specifications. These estimates allow us to inspect pre-treatment coefficients and assess whether excluding potentially exposed municipalities reduces comparability between treated and control groups or whether spillovers drive the baseline results.

Again, we start by estimating the effect of the Bonny treatment on the eligible municipalities compared to the reference group (25-30km bin). This distance band is sufficiently far to be plausibly unaffected by the treatment, yet close enough to remain

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[14] In our context, $d_i = 0$ indicates the treated municipalities, $d_i = 1$ the exposed controls in the 0-5km bin, up to $d_i = 6$ which indicates the municipalities located between 25 to 30km from the treatment borders. This specification allows us to estimate one $\hat{\beta}_d$ for each d_i in one single regression. We consider those municipalities as untreated, but also unexposed to spillovers.

comparable to treated areas in terms of regional trends and economic conditions. We then repeat the analysis for each 5 km distance bin (0-5 km, 5-10 km, etc.) to test for spillovers from Lex Bonny relative to the same reference group.[15]

We estimate the dynamic spatial effects using the following specification:

$$Y_{i,t} = \exp\left(\sum_{k=-8, k\neq -1}^{10} \beta_k \mathbf{1}[t - T_i = k] + \boldsymbol{\eta}(\mathbf{X}_i \cdot Post_t)' + \xi_i + \xi_t + \theta_i t \right) \cdot \epsilon_{i,t}, \quad (2.3)$$

where $Y_{i,t}$ is one of the firm outcome variables in municipality i in year t. The β_k is the cumulative treatment effect relative to the treatment period, which is indicated by T_i, measured relative to the 25–30 km reference group. The remainder of Equation (2.3) is similar to Equation (2.1).

The baseline dynamic DiD estimates the average effect of eligibility on treated municipalities. The spatial DiD then traces how this effect changes with distance from treated areas. Comparing the two helps us distinguish localized displacement from broader spatial spillovers.

2.6 Main results

2.6.1 DiD with dynamic effects

Figure 2.5 presents the baseline estimates from Equation (2.1). Table 2.B.1 in the Appendix reports the full regression results. Although treated municipalities differ from controls in pre-treatment levels, the event-study estimates show no evidence of differential pre-trends in the number of firms. Pre-treatment coefficients are small and statistically insignificant across specifications.

Turning to the post-treatment period, the estimates reveal a positive and persistent effect of the Bonny program on the number of firms in treated municipalities. The effect appears immediately following treatment and grows gradually over time, consistent with cumulative adjustment. Across specifications, the point estimates increase monotonically and become statistically significant in the first post-treatment period. In the full specification, treated municipalities have 67.19% more firms than untreated municipalities ten years after treatment.[16]. The dynamic pattern remains very similar across specifications, even though the magnitudes differ slightly. This stability increases confidence in the results.

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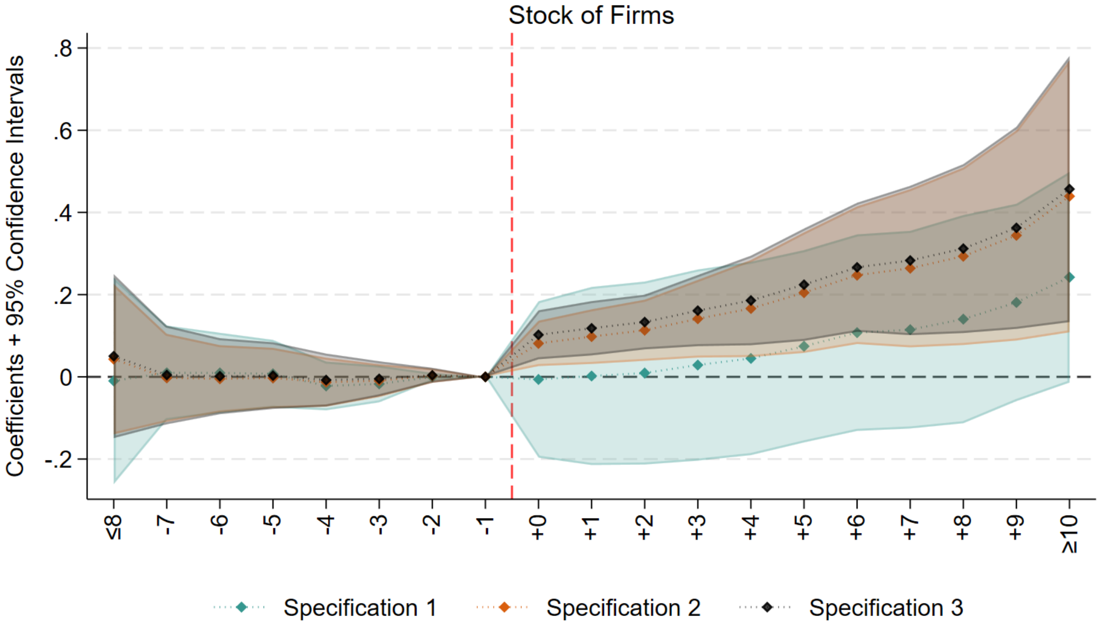
[15] We estimate one regression for each distance bin separately.

[16] We compute percentage effects as $100 \cdot \left(e^{\beta} - 1\right)$. For example, $100 \cdot \left(e^{0.514} - 1\right) = 67.19\%$.

We next decompose the change in the number of firms into firm entry, exit, and relocations into and out of treated municipalities. We estimate the same dynamic DiD specifications as in the baseline analysis, and present the corresponding event-study results in Figure 2.6[17].

Figure 2.6 presents the dynamic effects of the Bonny program on firm births, deaths, and firm relocations. For firm births, pre-treatment coefficients are close to 0 and statistically insignificant across specifications, which supports the parallel trends assumption. Following treatment, firm births increase immediately and grow steadily over time, indicating that increased entry is a key mechanism behind the rise in the number of firms documented in Figure 2.5. The evidence is weaker for firm deaths and relocations. For these outcomes, the pre-treatment coefficients do not support parallel trends. We therefore should interpret the post-treatment estimates more cautiously.

Figure 2.5: Cumulative effect before/after Lex Bonny- Number of firms



Notes: The figure presents our results estimating Equation (2.1). Specification 1 includes year and unit fixed effects. Specification 2 adds municipal-specific time trends, and Specification 3 also includes baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy. Table 2.B.1 in the Appendix provides all the details.

More precisely, firm births increase substantially in the long run, by approximately 120.56% to 128.67%, depending on the specification. Firm deaths exhibit an increase

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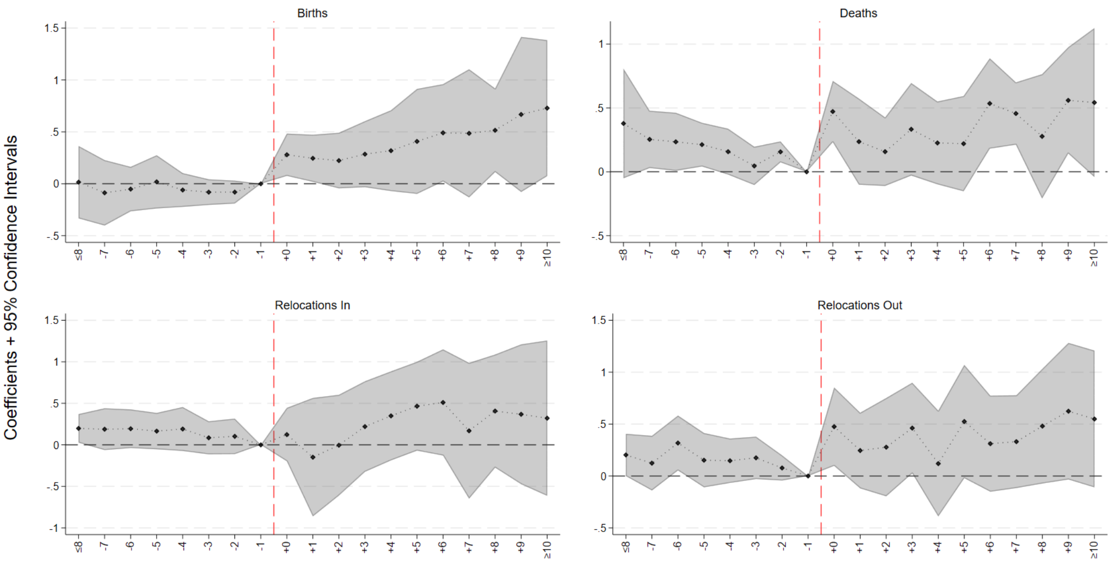
[17] The full set of coefficient estimates for these outcomes is reported in Table 2.B.2 in the Appendix.

over time. Similarly, relocations into and out of locations show slight increases[18]. However, given the lack of parallel pre-treatment trends for deaths and relocation, these magnitudes should not be interpreted as causal. Overall, the pattern is most consistent with treated municipalities attracting new firms. Deaths and relocations also react, but these movements may partly reflect pre-existing differences in trends.

Taken together, these results suggest that the positive effect of the Bonny program on the number of firms is driven primarily by increased firm entry. The next section examines whether spatial spillovers or local reallocation shape these effects using distance-based comparisons and spatial DiD specifications.

Figure 2.6: Cumulative effect before/after Lex Bonny - Details



Notes: The figure presents our results estimating Equation (2.1) when using the creation of firms as the outcome (and the full specification). The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy. Table 2.B.2 in the Appendix provides all the details.

2.6.2 Spatial DiD

Figure 2.7 presents the spatial DiD estimates. The spatial profile shows a clear positive effect on the number of firms inside the Bonny area. Outside the treated area, the coefficients decrease but remain mostly positive. While the spatial pattern

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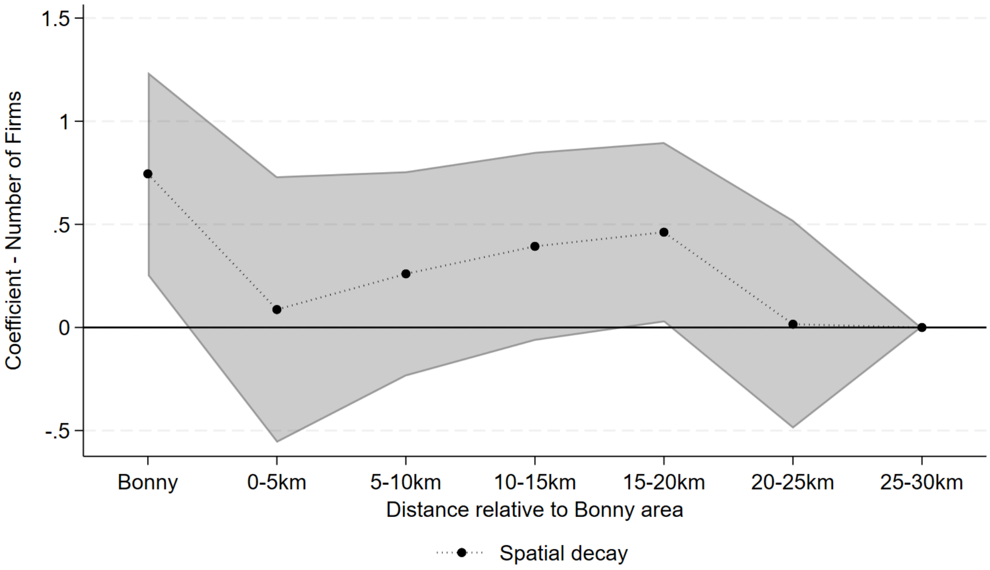
[18] For firm births, coefficients of 0.791 and 0.827 imply increases of approximately 120.56% to 128.67%, respectively, computed as $100 \cdot \left(e^{\beta} - 1\right)$. For firm deaths, coefficients of 0.527 and 0.633 imply increases of 69.38% and 88.30%, respectively, while coefficients for relocations into treated municipalities (0.268 and 0.272) imply increases of 30.74% and 31.28%, respectively. Coefficients for relocations out (0.463 and 0.468) imply changes of 58.89% and 59.71%.

suggests limited positive spillovers in the closer bins (0-5km and 5-10km), we observe stronger effects farther away (10-15km and 15-20km). Beyond 20 km, the effects largely disappear. Importantly, the coefficients in nearby untreated areas do not turn negative, which argues against strong displacement.

Figure 2.7: Spatial DiD with 25-30km as reference group - Number of firms

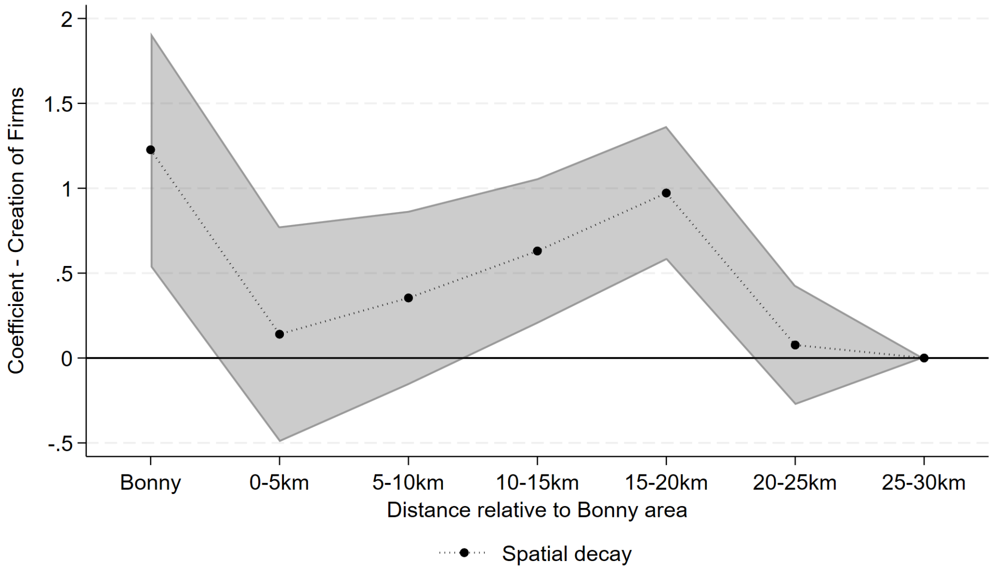


Notes: The figure presents our results estimating Equation (2.2) with the full specification. The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy.

However, the non-monotone profile likely reflects two opposing forces. First, municipalities very close to the Bonny border can benefit from positive spillovers. This raises outcomes in the nearest untreated rings and narrows the gap between treated and control municipalities in the baseline DiD, attenuating the estimated treatment effect. Second, the policy may divert entry at the treatment border. Eligibility creates a discontinuity in expected profits. Potential entrants that would otherwise locate just outside the Bonny perimeter may instead move a short distance inside the treated area to capture subsidies or tax relief. This mechanism is most important near the border, where firms can cross into the treated area at low cost while preserving access to the same labor and demand markets. Such sorting would depress observed entry in the 0–5 km untreated ring and generate a rebound farther away, thereby mechanically producing non-monotone spatial decay as in Figure 2.7. Figure 2.8 shows a very similar spatial pattern for firm creation, which indicates that this non-monotonicity mostly comes from entry rather than from deaths or relocations.

In short, Lex Bonny increases the number of firms within treated municipalities. Nearby untreated municipalities do not show negative effects, which argues against strong displacement. The non-monotonic spatial pattern likely reflects a combination of positive spillovers and border sorting.

Figure 2.8: Spatial DiD with 25-30km as reference group - Creation of firms



Notes: The figure presents our results estimating Equation (2.2) with the full specification. The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy.

2.6.3 Spatial DiD with dynamic effects

Starting with the treatment group, the estimates show some evidence of differential pretrends relative to the 25–30km reference group[19]. This reflects the trade-off emphasized by Butts (2021). Using controls farther from the treatment borders reduces spillover concerns, at the cost of comparability. After treatment, the coefficients turn positive and increase steadily over time. This pattern points to a persistent and growing effect of Lex Bonny on the number of firms.

It is informative to compare the spatial DiD estimates against the baseline DiD results. Obviously, the number of observations and municipalities decreases across specifications relative to the baseline regressions, as we consider only a subsample of

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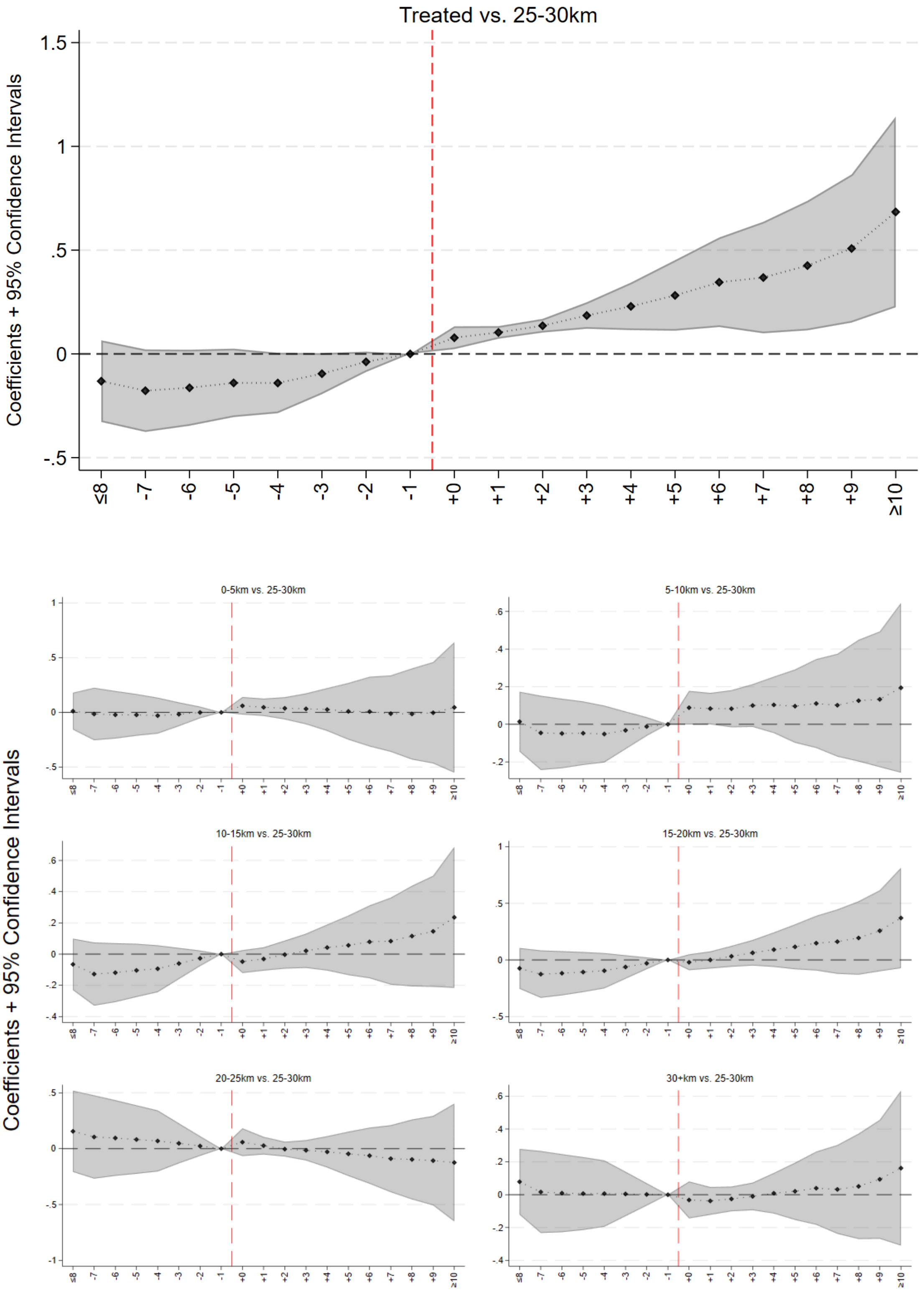
[19] The complete SDiD dynamic effects results are in Table 2.B.5 in the Appendix.

Figure 2.9: Cumulative effect before/after Lex Bonny - Number of firms, against 25-30km bin



Notes: The figure presents our results estimating Equation (2.3) with the full specification. The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy. The reference group consists of municipalities located 25-30km from the nearest Bonny border.

the entire control group[20]. Restricting the control group to geographically distant municipalities diminishes spatial exposure concerns, but comes at the cost of weaker pre-trend alignment. Therefore, the spatial DiD estimates complement the baseline DiD results.

Overall, the spatial DiD estimates have comparable signs and patterns, but are larger in magnitude than the baseline estimates. The long-term coefficients increase from 67.2% in the baseline to 88.5% in the spatial DiD[21]. Although pre-treatment differences remain visible, they are small relative to the large and persistent post-treatment effects. These results therefore reinforce the baseline finding that Lex Bonny increases the number of firms in treated municipalities.

We find no persistent long-run effects for most nearby untreated bins. Municipalities located within 0–5 km and 5–10km of treated areas do not experience increases in firm counts. While the 5-10km bins exhibit post-treatment increases in the first year following policy implementation, these effects flatten out directly. The number of firms increases in municipalities between 10-15km and 15-20km from treated areas relative to the reference group. Unlike in closer rings, this effect grows over time and does not slow down. Municipalities located 20-25km from the treated border, as well as those more than 30 km away, show no systematic post-treatment response.

2.6.4 Extensions

Taken together, the main results show that Lex Bonny increases the number of firms, mainly through entry, but that the response is spatially heterogeneous. The effects remain concentrated in treated municipalities and in a subset of nearby areas. Three questions naturally follow our main findings. First, are the policy effects different across the urban hierarchy? Second, does the aggregate effect mainly reflect the response of smaller or larger firms? Third, do increases in firm activity translate into improvements in employment and the local tax base?

Heterogeneity - Urban hierarchy

We first examine heterogeneity by urban hierarchy. Table 2.3 shows that Lex Bonny increases the number of firms in suburban municipalities but has little effect in rural ones. This pattern suggests that place-based subsidies are most effective when they

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[20] The baseline regression with the full specification relies on 1,642 municipalities and 52,527 observations. When using only eligible municipalities and the 25-30km bins as controls, the number of municipalities drops to 293 and the number of observations to 9,508.

[21] The reported percentage effects are derived as follows: $100 \cdot \left(e^{0.514} - 1\right) = 67.2\%$ and $100 \cdot \left(e^{0.634} - 1\right) = 88.5\%$.

Table 2.3: Heterogeneity - Suburban and rural municipalities

	# of Firms (1)	Births (2)	Deaths (3)	Relocation In (4)	Relocation Out (5)
Long-term effect					
Rural municipalities	0.057	-0.066	-0.515	-0.424	0.336
	(0.142)	(0.225)	(0.552)	(0.539)	(0.516)
Suburban municipalities	0.604***	1.035***	0.829***	0.424	0.495
	(0.140)	(0.345)	(0.246)	(0.516)	(0.334)
# Observations	48417	47733	41715	39577	40464
# Municipalities	1427	1413	1252	1184	1191

Notes: Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

complement existing agglomeration advantages.

Lex Bonny lowers entry and expansion costs, but it does not remove the economic advantages of agglomerations. Suburban municipalities benefit from stronger agglomeration economies, so the subsidy is more likely to shift marginal location decisions there. In rural municipalities, by contrast, eligibility alone is less likely to generate additional entry or sustained growth in the number of firms. Suburban municipalities give firms access to metropolitan markets while avoiding some of the costs of urban centers, such as higher land prices, congestion, and space constraints. If the policy lowers costs in these municipalities, it can incentivize firms to locate near the city rather than in the urban core, especially when they value space and accessibility. Rural municipalities cannot offer the same combination of market access and lower costs because they are farther from major markets. As a result, the same subsidy generates a weaker response in small municipalities.

Overall, the pattern in Table 2.3 suggests that Bonny eligibility interacts strongly with existing agglomeration economies. The policy appears most effective when it reinforces existing agglomeration advantages, whereas it has limited effects in small markets.

Heterogeneity - Firm sizes

The policy effect depends not only on urban hierarchy, but also potentially on the type of firms that respond. Therefore, we next examine heterogeneity by firm size.

We use two measures of firm size. First, we classify firms by nominal capital using the nationwide distribution in each year. We distinguish firms in the bottom

50th percentile, between the 50th and 75th percentiles, between the 75th and 90th percentiles, between the 90th and 99th percentiles, and in the top 1% percentile. Percentiles are computed from the nationwide distribution of capital for each year. Second, we measure firm size using the size of the board of directors, as larger firms typically require more board members. Table 2.4 and 2.5 present the corresponding long-term estimates.

The nominal capital estimates are positive throughout the distribution, suggesting that the increase in the number of firms is not limited to small ones. However, the event-study profiles reveal violations of parallel trends in all capital-based specifications (see Table 2.B.6 in the Appendix). Consequently, we interpret these results cautiously and do not use nominal capital to make causal claims about heterogeneous treatment effects.

Table 2.4: Heterogeneity - Number of firms, by nominal capital

	Bottom 50th (1)		50-75th (2)		75-90th (3)		Top 10% (4)		90-99th (5)		Top 1% (6)	
Long-term effect - Bonny	0.885***	(0.205)	0.348**	(0.152)	0.272*	(0.160)	0.385***	(0.081)	0.388***	(0.087)	0.141	(0.366)
# Observations	33104		31081		25771		22520		22094		8285	
# Municipalities	1382		1299		1076		940		922		352	

Notes: The results show the long-term effect calculated as in Davidson and MacKinnon (2004), using Equation (2.1). The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

In contrast, heterogeneity by board size displays parallel pre-treatment dynamics (see Table 2.B.7 in the Appendix). The long-term coefficients indicate that the increase in firm activity is concentrated among firms with smaller boards, while firms with larger boards show much weaker responses. This pattern suggests that the Bonny program is associated with the presence or growth of smaller firms, complementing the aggregate results presented above. This pattern suggests that the policy primarily attracts smaller firms.

Table 2.5: Heterogeneity - Number of firms, by board size

	1 board member (1)		2 board members (2)		3 board members (3)		4 board members (4)		5 board members (5)		6+ board members (6)	
Long-term effect - Bonny	0.891***	(0.214)	0.591***	(0.170)	0.386*	(0.208)	0.056	(0.166)	0.196	(0.309)	-0.401	(0.310)
# Observations	32382		31779		31314		25716		21411		16437	
# Municipalities	1352		1325		1306		1073		894		689	

Notes: The results show the long-term effect calculated as in Davidson and MacKinnon (2004), using Equation (2.1). The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Effects on workers & taxpayers

If Lex Bonny mainly increases the number of small firms, the next question is whether this growth translates into broader local gains in employment and the tax base. To study these outcomes, we estimate a single 2x2 spatial DiD across all distance bins, following Gallé et al. (2024). Because these variables are observed only in 1979 and 1989, we use 1979 as the pre-treatment period and 1989 as the post-treatment period. We estimate the following specification:

$$Y_{it} = \exp\left(\sum_{d=0,d\neq 6}^{6} \beta_d (D_{d_i=d} \cdot Post_t) + \boldsymbol{\eta}(\mathbf{X}_i \cdot Post_t)' + Post_t + \xi_i + \xi_t\right) \cdot \epsilon_{it}, \quad (2.4)$$

where $d_i = 0$ indicates treated municipalities, and, for example, $d_i = 1$ indicates municipalities between 0 and 5km away from the Bonny borders. We omit municipalities located between 25km and 30km from the treatment regions (i.e., $d_i = 6$), which serve as the reference group. As before, we restrict the sample to treated municipalities and controls within 30km of the Bonny borders.

Tables 2.6-2.8 examine whether the increase in the number of firms inside the Bonny perimeter translates into broader local development outcomes, focusing on taxpayers and employment. A first result stands out: taxpayers and employment respond more diffusely across space than firms. Effects are almost nonexistent within treated municipalities, whereas some untreated distance bands exhibit clearer responses.

Table 2.6 shows a modest change in the total number of taxpayers inside Bonny municipalities in the bottom 50th percentile relative to the 25–30km reference group. The Bonny coefficient is near 0 for total taxpayers, while the bottom 50% income group increases by 4.92%. In contrast, the upper tail moves in the opposite direction. The top 10% category declines in Bonny municipalities. A second result is that the largest increases in the total number of taxpayers do not occur in treated municipalities. They occur primarily in nearby municipalities, especially the 5–10km and 20–25km bins, where the coefficients are positive across all brackets. Although the approach does not directly identify mechanisms, the fact that taxpayer gains appear more pronounced in nearby rings than in treated municipalities is consistent with commuting and residential sorting.

Table 2.7 turns to employment. It distinguishes between workplace- and residence-based employment and further splits each measure by education level. Within Bonny municipalities, total workplace-based employment does not increase across all education groups, except for the higher-education category. This result suggests that the additional firms attracted by the policy do not create local jobs.

Table 2.6: Effect on taxpayers

	Taxpayers				
	Total (1)	Bottom 50% (2)	50-75th percentiles (3)	75-90th percentiles (4)	Top 10% (5)
Long-term - Bonny	0.015	0.048**	-0.009	-0.019	-0.044
	(0.020)	(0.024)	(0.023)	(0.030)	(0.039)
Long-term - 0-5km	0.028	0.023	0.049**	0.038	-0.012
	(0.022)	(0.025)	(0.022)	(0.027)	(0.033)
Long-term - 5-10km	0.049**	0.040*	0.069***	0.057**	0.041
	(0.022)	(0.022)	(0.025)	(0.027)	(0.036)
Long-term - 10-15km	0.021	0.011	0.050**	0.023	-0.002
	(0.023)	(0.022)	(0.025)	(0.027)	(0.041)
Long-term - 15-20km	-0.005	-0.015	-0.006	-0.000	0.030
	(0.034)	(0.045)	(0.029)	(0.027)	(0.034)
Long-term - 20-25km	0.061***	0.069***	0.060***	0.047**	0.039
	(0.017)	(0.018)	(0.020)	(0.023)	(0.024)
Long-term - 25-30km	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)
# Observations	2608	2596	2488	2120	2120
# Municipalities	1304	1298	1244	1060	1060

Notes: The results show the long-term effect, estimated using Equation (2.4). The full specification includes year and unit fixed effects, and baseline characteristics. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

The picture is somewhat different for residence-based workers. Total employment among residents does not change meaningfully in treated municipalities, but the higher-education group registers a clear increase. Lex Bonny appears to have attracted or retained more educated residents, even in the absence of a corresponding expansion in local jobs. As in Table 2.6, the largest and most consistent effects appear in nearby municipalities rather than in treated ones.

Table 2.8 uses the Business Census to measure jobs in firms active in the secondary and tertiary sectors. Total employment in Bonny municipalities declines slightly, driven by decreases in the secondary sector, while the tertiary sector shows no meaningful change. The results point in the same direction as Table 2.7. Lex Bonny mainly attracts firms that contribute little to local employment. Effects outside the treated municipalities are again more pronounced, particularly in the 20–25km bin, where the total number of jobs and both second- and third-sector jobs increase.

Tables 2.6 to 2.8 point to the same conclusion. Lex Bonny reshapes the local firm landscape without improving the local labour market. The labor-market and taxpayer responses that do emerge appear to spread across nearby areas rather than concentrate within treated municipalities.

Table 2.7: Effect on workplace-based and residence-based

	Workplace-based workers				Residence-based workers			
	Total (1)	Low education (2)	Middle education (3)	High education (4)	Total (5)	Low education (6)	Middle education (7)	High education (8)
Long-term - Bonny	-0.052	-0.064	-0.035	0.084	0.034	-0.027	0.055	0.218***
	(0.045)	(0.050)	(0.044)	(0.056)	(0.036)	(0.036)	(0.045)	(0.045)
Long-term - 0-5km	0.029	-0.023	0.076	0.192***	0.073**	-0.018	0.121***	0.276***
	(0.047)	(0.058)	(0.050)	(0.057)	(0.035)	(0.041)	(0.042)	(0.043)
Long-term - 5-10km	0.026	0.009	0.039	0.115**	0.094***	0.037	0.107***	0.228***
	(0.043)	(0.056)	(0.038)	(0.048)	(0.036)	(0.041)	(0.039)	(0.042)
Long-term - 10-15km	0.042	0.034	0.033	0.130**	0.059	0.023	0.054	0.166***
	(0.043)	(0.063)	(0.040)	(0.055)	(0.039)	(0.051)	(0.047)	(0.052)
Long-term - 15-20km	0.029	0.035	0.018	0.081	0.059*	0.064*	0.046	0.136***
	(0.043)	(0.050)	(0.045)	(0.054)	(0.036)	(0.035)	(0.046)	(0.036)
Long-term - 20-25km	0.095**	0.043	0.119***	0.103**	0.103***	0.006	0.108***	0.080*
	(0.046)	(0.048)	(0.043)	(0.052)	(0.037)	(0.029)	(0.041)	(0.042)
Long-term - 25-30km	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
# Observations	2608	2608	2608	2606	2622	2622	2622	2622
# Municipalities	1304	1304	1304	1303	1311	1311	1311	1311

Notes: The results show the long-term effect, estimated using Equation (2.4). The full specification includes year and unit fixed effects, and baseline characteristics. Standard errors (in parentheses) are clustered at the district level. *** p < 0.01, ** p < 0.05, and * p < 0.10.

As mentioned, one limitation is that the spatial DiD is implemented as a two-period comparison, which does not allow us to visually check the pre-treatment trends in these outcomes. The interpretation, therefore, relies on the assumption that, absent treatment, treated municipalities and the distance bins would have followed similar decade-long trends.

Table 2.8: Effect on workers (Business Census)

	Jobs in 2nd and 3rd sector					
	Total (1)		2nd sector (2)		3rd sector (3)	
Long-term - Bonny	-0.086**	(0.035)	-0.048	(0.047)	0.017	(0.026)
Long-term - 0-5km	0.020	(0.034)	0.060	(0.048)	0.031	(0.033)
Long-term - 5-10km	-0.010	(0.035)	0.023	(0.044)	0.029	(0.034)
Long-term - 10-15km	0.032	(0.029)	0.060	(0.043)	0.034	(0.029)
Long-term - 15-20km	0.017	(0.028)	0.022	(0.038)	0.033	(0.043)
Long-term - 20-25km	0.069*	(0.038)	0.081*	(0.049)	0.109***	(0.036)
Long-term - 25-30km	0.000	(.)	0.000	(.)	0.000	(.)
# Observations	2620		2604		2618	
# Municipalities	1310		1302		1309	

Notes: The results show the long-term effect, estimated using Equation (2.4). The full specification includes year and unit fixed effects, and baseline characteristics. Standard errors (in parentheses) are clustered at the district level. *** p < 0.01, ** p < 0.05, and * p < 0.10.

2.7 Robustness checks

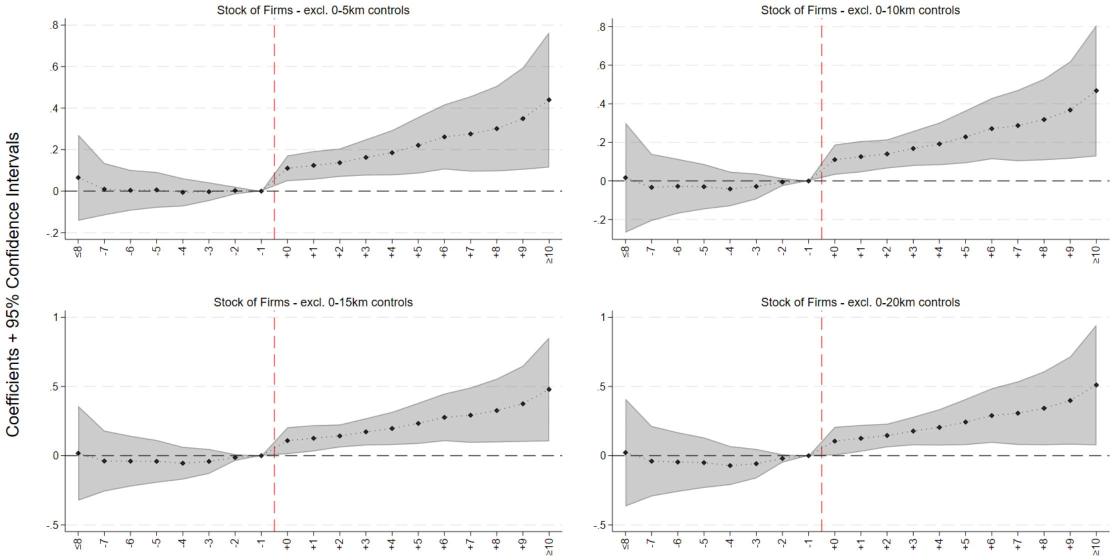
2.7.1 Exclusion of close controls

To assess whether the baseline estimates are driven by spatial spillovers or by contamination of the control group, we re-estimate the baseline specifications while progressively excluding control municipalities located near treated areas (Neumark and Kolko 2010, Kline and Moretti 2014). Specifically, we exclude control units within 0–5 km, 0–10 km, 0–15 km, and 0–20 km of treated municipalities. This is a comparison between treated units and untreated controls, from which we first stepwise exclude the municipalities most exposed to spillovers. By doing so, we focus on the treatment effects on the treated, without initially losing too much on comparability while diminishing spillover concerns. Figure 2.10 reports the event-study estimates, and Table 2.B.3 in the Appendix reports the full regression results.

Figure 2.10 shows that the main pattern remains unchanged across all doughnut specifications. Pre-trends continue to show no evidence of differential parallel trends violations after excluding nearby controls. Although excluding nearby controls reduces the sample size, the estimated post-treatment effects remain broadly similar. Point estimates increase as closer controls are removed. This is consistent with spatial spillovers that attenuate the baseline estimates. Overall, the effects appear robust to alternative definitions of the control group. The pattern is comparable to the baseline estimates, indicating that our main results are not driven by municipalities close to the treatment borders. Overall, the results remain robust to alternative control groups and support the view that Lex Bonny increases local firm activity rather than redistributing firms across space.

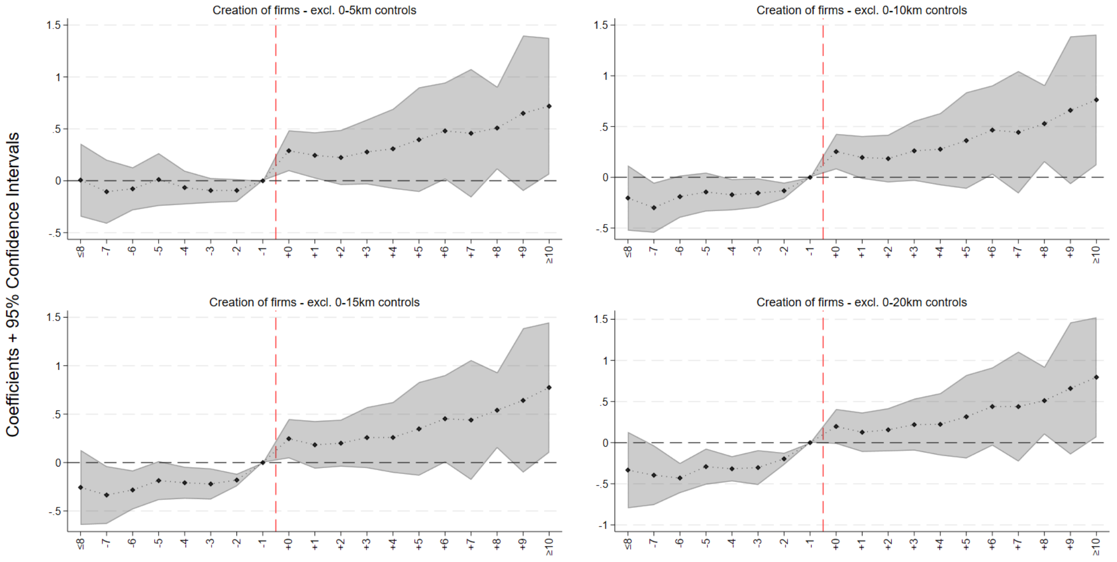
Figure 2.11 displays a rise in post-treatment coefficients when excluding nearby controls. This is consistent with positive spillovers to close control municipalities. For firm entry, the doughnut design reveals a sharper trade-off. Excluding nearby controls increases post-treatment coefficients, but it also worsens pre-treatment alignment. More distant municipalities appear less suitable as counterfactuals for entry dynamics.

Figure 2.10: Cumulative effect of Lex Bonny, excl. close controls - Number of firms



Notes: The figure presents our results estimating Equation (2.1) for the full specification. The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy. We progressively exclude control units located within 0-5km (top left), 0-10km (top right), 0-15km (bottom left), 0-20km (bottom right) from the control group. Table 2.B.3 in the Appendix provides all the details.

Figure 2.11: Cumulative effect of Lex Bonny, excl. close controls - Creation of firms



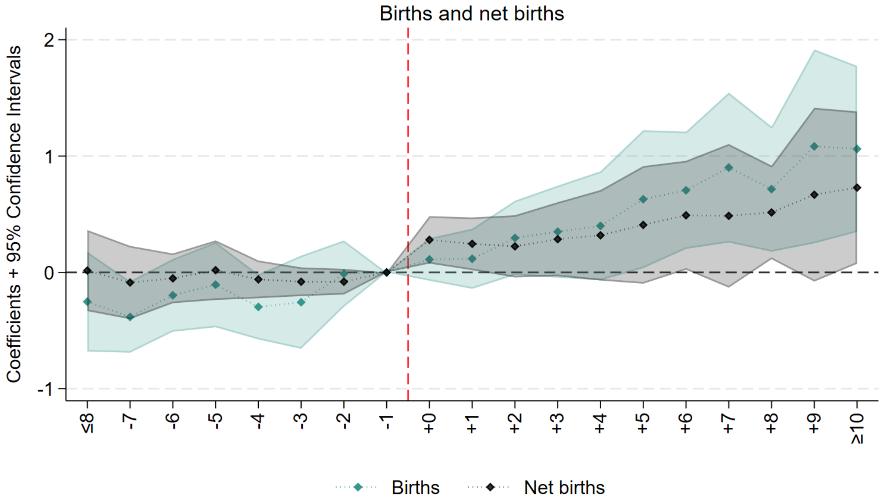
Notes: The figure presents our results estimating Equation (2.1) when using the creation of firms as the outcome (and the full specification). The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy. We progressively exclude control units located within 0-5km (top left), 0-10km (top right), 0-15km (bottom left), 0-20km (bottom right) from the control group.

2.7.2 Net births and net deaths

As a further robustness check, we construct "*net births*" and "*net deaths*" variables to mitigate potential overcounting of creations arising from false negatives in the matching procedure. When the procedure misses a true match, the same firm may appear as a death in one period and as a birth in the next. The net measures reduce the influence of such birth-death overcounting and provide outcomes less sensitive to remaining matching errors. We define "*net births*" as the number of firms created in period t minus the number of firm deaths in period $t-1$. We define "*net deaths*" as the number of firm deaths in period t minus the number of creations in period $t+1$[22].

The *net births* results closely match the baseline creation estimates, although they are less precise. This suggests that matching errors do not drive the entry results. The *net deaths* results differ more from the baseline death estimates, but they still show weak pre-trend alignment and little post-treatment response. Since the baseline death results are already inconclusive, this change does not affect the main interpretation.

Figure 2.12: Cumulative effect of Lex Bonny - Net births

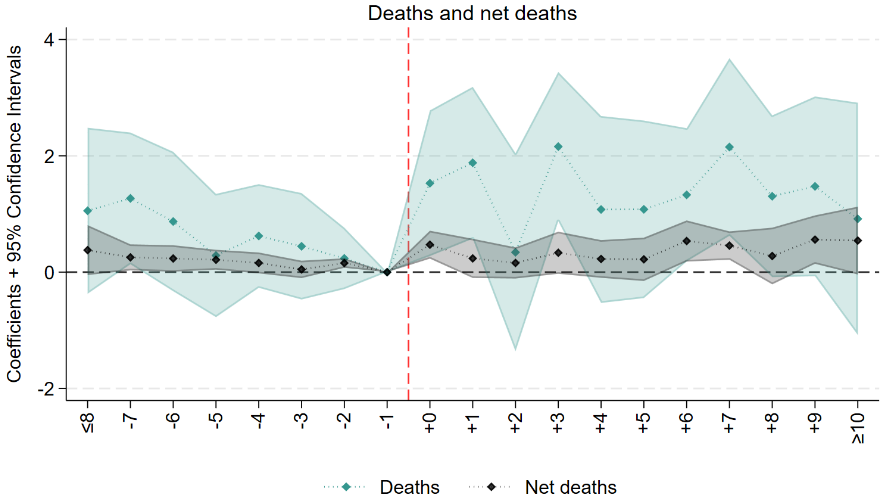


Notes: The figure presents our results estimating Equation (2.1) when using the creation of firms and net births as the outcome (for the full specification). The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy.

---

[22] "Both *net births*" and "*net deaths*" can take negative values, which are incompatible with PQML estimation. We recode negative values to zero. For "*net births*", negative values are rare given the predominantly growing firm population over the period, making this a minor concern. For "*net deaths*", the growth trend suggests more frequent negative values after recoding; therefore, these results should be interpreted with caution and are reported for completeness only. These net measures can also remove some genuine sequences of exit and entry, so they should be interpreted as robustness rather than as preferred outcomes.

Figure 2.13: Cumulative effect of Lex Bonny - Net deaths



Notes: The figure presents our results estimating Equation (2.1) when using the death of firms and net deaths as the outcome (for the full specification). The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy.

2.8 Interpretation of the results and discussion

The results indicate that Lex Bonny achieves its main goal of increasing the number of firms in targeted regions. The number of firms increases, with the increase driven mainly by firm entry. The response is strongest among smaller firms. This pattern is consistent with a policy that lowers fixed entry and investment costs in disadvantaged regions and therefore operates primarily at the extensive margin. By contrast, the evidence on local economic outcomes is weaker. Although more firms locate in treated municipalities, the results for workers and taxpayers do not indicate an increase in local employment or fiscal capacity. This gap is informative because the policy design rests on the assumption that increased firm activity ultimately generates jobs and broadens the local tax base.

Several mechanisms can reconcile these findings. First, the policy mainly attracts smaller firms, which likely generate fewer jobs and less tax revenue than larger ones. Second, the policy may have affected where activity is registered more than where production takes place. Some of the additional firms may consist of small legal entities or administrative relocations, thereby reconciling more firms with no employment responses. Third, some gains may benefit nearby untreated municipalities rather than remain within treated borders. Firms in Bonny municipalities may draw workers from neighboring areas, which spreads labor-market effects across the treatment borders. Fourth, the non-monotonic spatial profile is consistent with diverted entry at the policy boundary: some firms may choose to locate just inside the Bonny perimeter rather than just outside it in order to capture subsidies while preserving access to the same local market. Finally, some of the increase in firm counts may reflect transitions into self-employment rather than the creation of larger employing firms.

The taxpayer results point in the same direction. The policy appears to increase the number of taxpayers, mainly in lower-income brackets, without increasing the number in higher-income brackets. This pattern is consistent with a scenario in which the policy raises the number of taxpayers without generating a comparable increase in fiscal capacity per capita.

Finally, the policy does not reduce disparities between suburban and rural municipalities within the Bonny regions. Lex Bonny has much stronger effects in suburban municipalities than in rural ones. Place-based subsidies appear to complement agglomeration advantages. When the same policy targets both suburban and rural municipalities, firms respond more strongly in places that already offer these advantages. If policymakers also aim to reduce disparities within disadvantaged regions, they should account for the constraints faced by rural municipalities.

2.9 Conclusion

This chapter examines the long-run effects of Lex Bonny, a Swiss place-based policy that subsidizes private investment in distressed municipalities. Using newly digitized firm-level data covering the universe of Swiss corporations, we estimate the effects of eligibility on the number of firms and firm dynamics between 1960 and 1993. The empirical design combines dynamic difference-in-differences estimates with spatial comparisons to assess both local effects and spillovers.

Our results suggest three main findings. First, Lex Bonny increases the number of firms in eligible municipalities, and the effect grows gradually over time. Entries drive the increase in the number of firms. Second, the policy does not appear to generate large displacement effects. At the same time, the non-monotonic spatial profile is consistent with some diverted entry from the closest untreated municipalities into the treated area. Third, the aggregate firm response is concentrated among smaller firms and is much stronger in suburban than in rural municipalities. These results suggest that Lex Bonny is most effective where agglomeration economies are already larger.

This chapter also examines effects on employment and the number of taxpayers. The results show that more firms do not translate into broader improvements in local employment. Taxpayer responses are modest and concentrated at the bottom of the income distribution. These findings suggest that Lex Bonny raises firm density more than it raises local labor-market activity or fiscal capacity.

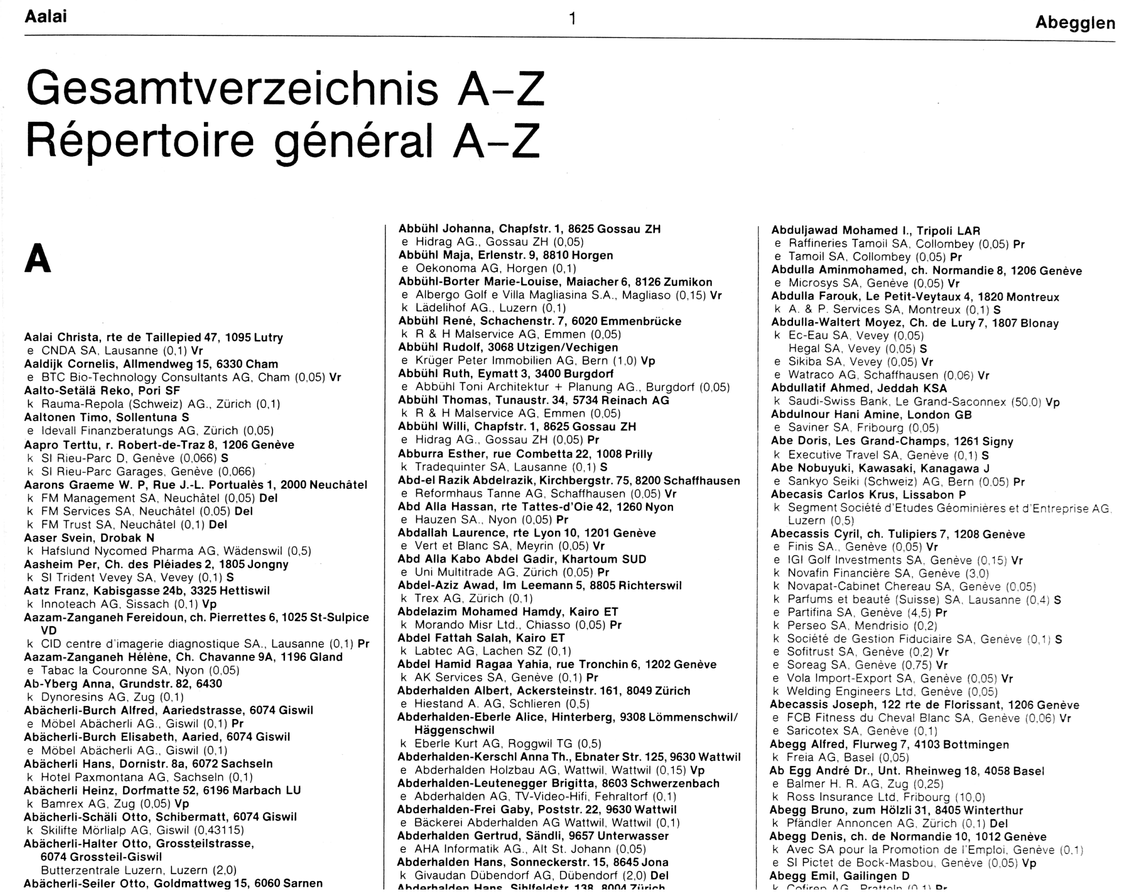
Overall, the chapter shows that place-based investment subsidies can durably increase firm presence in targeted areas, but that these gains need not generate broad local multiplier effects. The evidence points to a narrower effect of Lex Bonny than its policy goal: the program succeeds in attracting firms, especially smaller ones, but it does not produce equal gains in jobs or fiscal capacity.

Appendices

2.A Data

2.A.1 Raw data example

Figure 2.A.1: Example : "*Répertoire des administrateurs des sociétés anonymes suisses*"



Aalai 1 **Abegglen**

Gesamtverzeichnis A–Z
Répertoire général A–Z

A

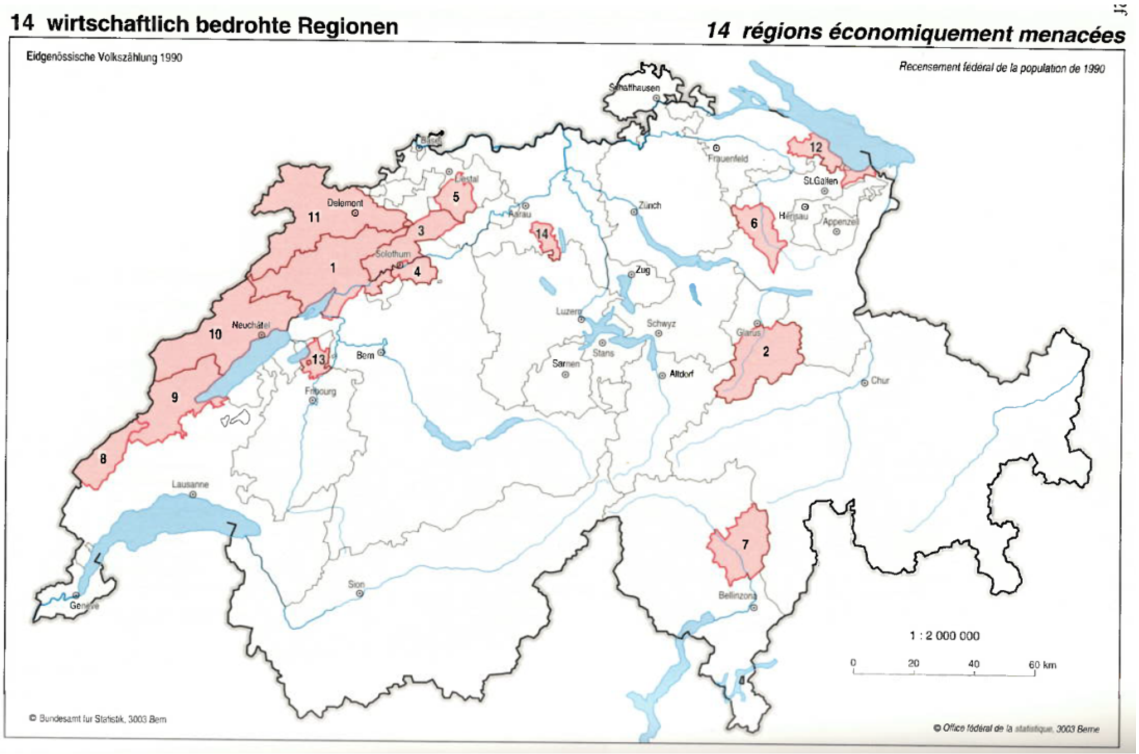
Aalai Christa, rte de Taillepied 47, 1095 Lutry
e CNDA SA, Lausanne (0,1) **Vr**
Aaldijk Cornelis, Allmendweg 15, 6330 Cham
e BTC Bio-Technology Consultants AG, Cham (0,05) **Vr**
Aalto-Setälä Reko, Pori SF
k Rauma-Repola (Schweiz) AG., Zürich (0,1)
Aaltonen Timo, Sollentuna S
e Idevall Finanzberatungs AG, Zürich (0,05)
Aapro Terttu, r. Robert-de-Traz 8, 1206 Genève
k SI Rieu-Parc D, Genève (0,066) **S**
k SI Rieu-Parc Garages, Genève (0,066)
Aarons Graeme W. P, Rue J.-L. Portualès 1, 2000 Neuchâtel
k FM Management SA, Neuchâtel (0,05) **Del**
k FM Services SA, Neuchâtel (0,05) **Del**
k FM Trust SA, Neuchâtel (0,1) **Del**
Aaser Svein, Drobak N
k Hafslund Nycomed Pharma AG, Wädenswil (0,5)
Aasheim Per, Ch. des Pléiades 2, 1805 Jongny
k SI Trident Vevey SA, Vevey (0,1) **S**
Aatz Franz, Kabisgasse 24b, 3325 Hettiswil
k Innoteach AG, Sissach (0,1) **Vp**
Aazam-Zanganeh Fereidoun, ch. Pierrettes 6, 1025 St-Sulpice VD
k CID centre d'imagerie diagnostique SA., Lausanne (0,1) **Pr**
Aazam-Zanganeh Hélène, Ch. Chavanne 9A, 1196 Gland
e Tabac la Couronne SA, Nyon (0,05)
Ab-Yberg Anna, Grundstr. 82, 6430
k Dynoresins AG, Zug (0,1)
Abächerli-Burch Alfred, Aariedstrasse, 6074 Giswil
e Möbel Abächerli AG., Giswil (0,1) **Pr**
Abächerli-Burch Elisabeth, Aaried, 6074 Giswil
e Möbel Abächerli AG., Giswil (0,1)
Abächerli Hans, Dornistr. 8a, 6072 Sachseln
k Hotel Paxmontana AG, Sachseln (0,1)
Abächerli Heinz, Dorfmatte 52, 6196 Marbach LU
k Bamrex AG, Zug (0,05) **Vp**
Abächerli-Schäli Otto, Schibermatt, 6074 Giswil
k Skilifte Mörlialp AG, Giswil (0,43115)
Abächerli-Halter Otto, Grossteilstrasse, 6074 Grossteil-Giswil
Butterzentrale Luzern, Luzern (2,0)
Abächerli-Seiler Otto, Goldmattweg 15, 6060 Sarnen
Abbühl Johanna, Chapfstr. 1, 8625 Gossau ZH
e Hidrag AG., Gossau ZH (0,05)
Abbühl Maja, Erlenstr. 9, 8810 Horgen
e Oekonoma AG, Horgen (0,1)
Abbühl-Borter Marie-Louise, Maiacher 6, 8126 Zumikon
e Albergo Golf e Villa Magliasina S.A., Magliaso (0,15) **Vr**
k Lädelihof AG., Luzern (0,1)
Abbühl René, Schachenstr. 7, 6020 Emmenbrücke
k R & H Malservice AG, Emmen (0,05)
Abbühl Rudolf, 3068 Utzigen/Vechigen
e Krüger Peter Immobilien AG, Bern (1,0) **Vp**
Abbühl Ruth, Eymatt 3, 3400 Burgdorf
e Abbühl Toni Architektur + Planung AG., Burgdorf (0,05)
Abbühl Thomas, Tunaustr. 34, 5734 Reinach AG
k R & H Malservice AG, Emmen (0,05)
Abbühl Willi, Chapfstr. 1, 8625 Gossau ZH
e Hidrag AG., Gossau ZH (0,05) **Pr**
Abburra Esther, rue Combetta 22, 1008 Prilly
k Tradequinter SA, Lausanne (0,1) **S**
Abd-el Razik Abdelrazik, Kirchbergstr. 75, 8200 Schaffhausen
e Reformhaus Tanne AG, Schaffhausen (0,05) **Vr**
Abd Alla Hassan, rte Tattes-d'Oie 42, 1260 Nyon
e Hauzen SA., Nyon (0,05) **Pr**
Abdallah Laurence, rte Lyon 10, 1201 Genève
e Vert et Blanc SA, Meyrin (0,05) **Vr**
Abd Alla Kabo Abdel Gadir, Khartoum SUD
e Uni Multitrade AG, Zürich (0,05) **Pr**
Abdel-Aziz Awad, Im Leemann 5, 8805 Richterswil
k Trex AG, Zürich (0,1)
Abdelazim Mohamed Hamdy, Kairo ET
k Morando Misr Ltd., Chiasso (0,05) **Pr**
Abdel Fattah Salah, Kairo ET
k Labtec AG, Lachen SZ (0,1)
Abdel Hamid Ragaa Yahia, rue Tronchin 6, 1202 Genève
k AK Services SA, Genève (0,1) **Pr**
Abderhalden Albert, Ackersteinstr. 161, 8049 Zürich
e Hiestand A. AG, Schlieren (0,5)
Abderhalden-Eberle Alice, Hinterberg, 9308 Lömmenschwil/Häggenschwil
k Eberle Kurt AG, Roggwil TG (0,5)
Abderhalden-Kerschl Anna Th., Ebnater Str. 125, 9630 Wattwil
e Abderhalden Holzbau AG, Wattwil, Wattwil (0,15) **Vp**
Abderhalden-Leutenegger Brigitta, 8603 Schwerzenbach
e Abderhalden AG, TV-Video-Hifi, Fehraltorf (0,1)
Abderhalden-Frei Gaby, Poststr. 22, 9630 Wattwil
e Bäckerei Abderhalden AG Wattwil, Wattwil (0,1)
Abderhalden Gertrud, Sändli, 9657 Unterwasser
e AHA Informatik AG., Alt St. Johann (0,05)
Abderhalden Hans, Sonneckerstr. 15, 8645 Jona
k Givaudan Dübendorf AG, Dübendorf (2,0) **Del**
Abderhalden Hans, Sihlfeldstr. 138, 8004 Zürich
Abduljawad Mohamed I., Tripoli LAR
e Raffineries Tamoil SA, Collombey (0,05) **Pr**
e Tamoil SA, Collombey (0,05) **Pr**
Abdulla Aminmohamed, ch. Normandie 8, 1206 Genève
e Microsys SA, Genève (0,05) **Vr**
Abdulla Farouk, Le Petit-Veytaux 4, 1820 Montreux
k A. & P. Services SA, Montreux (0,1) **S**
Abdulla-Waltert Moyez, Ch. de Lury 7, 1807 Blonay
k Ec-Eau SA, Vevey (0,05)
Hegal SA, Vevey (0,05) **S**
e Sikiba SA, Vevey (0,05) **Vr**
e Watraco AG, Schaffhausen (0,06) **Vr**
Abdullatif Ahmed, Jeddah KSA
k Saudi-Swiss Bank, Le Grand-Saconnex (50,0) **Vp**
Abdulnour Hani Amine, London GB
e Saviner SA, Fribourg (0,05)
Abe Doris, Les Grand-Champs, 1261 Signy
k Executive Travel SA, Genève (0,1) **S**
Abe Nobuyuki, Kawasaki, Kanagawa J
e Sankyo Seiki (Schweiz) AG, Bern (0,05) **Pr**
Abecasis Carlos Krus, Lissabon P
k Segment Société d'Etudes Géominières et d'Entreprise AG, Luzern (0,5)
Abecassis Cyril, ch. Tulipiers 7, 1208 Genève
e Finis SA., Genève (0,05) **Vr**
e IGI Golf Investments SA, Genève (0,15) **Vr**
k Novafin Financière SA, Genève (3,0)
k Novapat-Cabinet Chereau SA, Genève (0,05)
k Parfums et beauté (Suisse) SA, Lausanne (0,4) **S**
e Partifina SA, Genève (4,5) **Pr**
k Perseo SA, Mendrisio (0,2)
k Société de Gestion Fiduciaire SA, Genève (0,1) **S**
e Sofitrust SA, Genève (0,2) **Vr**
e Soreag SA, Genève (0,75) **Vr**
e Vola Import-Export SA, Genève (0,05) **Vr**
k Welding Engineers Ltd, Genève (0,05)
Abecassis Joseph, 122 rte de Florissant, 1206 Genève
e FCB Fitness du Cheval Blanc SA, Genève (0,06) **Vr**
e Saricotex SA, Genève (0,1)
Abegg Alfred, Flurweg 7, 4103 Bottmingen
k Freia AG, Basel (0,05)
Ab Egg André Dr., Unt. Rheinweg 18, 4058 Basel
e Balmer H. R. AG, Zug (0,25)
k Ross Insurance Ltd, Fribourg (10,0)
Abegg Bruno, zum Hölzli 31, 8405 Winterthur
k Pfändler Annoncen AG, Zürich (0,1) **Del**
Abegg Denis, ch. de Normandie 10, 1012 Genève
k Avec SA pour la Promotion de l'Emploi, Genève (0,1)
e SI Pictet de Bock-Masbou, Genève (0,05) **Vp**
Abegg Emil, Gailingen D
k Cofiren AG., Pratteln (0,1) **Pr**

Notes: Example of a page in the publications illustrating raw data before digitalization.

2.A.2 Documentations on the treatment coverage

Ordinance listing the municipalities covered by the initial Lex Bonny in 1979

Figure 2.A.2: Map of the municipalities covered by Lex Bonny in 1990



Notes: The map shows the state in 1990 according to the Federal Statistical Office (Swiss Federal Statistical Office 1990).

Décision concernant la détermination de régions dont l'économie est menacée

du 9 mai 1979

Le Département fédéral de l'économie publique,

vu l'article 3 de l'ordonnance du 21 février 1979[1] sur l'aide financière en faveur des régions dont l'économie est menacée,

décide:

Article premier

Sont réputées régions menacées au sens de l'arrêté fédéral du 6 octobre 1978[2] instituant une aide financière en faveur des régions dont l'économie est menacée, les régions suivantes:

a. Dans le canton de Berne:
   - les districts de Bienne, Büren (à l'exception des communes de Büetigen, Busswil bei Büren, Diessbach bei Büren, Dotzigen et Wengi), Courtelary, Moutier, La Neuveville et Nidau (à l'exception des communes de Bühl, Walperswil et Worben).

b. Dans le canton de Glaris:
   - les communes de Betschwanden, Braunwald, Diesbach, Elm, Engi, Haslen, Hätzingen, Leuggelbach, Linthal, Luchsingen, Matt, Mitlödi, Nidfurn, Rüti, Schwanden, Schwändi et Sool.

c. Dans le canton de Soleure:
   - les districts de Balsthal-Thal, Kriegstetten (à l'exception de la commune de Steinhof), Lebern (à l'exception de la commune de Kammersrohr) et Soleure,
   - les communes de Lüsslingen et de Nennigkofen.

d. Dans le canton de Bâle-Campagne:
   - le district de Waldenburg (à l'exception des communes de Bretzwil et de Lauwil),
   - les communes de Bubendorf, Itingen, Lausen, Ramlinsburg, Tenniken, Ziefen et Zunzgen.

e. Dans le canton de Saint-Gall:
   - les communes de Bütschwil, Ebnat-Kappel, Ganterschwil, Krinau, Lichtensteig, Mosnang, Oberhelfenschwil et Wattwil.

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[1] RO **1979** 246
[2] RO **1979** 240

f. Dans le canton du Tessin:
   - le district de Riviera (à l'exception de la commune de Claro),
   - les communes de Bodio, Dongio, Giornico, Ludiano, Malvaglia, Personico, Pollegio et Semione.

g. Dans le canton de Vaud:
   - les districts de Grandson, Orbe (à l'exception des communes de Bavois et de Corcelles-sur-Chavornay) et de la Vallée,
   - les communes de Chamblon, Champvent, Cheseaux-Noréaz, Essert-sous-Champvent, Gressy, Mathod, Montagny-près-Yverdon, Orges, Suscévaz, Treycovagnes, Valeyres-sous-Montagny, Villars-sous-Champvent, Vugelles-la-Mothe, Yverdon et Yvonand.

h. Le canton de Neuchâtel.

i. Le canton du Jura.

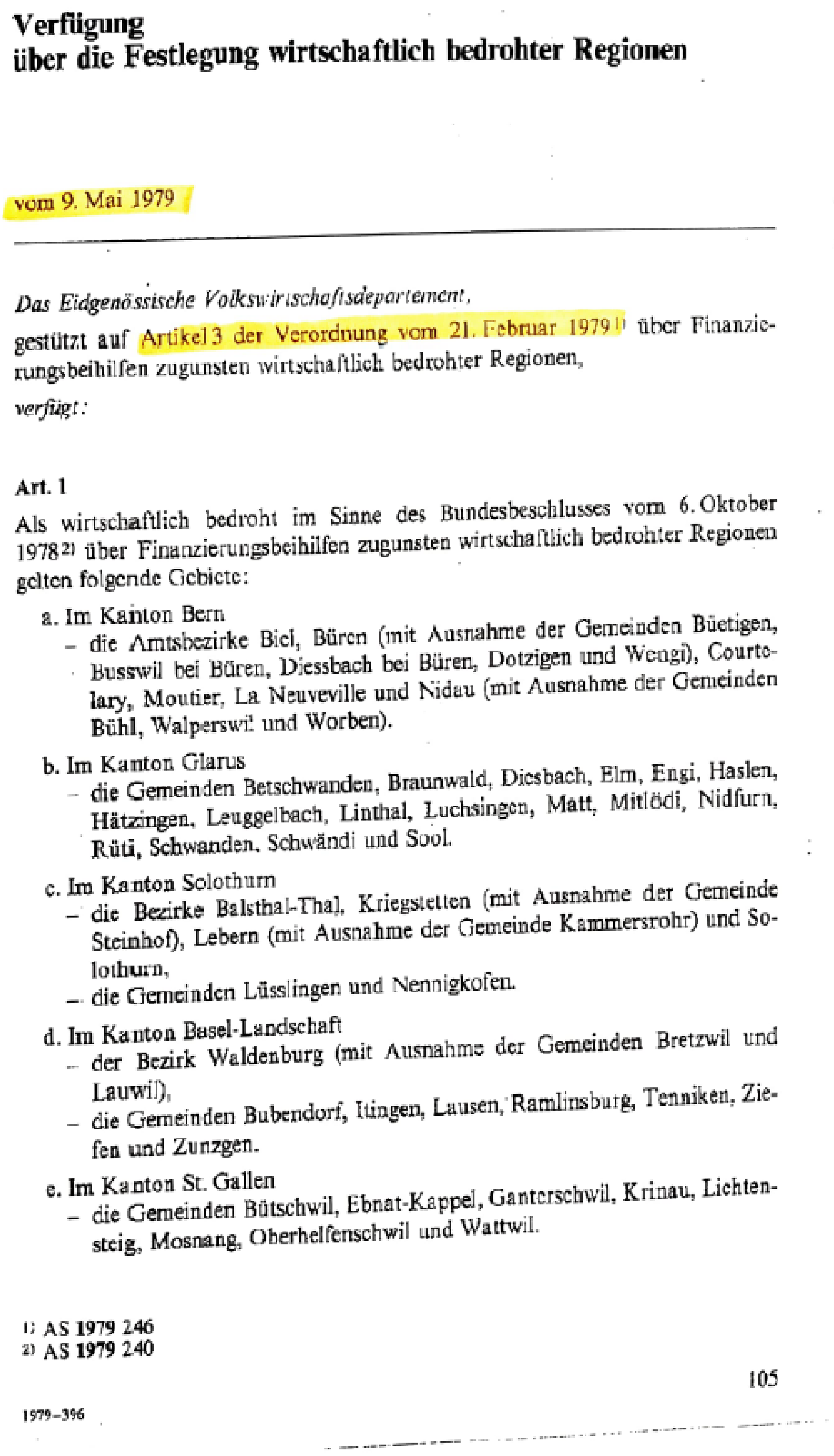
Art. 2

La présente décision entre en vigueur dès sa publication dans la Feuille fédérale. Elle peut être attaquée par voie de recours au Conseil fédéral dans un délai de 30 jours à dater de sa publication.

9 mai 1979

Département fédéral de l'économie publique:
Honegger

25203



Verfügung
über die Festlegung wirtschaftlich bedrohter Regionen

vom 9. Mai 1979

Das Eidgenössische Volkswirtschaftsdepartement,

gestützt auf Artikel 3 der Verordnung vom 21. Februar 1979[1] über Finanzierungsbeihilfen zugunsten wirtschaftlich bedrohter Regionen,

verfügt:

Art. 1

Als wirtschaftlich bedroht im Sinne des Bundesbeschlusses vom 6. Oktober 1978[2] über Finanzierungsbeihilfen zugunsten wirtschaftlich bedrohter Regionen gelten folgende Gebiete:

a. Im Kanton Bern
  - die Amtsbezirke Biel, Büren (mit Ausnahme der Gemeinden Büetigen, Busswil bei Büren, Diessbach bei Büren, Dotzigen und Wengi), Courtelary, Moutier, La Neuveville und Nidau (mit Ausnahme der Gemeinden Bühl, Walperswil und Worben).

b. Im Kanton Glarus
  - die Gemeinden Betschwanden, Braunwald, Diesbach, Elm, Engi, Haslen, Hätzingen, Leuggelbach, Linthal, Luchsingen, Matt, Mitlödi, Nidfurn, Rüti, Schwanden, Schwändi und Sool.

c. Im Kanton Solothurn
  - die Bezirke Balsthal-Thal, Kriegstetten (mit Ausnahme der Gemeinde Steinhof), Lebern (mit Ausnahme der Gemeinde Kammersrohr) und Solothurn,
  - die Gemeinden Lüsslingen und Nennigkofen.

d. Im Kanton Basel-Landschaft
  - der Bezirk Waldenburg (mit Ausnahme der Gemeinden Bretzwil und Lauwil),
  - die Gemeinden Bubendorf, Itingen, Lausen, Ramlinsburg, Tenniken, Ziefen und Zunzgen.

e. Im Kanton St. Gallen
  - die Gemeinden Bütschwil, Ebnat-Kappel, Ganterschwil, Krinau, Lichtensteig, Mosnang, Oberhelfenschwil und Wattwil.

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[1] AS 1979 246
[2] AS 1979 240

f. Im Kanton Tessin
  - der Bezirk Riviera (mit Ausnahme der Gemeinde Claro),
  - die Gemeinden Bodio, Dongio, Giornico, Ludiano, Malvaglia, Personico, Pollegio und Semione.

g. Im Kanton Waadt
  - die Bezirke Grandson, Orbe (mit Ausnahme der Gemeinden Bavois und Corcelles-sur-Chavornay) und La Vallée,
    die Gemeinden Chamblon, Champvent, Cheseaux-Noréaz, Essert-sous-Champvent, Gressy, Mathod, Montagny-près-Yverdon, Orges, Suscévaz, Treycovagnes, Valeyres-sous-Montagny, Villars-sous-Champvent, Vugelles-la-Mothe, Yverdon und Yvonand.

h. Der Kanton Neuenburg.

i. Der Kanton Jura.

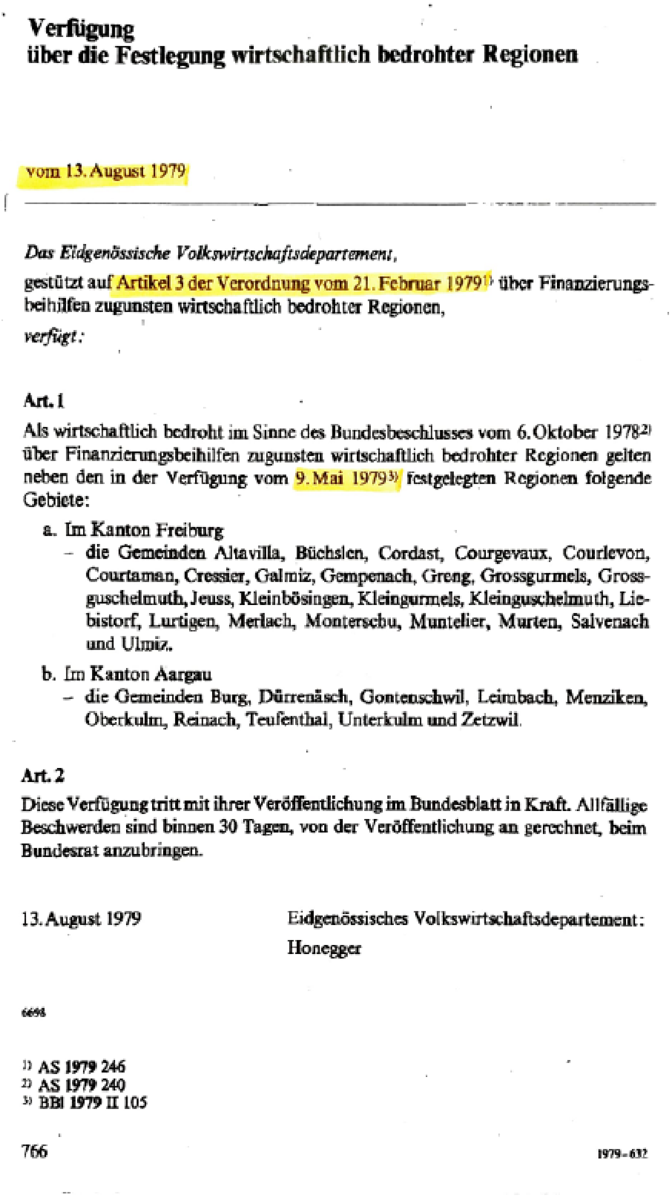
Art. 2

Diese Verfügung tritt mit ihrer Veröffentlichung im Bundesblatt in Kraft. Allfällige Beschwerden sind binnen 30 Tagen, von der Veröffentlichung an gerechnet, beim Bundesrat anzubringen.

9. Mai 1979

Eidgenössisches Volkswirtschaftsdepartement:
Honegger

6548



Verfügung über die Festlegung wirtschaftlich bedrohter Regionen

vom 13. August 1979

Das Eidgenössische Volkswirtschaftsdepartement,

gestützt auf Artikel 3 der Verordnung vom 21. Februar 1979[1] über Finanzierungsbeihilfen zugunsten wirtschaftlich bedrohter Regionen,

verfügt:

Art. 1

Als wirtschaftlich bedroht im Sinne des Bundesbeschlusses vom 6. Oktober 1978[2] über Finanzierungsbeihilfen zugunsten wirtschaftlich bedrohter Regionen gelten neben den in der Verfügung vom 9. Mai 1979[3] festgelegten Regionen folgende Gebiete:

a. Im Kanton Freiburg
   - die Gemeinden Altavilla, Büchslen, Cordast, Courgevaux, Courlevon, Courtaman, Cressier, Galmiz, Gempenach, Greng, Grossgurmels, Grossguschelmuth, Jeuss, Kleinbösingen, Kleingurmels, Kleinguschelmuth, Liebistorf, Lurtigen, Merlach, Monterschu, Muntelier, Murten, Salvenach und Ulmiz.

b. Im Kanton Aargau
   - die Gemeinden Burg, Dürrenäsch, Gontenschwil, Leimbach, Menziken, Oberkulm, Reinach, Teufenthal, Unterkulm und Zetzwil.

Art. 2

Diese Verfügung tritt mit ihrer Veröffentlichung im Bundesblatt in Kraft. Allfällige Beschwerden sind binnen 30 Tagen, von der Veröffentlichung an gerechnet, beim Bundesrat anzubringen.

13. August 1979 — Eidgenössisches Volkswirtschaftsdepartement:

Honegger

6698

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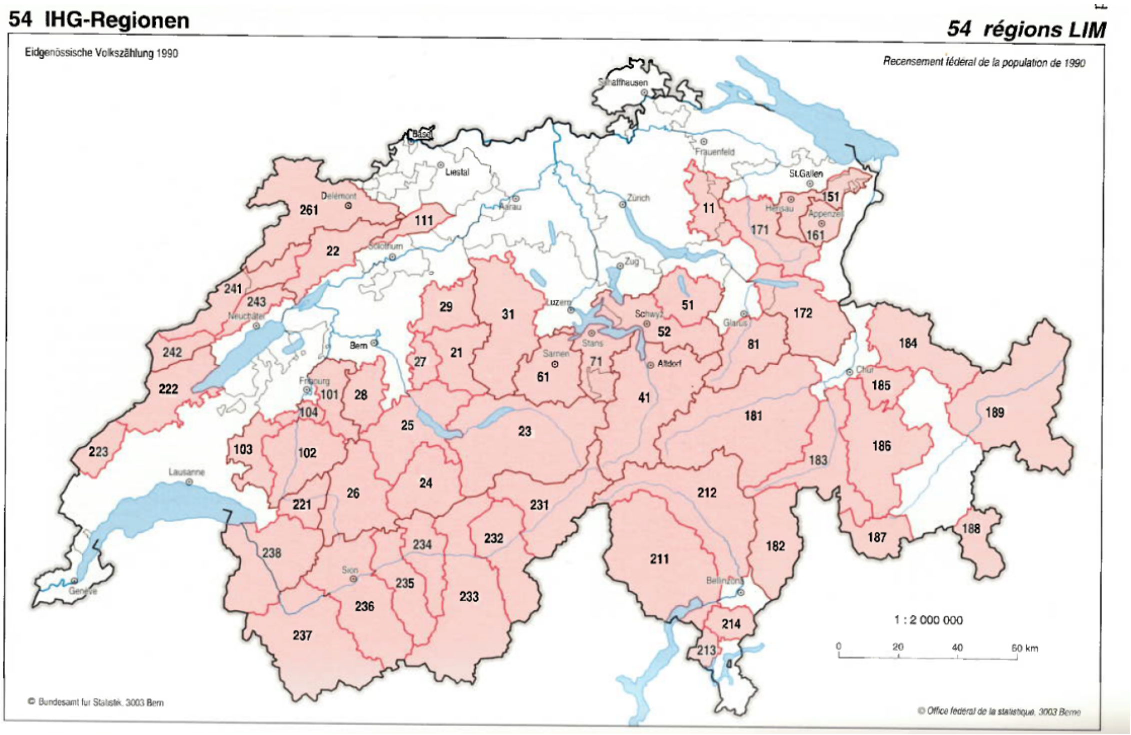
[1] AS 1979 246
[2] AS 1979 240
[3] BBl 1979 II 105

Coverage of The 1974 Federal Law on Aid for Infrastructure Investments in Mountain Areas

Figure 2.A.3: Map of the municipalities covered by LIM in 1990



Notes: The map shows the state in 1990 according to the Federal Statistical Office (Swiss Federal Statistical Office 1990).

Tableau 9: Régions LIM 2000

Code	Nom de la région LIM	Nombre de communes	Numéros des communes
11	Zürcher Berggebiet	15	111,114,117,120,171,179,181-182,222,226,228,3333,3337,4721,4726
21	Oberes Emmental	10	613,901-909
22	Jura-Bienne	42	371-372,431,433,436,438-440,442,444,447,681-684,687,690-692,694,696-697 699-704,706-715,721-725
23	Oberland-Ost	29	571-582,584-594,781-786
24	Kandertal	5	561,563-565,567
25	Thun-Innertport	40	562,566,761-769,871,885,921-947
26	Obersimmental-Saanenland	7	791-794,841-843
27	Kiesental	20	601-610,612,614-615,617,619-620,624,626,628-629
28	Schwarzwasser	11	357,851-854,864,877,879-880,882,887
29	Trachselwald	17	322,326,335,339,406-407,424,951-960
31	Luzerner Berggebiet	36	1001-1009,1083,1086,1098,1107,1121-1124,1126-1133,1135-1138,1143-1146 1148-1150
41	Uri	20	1201-1220
51	Einsiedeln	7	1301,1343,1348,1361,1368,1370,1375
52	Innerschwyz	16	1056,1068-1069,1311,1331,1362-1367,1369,1371-1374
61	Sarneraatal	6	1401,1403-1407
71	Nidwalden	12	1402,1501-1511
81	Glarner Hinterland-Sernftal	17	1601,1603-1606,1610-1616,1621,1626-1629
101	Sense	19	2291-2296,2298-2310
102	Gruyère	40	2121-2161
103	Glâne-Veveyse	51	2061-2064,2066-2067,2069,2071-2072,2074-2075,2077,2079,2081-2083, 2085-2087,2089,2091-2092,2094-2097,2099-2100,2102-2103,2105, 2107-2108,2110,2112-2113,2321-2336
104	Haute-Sarine	18	2171,2176,2189-2194,2210,2214,2220,2222-2223,2225-2227,2229,2231-2232
111	Thal	9	2421-2429
151	Appenzell A.Rh.	22	3001-3007,3021-3025,3031-3038,3111,3212
161	Appenzell I.Rh.	5	3101-3105
171	Toggenburg	17	3351-3352,3354-3357,3371-3377,3391,3394,3403,3406
172	Sarganserland-Walensee	14	1608,1617-1618,1624,3291-3298,3311,3316
181	Surselva	48	3571-3584,3586-3587,3591-3596,3598-3606,3611-3616,3651-3652,3732 3734,3981-3987
182	Moesano	17	3801,3803-3806,3808,3810-3811,3821-3823,3831-3836
183	Regio Viamala	41	3503,3631-3642,3661-3670,3681,3691-3695,3701-3712
184	Prättigau	15	3861-3863,3871,3881-3883,3891-3893,3961-3962,3971-3973
185	Schanfigg	12	3914-3915,3921-3930
186	Mittelbünden	25	3501-3502,3504-3506,3511-3515,3521-3523,3531-3534,3536,3538-3541,3911-3913
187	Bregaglia	5	3771,3773-3776
188	Poschiavo	2	3551,3561
189	Unterengadin-Münstertal	18	3741-3746,3751-3753,3761-3763,3841-3846
211	Locarnese e Vallemaggia	63	5091-5099,5102,5104-5123,5125,5127-5136,5301-5322
212	Tre Valli	47	5006,5012,5015,5031-5047,5061-5081,5281-5286
213	Malcantone	26	5141,5143,5145-5146,5149,5151,5156,5159,5161,5171,5175,5178,5181,5183, 5188,5193,5200,5202,5204,5206-5207,5213,5216,5222,5230,5232
214	Valli di Lugano	27	5009,5011,5148,5150,5153,5155,5164-5165,5173-5174,5177,5187,5190-5191, 5199,5208,5212,5217-5218,5220,5223-5224,5226-5229,5235
221	Pays-d'Enhaut	3	5841-5843
222	Nord Vaudois	59	5551-5570,5741-5745,5747-5750,5752,5754-5766,5904-5905,5909,5916, 5918-5919,5922,5924,5926,5930-5931,5933,5936-5939
223	Vallée de Joux	3	5871-5873
231	Goms	21	6051-6052,6054-6067,6070-6071,6073,6177-6178
232	Brig-Oestlich Raron	16	6001-6002,6006-6011,6171-6176,6179-6180
233	Visp-Westlich Raron	32	6004,6191-6202,6281-6283,6285-6300
234	Leuk	15	6101-6117
235	Sierre	19	6231-6235,6237-6245,6247-6251
236	Sion	21	6021-6025,6081-6089,6246,6261,6263-6267
237	Martigny	22	6031-6036,6131-6137,6139-6142,6211-6212,6214,6218-6219
238	Chablais	28	5401-5413,5415,6151-6159,6213,6215-6217,6220
241	Centre-Jura	19	432,434-435,437,441,443,445-446,448,6421-6423,6431-6437
242	Val-de-Travers	11	6501-6511
243	Val-de-Ruz	19	6405,6413,6453,6456,6471-6484,6486
261	Jura	83	6701-6806

2.B Results

2.B.1 Main results tables

Number of firms

Table 2.B.1: Cumulative effect before/after Lex Bonny - Number of firms

	Number of Firms					
	(1)		(2)		(3)	
≤8	-0.010	(0.128)	0.043	(0.093)	0.050	(0.102)
-7	0.010	(0.059)	-0.002	(0.055)	0.005	(0.061)
-6	0.009	(0.050)	-0.005	(0.042)	0.002	(0.047)
-5	0.007	(0.042)	-0.003	(0.037)	0.003	(0.041)
-4	-0.022	(0.030)	-0.013	(0.030)	-0.008	(0.033)
-3	-0.017	(0.023)	-0.009	(0.020)	-0.005	(0.022)
-2	-0.000	(0.005)	0.003	(0.009)	0.004	(0.009)
-1	0.000	(.)	0.000	(.)	0.000	(.)
+0	-0.006	(0.097)	0.081***	(0.028)	0.102***	(0.030)
+1	0.002	(0.111)	0.098***	(0.034)	0.118***	(0.034)
+2	0.009	(0.114)	0.113***	(0.038)	0.133***	(0.034)
+3	0.029	(0.119)	0.141***	(0.048)	0.161***	(0.044)
+4	0.045	(0.120)	0.166***	(0.060)	0.186***	(0.056)
+5	0.074	(0.119)	0.205***	(0.075)	0.224***	(0.070)
+6	0.108	(0.122)	0.247***	(0.085)	0.267***	(0.080)
+7	0.115	(0.123)	0.264***	(0.098)	0.283***	(0.093)
+8	0.140	(0.129)	0.293***	(0.110)	0.312***	(0.105)
+9	0.181	(0.122)	0.344***	(0.130)	0.363***	(0.125)
≥10	0.242*	(0.131)	0.440***	(0.169)	0.457***	(0.165)
Long-term effect - Bonny	0.248*	(0.146)	0.497***	(0.151)	0.514***	(0.146)
# Observations	48926		48798		48417	
# Municipalities	1439		1436		1427	
Year FE	Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes	
Municipal time trends	No		Yes		Yes	
Pretreatment controls	No		No		Yes	

Notes: The table reports cumulative DiD estimates of Equation (2.1). Specification (1) includes year and municipality fixed effects. Specification (2) adds municipal-specific time trends. Specification (3) further adds pretreatment baseline characteristics. The sample includes all municipalities not covered by the LIM policy. Standard errors in parentheses are clustered at the district level. The long-term effect is computed following Davidson and MacKinnon (2004). *** p < 0.01, ** p < 0.05, and * p < 0.10.

Flow variables

Table 2.B.2: Cumulative effect before/after Lex Bonny - Flow variables

	Births				Deaths				Relocation In				Relocation Out			
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
≤8	0.007	(0.163)	0.016	(0.178)	0.376**	(0.190)	0.379*	(0.219)	0.181**	(0.090)	0.198**	(0.087)	0.228**	(0.108)	0.203*	(0.104)
-7	-0.098	(0.150)	-0.087	(0.161)	0.249**	(0.105)	0.254**	(0.114)	0.186	(0.128)	0.189	(0.128)	0.152	(0.143)	0.124	(0.134)
-6	-0.061	(0.109)	-0.051	(0.109)	0.230**	(0.098)	0.235**	(0.116)	0.192	(0.122)	0.194	(0.118)	0.345**	(0.139)	0.318**	(0.135)
-5	0.007	(0.126)	0.019	(0.131)	0.207**	(0.081)	0.213**	(0.087)	0.172	(0.113)	0.166	(0.111)	0.173	(0.137)	0.152	(0.133)
-4	-0.069	(0.082)	-0.060	(0.083)	0.152*	(0.087)	0.158*	(0.091)	0.194	(0.136)	0.191	(0.135)	0.162	(0.113)	0.147	(0.109)
-3	-0.086	(0.063)	-0.080	(0.063)	0.036	(0.068)	0.046	(0.076)	0.092	(0.102)	0.085	(0.101)	0.186*	(0.108)	0.175*	(0.104)
-2	-0.080	(0.055)	-0.080	(0.056)	0.158***	(0.042)	0.156***	(0.042)	0.100	(0.109)	0.102	(0.109)	0.081	(0.061)	0.078	(0.062)
-1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
+0	0.238**	(0.102)	0.280***	(0.104)	0.353***	(0.125)	0.472***	(0.122)	0.121	(0.148)	0.124	(0.164)	0.481**	(0.199)	0.476**	(0.193)
+1	0.206*	(0.124)	0.246**	(0.116)	0.119	(0.148)	0.236	(0.172)	-0.148	(0.346)	-0.149	(0.364)	0.249	(0.196)	0.245	(0.186)
+2	0.185	(0.148)	0.224	(0.136)	0.041	(0.143)	0.158	(0.137)	-0.005	(0.299)	-0.004	(0.309)	0.282	(0.235)	0.278	(0.241)
+3	0.245	(0.178)	0.285*	(0.162)	0.216	(0.198)	0.333*	(0.185)	0.217	(0.256)	0.221	(0.278)	0.409*	(0.234)	0.462**	(0.223)
+4	0.279	(0.218)	0.319	(0.198)	0.108	(0.188)	0.226	(0.165)	0.291	(0.263)	0.349	(0.273)	0.120	(0.264)	0.119	(0.260)
+5	0.369	(0.281)	0.408	(0.258)	0.103	(0.215)	0.220	(0.190)	0.466*	(0.250)	0.466*	(0.273)	0.522*	(0.282)	0.525*	(0.278)
+6	0.451*	(0.260)	0.491**	(0.239)	0.417*	(0.224)	0.535***	(0.180)	0.511*	(0.306)	0.511	(0.326)	0.298	(0.237)	0.311	(0.236)
+7	0.445	(0.342)	0.486	(0.315)	0.340**	(0.149)	0.457***	(0.124)	0.162	(0.403)	0.169	(0.417)	0.332	(0.226)	0.331	(0.228)
+8	0.476**	(0.228)	0.516**	(0.205)	0.160	(0.294)	0.278	(0.248)	0.415	(0.332)	0.407	(0.347)	0.482*	(0.289)	0.480*	(0.282)
+9	0.629	(0.407)	0.668*	(0.381)	0.440*	(0.254)	0.560***	(0.212)	0.376	(0.410)	0.368	(0.429)	0.624*	(0.338)	0.624*	(0.335)
≥10	0.687*	(0.358)	0.729**	(0.334)	0.420	(0.343)	0.542*	(0.298)	0.338	(0.454)	0.321	(0.477)	0.550	(0.338)	0.549	(0.336)
Long-term effect - Bonny	0.791**	(0.350)	0.827**	(0.324)	0.527	(0.335)	0.633**	(0.288)	0.268	(0.440)	0.272	(0.461)	0.463	(0.301)	0.468	(0.301)
# Observations	48031		47733		41978		41822		39837		39626		39384		39181	
# Municipalities	1422		1413		1257		1252		1190		1184		1183		1177	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Pretreatment controls	No		Yes		No		Yes		No		Yes		No		Yes	

Notes: The table reports cumulative DiD estimates of Equation (2.1), using firm births, deaths, and relocations as outcomes. For each outcome, specification (1) includes year and municipality fixed effects and municipal-specific time trends; specification (2) further adds pretreatment baseline characteristics. The sample includes all municipalities not covered by the LIM policy. Standard errors in parentheses are clustered at the district level. The long-term effect is computed following Davidson and MacKinnon (2004). *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

2.B.2 Exclusion of close-controls

Number of firms

Table 2.B.3: Cumulative effect of Lex Bonny, excl. of close-controls - Number of firms

	Excl. 0-5km (1)		Excl. 0-10km (2)		Excl. 0-15km (3)		Excl. 0-20km (4)	
≤8	0.065	(0.106)	0.017	(0.146)	0.018	(0.175)	0.022	(0.199)
-7	0.010	(0.064)	-0.033	(0.089)	-0.039	(0.112)	-0.040	(0.130)
-6	0.004	(0.050)	-0.028	(0.072)	-0.039	(0.093)	-0.046	(0.110)
-5	0.006	(0.044)	-0.030	(0.060)	-0.041	(0.078)	-0.050	(0.093)
-4	-0.006	(0.035)	-0.041	(0.046)	-0.054	(0.060)	-0.071	(0.072)
-3	-0.003	(0.023)	-0.029	(0.034)	-0.041	(0.046)	-0.058	(0.054)
-2	0.003	(0.009)	-0.006	(0.010)	-0.013	(0.012)	-0.020	(0.015)
-1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
+0	0.110***	(0.031)	0.110***	(0.040)	0.109**	(0.049)	0.105**	(0.053)
+1	0.124***	(0.035)	0.126***	(0.041)	0.126***	(0.048)	0.126**	(0.049)
+2	0.137***	(0.035)	0.140***	(0.038)	0.143***	(0.042)	0.146***	(0.043)
+3	0.163***	(0.044)	0.168***	(0.046)	0.172***	(0.050)	0.178***	(0.052)
+4	0.185***	(0.055)	0.192***	(0.056)	0.197***	(0.061)	0.205***	(0.067)
+5	0.221***	(0.069)	0.228***	(0.070)	0.233***	(0.075)	0.243***	(0.085)
+6	0.261***	(0.080)	0.271***	(0.081)	0.277***	(0.088)	0.290***	(0.100)
+7	0.276***	(0.092)	0.287***	(0.094)	0.293***	(0.102)	0.307***	(0.117)
+8	0.301***	(0.105)	0.318***	(0.108)	0.326***	(0.117)	0.343**	(0.136)
+9	0.350***	(0.126)	0.368***	(0.129)	0.375***	(0.140)	0.398**	(0.162)
≥10	0.440***	(0.166)	0.468***	(0.174)	0.479**	(0.191)	0.511**	(0.222)
Long-term effect - Bonny	0.511***	(0.146)	0.528***	(0.150)	0.558***	(0.159)	0.615***	(0.178)
# Observations	39447		30671		22715		17951	
# Municipalities	1162		903		669		528	
Year FE	Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes	
Pretreatment controls	Yes		Yes		Yes		Yes	

Notes: All regressions include the full set of fixed effects, the municipal-specific time-trends, and the pretreatment controls. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$. The treatment group is unchanged across specifications, while we successively exclude more municipalities from the control group. (1) excludes municipalities within 5km from the borders of the nearest municipality covered by the Lex Bonny. (2) further excludes those within 5-10km of the borders. We continue until we exclude all municipalities within 20km from the treatment borders (in (4)).

Creation of firms

Table 2.B.4: Cumulative effect of Lex Bonny, excl. of close-controls - Creation of firms

	Excl. 0-5km		Excl. 0-10km		Excl. 0-15km		Excl. 0-20km	
	(1)		(2)		(3)		(4)	
≤8	0.007	(0.179)	-0.203	(0.164)	-0.256	(0.198)	-0.333	(0.237)
-7	-0.105	(0.157)	-0.299**	(0.126)	-0.335**	(0.152)	-0.396**	(0.185)
-6	-0.077	(0.106)	-0.190*	(0.106)	-0.283***	(0.102)	-0.430***	(0.093)
-5	0.012	(0.130)	-0.144	(0.097)	-0.186*	(0.102)	-0.291***	(0.112)
-4	-0.065	(0.083)	-0.171**	(0.078)	-0.208**	(0.084)	-0.318***	(0.078)
-3	-0.093	(0.061)	-0.154**	(0.073)	-0.221***	(0.082)	-0.303***	(0.108)
-2	-0.093*	(0.056)	-0.132***	(0.040)	-0.180***	(0.033)	-0.197***	(0.037)
-1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
+0	0.290***	(0.100)	0.254***	(0.089)	0.247**	(0.103)	0.197*	(0.108)
+1	0.245**	(0.113)	0.196*	(0.107)	0.183	(0.125)	0.127	(0.122)
+2	0.224*	(0.135)	0.185	(0.120)	0.200	(0.124)	0.156	(0.133)
+3	0.278*	(0.159)	0.261*	(0.150)	0.258	(0.161)	0.219	(0.161)
+4	0.308	(0.196)	0.277	(0.181)	0.259	(0.186)	0.223	(0.193)
+5	0.396	(0.257)	0.362	(0.242)	0.348	(0.246)	0.315	(0.258)
+6	0.480**	(0.238)	0.466**	(0.224)	0.453**	(0.230)	0.439*	(0.242)
+7	0.457	(0.316)	0.444	(0.308)	0.439	(0.316)	0.438	(0.341)
+8	0.508**	(0.203)	0.529***	(0.194)	0.540***	(0.200)	0.511**	(0.210)
+9	0.650*	(0.382)	0.660*	(0.371)	0.642*	(0.380)	0.659	(0.410)
≥10	0.719**	(0.335)	0.764**	(0.328)	0.775**	(0.343)	0.796**	(0.372)
Long-term effect - Bonny	0.826**	(0.326)	0.856***	(0.326)	0.888***	(0.337)	0.965***	(0.364)
# Observations	39014		30449		22529		17813	
# Municipalities	1153		899		665		525	
Year FE	Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes	
Pretreatment controls	Yes		Yes		Yes		Yes	

Notes: All regressions include the full set of fixed effects, the municipal-specific time-trends, and the pretreatment controls. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$. The treatment group is unchanged across specifications, while we successively exclude more municipalities from the control group. (1) excludes municipalities within 5km from the borders of the nearest municipality covered by the Lex Bonny. (2) further excludes those within 5-10km of the borders. We continue until we exclude all municipalities within 20km from the treatment borders (in (4)).

2.B.3 Spatial DiD tables

Number of firms

Table 2.B.5: Cumulative effect before/after Lex Bonny - Number of firms vs 25-30km controls

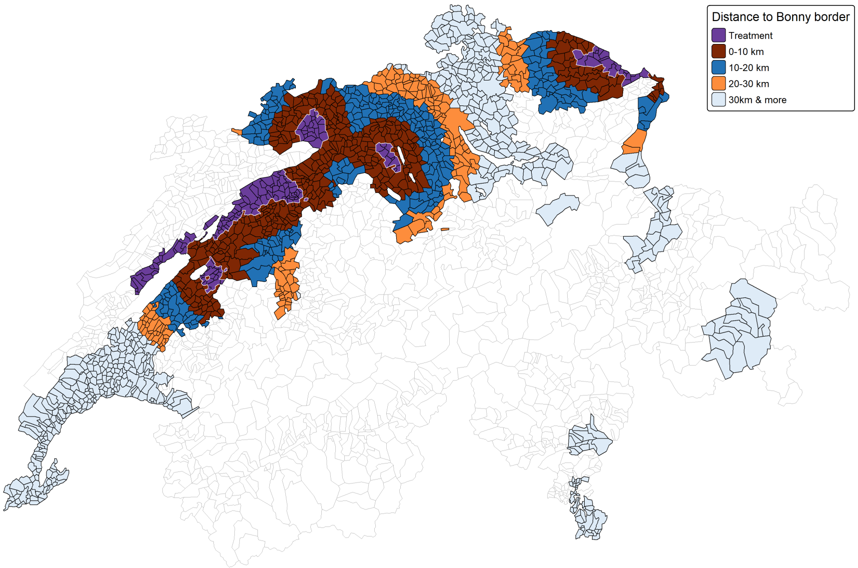
	Treated		0-5km		5-10km		10-15km		15-20km		20-25km		+30km	
	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
≤8	-0.115	(0.096)	0.054	(0.090)	0.036	(0.085)	-0.001	(0.100)	-0.059	(0.090)	0.155	(0.177)	0.085	(0.097)
-7	-0.164*	(0.098)	0.038	(0.122)	-0.022	(0.102)	-0.051	(0.122)	-0.111	(0.104)	0.104	(0.182)	0.022	(0.122)
-6	-0.145	(0.090)	0.035	(0.113)	-0.028	(0.095)	-0.046	(0.115)	-0.106	(0.097)	0.095	(0.166)	0.015	(0.116)
-5	-0.120	(0.082)	0.036	(0.104)	-0.030	(0.086)	-0.036	(0.106)	-0.097	(0.088)	0.082	(0.150)	0.013	(0.108)
-4	-0.117	(0.072)	0.031	(0.094)	-0.037	(0.076)	-0.030	(0.095)	-0.088	(0.077)	0.070	(0.133)	0.012	(0.098)
-3	-0.078	(0.049)	0.023	(0.062)	-0.023	(0.051)	-0.019	(0.063)	-0.058	(0.051)	0.048	(0.087)	0.008	(0.066)
-2	-0.030	(0.024)	0.019	(0.031)	-0.007	(0.025)	-0.007	(0.032)	-0.027	(0.026)	0.024	(0.043)	0.004	(0.033)
-1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
+0	0.034	(0.029)	-0.016	(0.031)	0.005	(0.025)	0.007	(0.031)	0.025	(0.026)	-0.028	(0.041)	-0.005	(0.033)
+1	0.081**	(0.039)	0.091**	(0.039)	0.123***	(0.041)	0.005	(0.039)	-0.009	(0.026)	-0.029	(0.041)	-0.011	(0.048)
+2	0.111***	(0.034)	0.079**	(0.039)	0.117***	(0.043)	0.022	(0.042)	0.020	(0.031)	-0.060*	(0.034)	-0.002	(0.041)
+3	0.151***	(0.041)	0.075	(0.052)	0.129***	(0.045)	0.038	(0.050)	0.047	(0.040)	-0.070	(0.043)	0.010	(0.043)
+4	0.190***	(0.058)	0.065	(0.076)	0.130**	(0.062)	0.048	(0.068)	0.073	(0.059)	-0.087	(0.066)	0.024	(0.059)
+5	0.236***	(0.081)	0.047	(0.107)	0.116	(0.083)	0.052	(0.090)	0.095	(0.082)	-0.104	(0.094)	0.032	(0.082)
+6	0.293***	(0.102)	0.041	(0.135)	0.121	(0.102)	0.064	(0.111)	0.126	(0.103)	-0.122	(0.119)	0.047	(0.105)
+7	0.318**	(0.126)	0.025	(0.150)	0.113	(0.120)	0.060	(0.136)	0.138	(0.125)	-0.147	(0.142)	0.034	(0.129)
+8	0.366**	(0.148)	0.017	(0.183)	0.130	(0.144)	0.081	(0.159)	0.166	(0.145)	-0.156	(0.170)	0.051	(0.152)
+9	0.442***	(0.169)	0.026	(0.205)	0.133	(0.162)	0.102	(0.176)	0.224	(0.161)	-0.168	(0.191)	0.090	(0.173)
≥10	0.607***	(0.219)	0.074	(0.270)	0.184	(0.207)	0.166	(0.229)	0.329	(0.204)	-0.184	(0.254)	0.155	(0.227)
Long-term	0.634***	(0.231)	0.090	(0.285)	0.257	(0.227)	0.204	(0.240)	0.374*	(0.210)	-0.105	(0.240)	0.230	(0.246)
# Observations	9508		12622		12524		13046		10064		9111		12347	
# Municipalities	293		398		391		401		310		280		385	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipal time trends	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Pretreatment controls	Yes		Yes		Yes		Yes		Yes		Yes		Yes	

Notes: All regressions include the full set of fixed effects, the municipal-specific time-trends, and the pretreatment controls. Standard errors (in parentheses) are clustered at the district level. *** p < 0.01, ** p < 0.05, and * p < 0.10. The control group is always municipalities located 25-30km from the borders of the nearest municipality covered by the Lex Bonny. Column (1) uses the standard treatment group. Columns (2) to (7) consider different bins as treated, starting from municipalities located within 5km from the treatment borders, up to those located 30km or more.

Robustness - Alternative bins

We vary bin sizes to investigate whether the arbitrary ring definition could drive results. Instead of 5km bins, we enlarge them to 10km. It reduces the number of bins, but increases the number of observations within them. Also, the reference bin is now 20-30km from the Bonny borders instead of 25-30km. Figure 2.B.4 shows how different bin sizes affect the spatial group composition. The dynamics and magnitudes of the effects are close to the estimation using 25-30km as the reference bin. Changing the composition of the control group does not affect the results for the treatment group, although the coefficients differ slightly during the pretreatment period.

Figure 2.B.4: Treatment group, 10km bins, and control groups



Notes: The map shows the locations of the treated, potentially exposed but untreated, and untreated municipalities. The treatment group consists of municipalities covered only by the Lex Bonny.

We also include controls in the 30-35 km bin in the control group to check if the results are driven by the decision to use 25-30km as the reference group. Adding controls farther from the treatment border does not affect the pattern of spatial treatment effects.

Figure 2.B.5: Cumulative effect before/after Lex Bonny - Number of firms against 20-30km bin

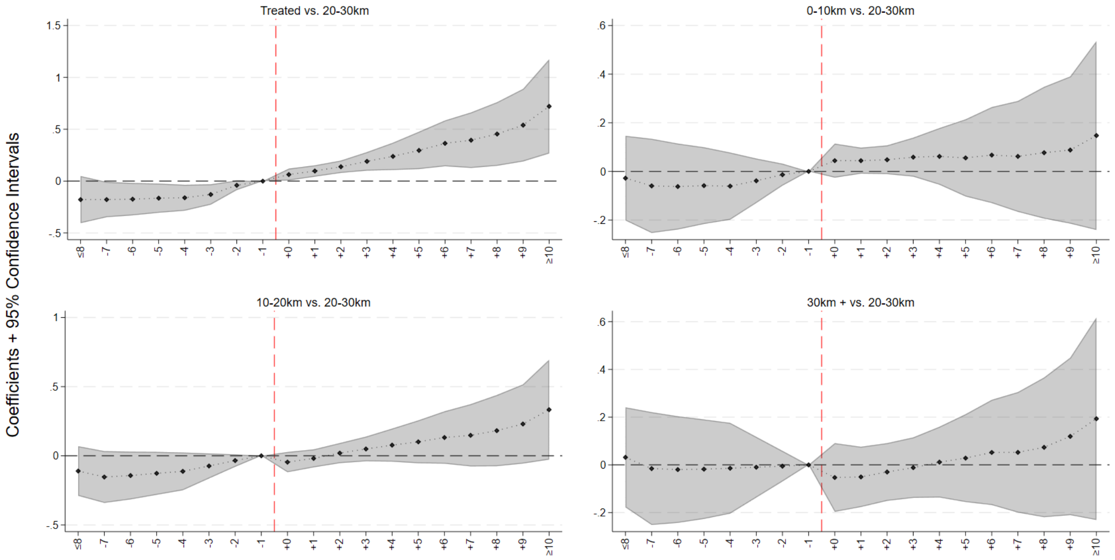
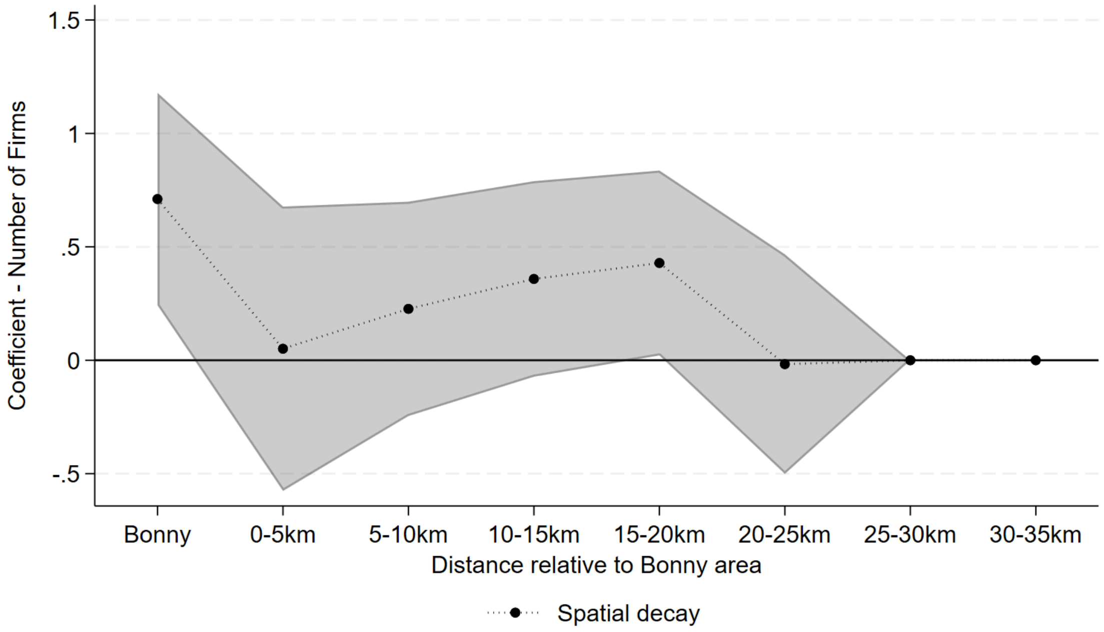
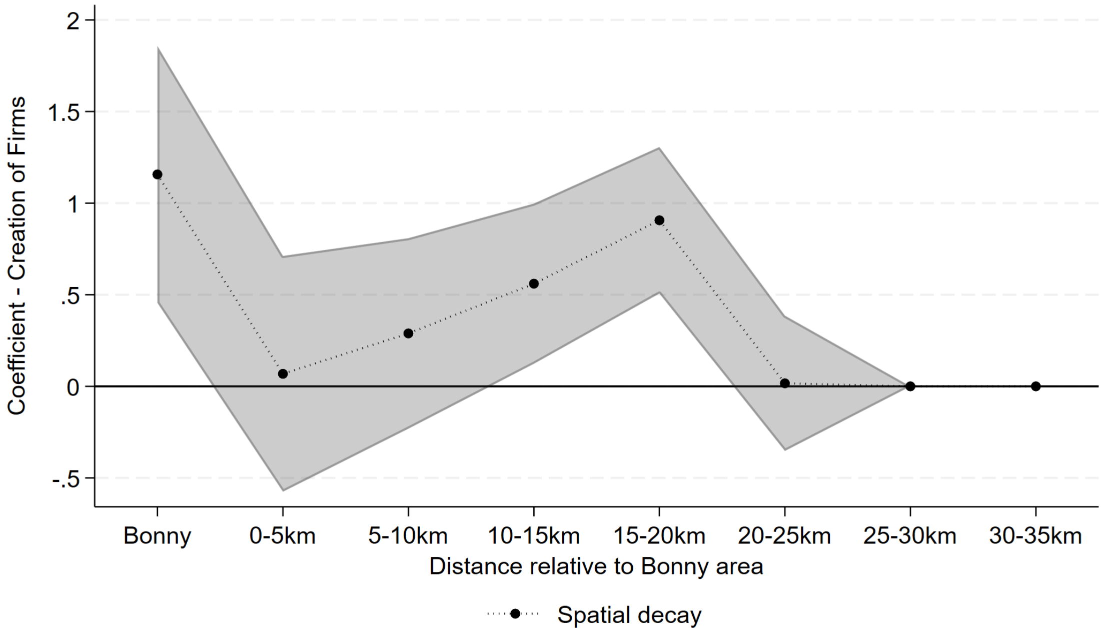


Figure 2.B.6: Spatial DiD with 25-35km as reference group - Number of firms



Notes: The figure presents our results estimating Equation (2.2) with the full specification and an alternative reference group. The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy.

Figure 2.B.7: Spatial DiD with 25-35km as reference group - Creation of firms



Notes: The figure presents our results estimating Equation (2.2) with the full specification and an alternative reference group. The full specification includes year and unit fixed effects, municipal-specific time trends, and baseline characteristics. Standard errors are clustered at the district level. The sample includes all municipalities not covered by the LIM policy.

2.B.4 Heterogeneity of firms - Number of firms & Board size dynamics

Table 2.B.6: Heterogeneity of firms - Dynamics of number of firms, by nominal capital

	Bottom 50th (1)		50-75th (2)		75-90th (3)		Top 10% (4)		90-99th (5)		Top 1% (6)	
≤8	0.164	(0.127)	0.034	(0.073)	0.099**	(0.049)	-0.010	(0.051)	0.016	(0.048)	-0.159	(0.201)
-7	0.099	(0.081)	0.020	(0.060)	0.085**	(0.041)	0.022	(0.038)	0.044	(0.035)	-0.091	(0.176)
-6	0.102	(0.063)	0.028	(0.049)	0.097***	(0.036)	0.003	(0.034)	0.016	(0.030)	-0.049	(0.168)
-5	0.104*	(0.056)	0.040	(0.057)	0.082**	(0.033)	0.054	(0.038)	0.063*	(0.035)	0.050	(0.162)
-4	0.084*	(0.047)	0.025	(0.062)	0.071**	(0.029)	0.026	(0.035)	0.038	(0.031)	-0.014	(0.136)
-3	0.104***	(0.035)	0.036	(0.052)	0.034	(0.022)	0.044**	(0.022)	0.050**	(0.020)	0.065	(0.113)
-2	0.091***	(0.028)	0.048	(0.049)	0.039**	(0.015)	0.032***	(0.012)	0.038***	(0.011)	0.013	(0.070)
-1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
+0	0.372***	(0.094)	0.076	(0.072)	0.208***	(0.063)	0.111*	(0.062)	0.116*	(0.065)	0.021	(0.244)
+1	0.386***	(0.097)	0.100	(0.076)	0.226***	(0.070)	0.128*	(0.074)	0.159**	(0.070)	-0.191	(0.243)
+2	0.417***	(0.103)	0.118	(0.075)	0.211***	(0.074)	0.164*	(0.086)	0.194**	(0.082)	-0.151	(0.194)
+3	0.474***	(0.113)	0.146*	(0.079)	0.188**	(0.079)	0.180**	(0.080)	0.213***	(0.073)	-0.168	(0.232)
+4	0.518***	(0.121)	0.162*	(0.087)	0.195**	(0.085)	0.204***	(0.076)	0.232***	(0.073)	-0.106	(0.221)
+5	0.562***	(0.131)	0.207**	(0.100)	0.196**	(0.095)	0.255***	(0.078)	0.279***	(0.074)	-0.042	(0.277)
+6	0.614***	(0.136)	0.233**	(0.108)	0.262**	(0.109)	0.290***	(0.086)	0.318***	(0.083)	-0.046	(0.253)
+7	0.639***	(0.145)	0.237**	(0.115)	0.269**	(0.124)	0.328***	(0.086)	0.368***	(0.080)	-0.116	(0.263)
+8	0.688***	(0.154)	0.250**	(0.121)	0.298**	(0.127)	0.345***	(0.091)	0.378***	(0.087)	-0.052	(0.275)
+9	0.757***	(0.172)	0.290**	(0.136)	0.338**	(0.145)	0.367***	(0.088)	0.379***	(0.090)	0.154	(0.339)
≥10	0.917***	(0.208)	0.360**	(0.162)	0.329**	(0.165)	0.449***	(0.091)	0.456***	(0.096)	0.220	(0.395)
Long-term effect - Bonny	0.885***	(0.205)	0.348**	(0.152)	0.272*	(0.160)	0.385***	(0.081)	0.388***	(0.087)	0.141	(0.366)
# Observations	33104		31081		25771		22520		22094		8285	
# Municipalities	1382		1299		1076		940		922		352	

Notes: All regressions include the full set of fixed effects, the municipal-specific time-trends, and the pretreatment controls. The sample includes all municipalities not covered by the LIM policy. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Table 2.B.7: Heterogeneity of firms - Dynamics of number of firms, by board size

	1 board member (1)		2 board members (2)		3 board members (3)		4 board members (4)		5 board members (5)		6+ board members (6)	
≤8	0.109	(0.119)	0.020	(0.064)	0.013	(0.069)	0.043	(0.105)	0.020	(0.131)	-0.033	(0.119)
-7	0.034	(0.072)	0.010	(0.046)	0.028	(0.062)	0.007	(0.087)	0.009	(0.110)	0.071	(0.097)
-6	0.025	(0.055)	0.017	(0.036)	0.030	(0.047)	0.016	(0.085)	0.021	(0.098)	0.121	(0.075)
-5	0.033	(0.045)	0.011	(0.038)	0.019	(0.043)	0.044	(0.073)	0.053	(0.100)	0.151*	(0.082)
-4	0.016	(0.038)	0.006	(0.033)	0.010	(0.041)	0.049	(0.060)	0.060	(0.100)	0.119	(0.080)
-3	0.007	(0.022)	0.022	(0.026)	0.022	(0.025)	0.049	(0.050)	0.078	(0.073)	0.032	(0.055)
-2	0.012	(0.009)	0.025	(0.016)	0.016	(0.012)	0.033	(0.034)	0.072	(0.050)	0.004	(0.029)
-1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
+0	0.291***	(0.084)	0.168**	(0.075)	0.116	(0.077)	0.023	(0.118)	0.232	(0.196)	0.070	(0.143)
+1	0.287***	(0.090)	0.219***	(0.077)	0.143*	(0.079)	0.060	(0.119)	0.175	(0.219)	0.075	(0.154)
+2	0.327***	(0.090)	0.237***	(0.077)	0.166**	(0.081)	-0.000	(0.114)	0.221	(0.230)	0.002	(0.147)
+3	0.389***	(0.102)	0.275***	(0.088)	0.172*	(0.089)	0.020	(0.112)	0.118	(0.220)	-0.004	(0.159)
+4	0.412***	(0.112)	0.305***	(0.093)	0.234**	(0.109)	0.017	(0.115)	0.056	(0.237)	0.012	(0.188)
+5	0.473***	(0.125)	0.351***	(0.108)	0.265**	(0.119)	0.005	(0.128)	0.133	(0.250)	-0.015	(0.201)
+6	0.535***	(0.134)	0.366***	(0.116)	0.298**	(0.140)	0.119	(0.142)	0.159	(0.256)	0.025	(0.207)
+7	0.612***	(0.141)	0.396***	(0.125)	0.271*	(0.157)	0.101	(0.149)	0.184	(0.271)	-0.313	(0.270)
+8	0.623***	(0.147)	0.453***	(0.135)	0.285*	(0.171)	0.096	(0.142)	0.360	(0.302)	-0.195	(0.254)
+9	0.676***	(0.169)	0.498***	(0.145)	0.358*	(0.186)	0.106	(0.155)	0.343	(0.334)	-0.070	(0.292)
≥10	0.833***	(0.207)	0.587***	(0.171)	0.406*	(0.211)	0.085	(0.164)	0.270	(0.349)	-0.119	(0.351)
Long-term effect - Bonny	0.891***	(0.214)	0.591***	(0.170)	0.386*	(0.208)	0.056	(0.166)	0.196	(0.309)	-0.401	(0.310)
# Observations	32382		31779		31314		25716		21411		16437	
# Municipalities	1352		1325		1306		1073		894		689	

Notes: All regressions include the full set of fixed effects, the municipal-specific time-trends, and the pretreatment controls. The sample includes all municipalities not covered by the LIM policy. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Chapter 3

Firms, Directors, and the Geography of Corporate Network: Evidence from Swiss Firms, 1960-2000[*]

3.1 Introduction

Administrative, linguistic, and religious borders within countries shape how organizations connect, exchange information, and coordinate resources. In corporate networks, these borders can shape executive labor markets, constrain board recruitment, and impact the diffusion of practices and innovation. Switzerland offers an exceptional setting to study these forces. Its long-standing federalism and language regions coincide with a twentieth-century trajectory of market unification and large-scale investments in transport infrastructure.

These overlapping dynamics raise three questions. First, how has the geography of the Swiss corporate networks changed between 1960 and 2000? Second, how do distance and internal borders shape the Swiss corporate network as the national market integrates over time? Third, does a reduction in travel time between municipalities generate stronger network connections between municipalities?

To do so, we collect data on all Swiss corporations from the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*" for the years 1934, 1943, 1960, 1962-1966, 1969, 1972, 1975, and 1979-2003. This allows us to build three separate networks. First, the bipartite network in which firms and directors connect when the latter sits on the board of the former. We then project the bipartite network into two unimodal networks. In the firm-to-firm network, two firms are linked if they share at least one board member. In the director-to-director network, two directors are linked if they sit on the same board. We aggregate the different networks at the municipal level and study the evolution of inter-municipal connections over 1960-2000. Aggregation at the municipal level allows us to characterize the spatial distribution

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[*] This chapter is a collaborative work with Mark Schelker. We thank Stefani Stefanova for her comments and help on the gravity equations. We thank Emilie Dousse and Julie Uldry for their fantastic and careful assistance in the data cleaning process.

of links and track how border frictions evolve over time.

We connect two strands of literature. The first studies interlocking directorates and elite networks, but usually pays little attention to geography. The second studies border effects and economic geography, but usually focuses on trade, migration, or production rather than corporate networks. By embedding corporate networks in space and over time, we provide new evidence on within-country integration.

This chapter makes three main contributions. First, it assembles new historical data linking the universe of Swiss corporations to their directors over 1960–2000. Second, it compares the geography of connectivity across three related networks (the bipartite network and its firm–firm and director–director projections) within a common empirical framework. Third, it quantifies within-country border effects in an organizational network, focusing on cantonal, linguistic, and religious boundaries. Finally, it documents how these spatial frictions evolve over four decades, providing evidence on whether integration and declining travel costs affect internal discontinuities or leave them intact.

The results show that geography strongly shapes Swiss corporate networks. Distance reduces inter-municipal connections, and cantonal and language borders impose large penalties on link formation across all three network layers. These effects remain economically important over time despite market integration and transport improvements. Religious boundaries matter less and are declining in importance. Overall, internal administrative and linguistic borders continue to structure corporate links even in a small and highly integrated federal state.

The remainder of the chapter is organized as follows. Section 3.2 presents the conceptual framework and related literature. Section 3.3 provides the historical and institutional context. Sections 3.4 and 3.5 describe the data, the construction of the networks, and the descriptive evidence. Section 3.6 presents the empirical strategy. Section 3.7 reports the regression results. Section 3.8 concludes.

3.2 Theoretical framework and related literature

3.2.1 Firm networks and interlocking directorates

The management, finance, and sociology literatures have extensively studied the formation of interlocking directorates (Dooley 1969, Zajac 1988, Mizruchi 1996, Brennecke and Rank 2017). Those studies do not formally analyze the strategic incentives to interlock and their economic relevance. Battaggion and Cerasi (2020) provide an important exception. They develop an analytical model of strategic interlock formation and highlight a central trade-off. On the one hand, interlocks can improve corporate outcomes and affect market competition. Firms use them to access private information, monitor business relationships, adopt better managerial practices, and gain competitive advantages (Loderer and Peyer 2002, Ozmel et al. 2013, Lamb and Roundy 2016, Mazzola et al. 2016). On the other hand, interlocks can weaken board independence, facilitate the extraction of private benefits at the expense of minority shareholders, and soften competition. When firms share directors, they may coordinate their conduct more easily. Several studies describe the evolution of interlocks in a descriptive way (Carbonai and Di Bartolomeo 2006, Santella et al. 2009, Helmers et al. 2017). By contrast, Barone et al. (2022) identify causal effects. They exploit a reform that bans common directorships between Italian banks active in the same local market and show that removing these ties reduces interest rates on loans by 10 to 30 basis points.

Switzerland has no law that prohibits such connections, and interlocking directorates are common. Swiss firms can therefore strategically form links. They often target firms in the same industries, which increases the density of within-industry interlocks, facilitates information flows, and makes collusion easier (Gualdani 2021). Along the vertical dimension, it can lead to preferential treatment, foreclosing of rivals, exclusive dealing, and vertical integration (Croci and Grassi 2014).

Another explanation for the formation of interlocks is the search-and-matching process. This mechanism requires directors to hold multiple board seats, which is possible in Switzerland. Firms and directors seek productive matches, and each new match generates a positive matching externality (Helsley and Strange 1990). Koskinen and Edling (2012) propose peer referral as a specific channel. When seeking a new director appointment, an incumbent board member may recommend someone they know from another board.

3.2.2 Geography, border effects, and their role in networks

Despite early claims about the *death of distance* (Cairncross 1997), geography still constrains economic interactions. Geographical distances also play an important role in the network literature. When the cost of forming links rises with distance, agents prefer nearby connections (Johnson and Gilles 2003, Jackson and Rogers 2005, Galeotti et al. 2006). Firms cluster geographically in production networks (Bernard et al. 2019), and director networks also display strong spatial clustering (Heemskerk 2013, Heemskerk et al. 2013).

Although knowledge spillovers are spatially limited and rapidly fade out (Kerr and Kominers 2015, Kerr and Robert-Nicoud 2020), links do exist over longer distances. Firms located further away might have relevant information that is not accessible without establishing another type of connection, one that is not based on geographical closeness. Hence, networking can also act as a substitute for geographical proximity in knowledge sharing. One such substitute is sharing board members, which facilitates information sharing (Mizruchi 1996, Howard et al. 2017, Gualdani 2021), even across geographically distant firms (Heemskerk 2013, Heemskerk et al. 2013).

Distance is not the only obstacle to interactions. Borders delimit clear changes in various dimensions, from regulation to language or religion, and increase the costs of interactions between firms or people (McCallum 1995, Chen 2004). They divide an otherwise continuous space and increase transaction costs. National borders limit economic interactions and potentially slow economic growth (McCallum 1995). Similar costs exist at the regional level, where the borders also affect local economic growth (Andresen 2010, Bacchiega et al. 2016). Administrative barriers and regulatory differences are not the only factors at play. Schulze and Wolf (2009) investigate the effect of ethnic and language differences in the Habsburg Empire. As transport costs decline due to the construction of railways, the relative importance of other non-distance barriers increases. The growing importance of linguistic and ethnic proximity in trade decisions has led to a decline in interactions across groups.

Geographical distance and national borders are substantial barriers to collaboration (Hoekman et al. 2009, Scherngell and Barber 2009, Singh and Marx 2013). Empirical evidence even suggests that their disruptive effects have not decreased over time (Schulze and Wolf 2009, Hoekman et al. 2010, Morescalchi et al. 2015). One way to avoid those trade and border costs is to colocate, which can lead to the agglomeration of economic activities. This is especially true for researchers and innovative activities[2].

Consistent with our empirical approach, Azar (2021) documents that a gravity equation can also help explain the pattern of interlocking directors, with the probability of interlocks decreasing with the distance between US companies. Azar (2021) finds that having similar shareholders increases the probability of having common directors. He also shows that more distant firms are less likely to share directors. If distance shapes interlock formation, discontinuous increases in connection costs from borders should do so as well.

Cultural and linguistic barriers should also be considered. Several studies show

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[2] See Kerr and Kominers (2015) and Kerr and Robert-Nicoud (2020) for knowledge spillovers, Katz and Shapiro (1994), Katz and Martin (1997), Storper and Venables (2004), and Catalini (2018) for research collaborations, and Morescalchi et al. (2015) for co-patenting

that immigrants tend to favor trade with their country of origin, mainly because they can leverage existing social ties and cultural alignment (Gould 1994, Head and Ries 1998, Dunlevy and Hutchinson 1999, Wagner et al. 2002, Rauch and Trindade 2002). Linguistic differences generate additional translation costs. Melitz (2008) investigates this question and proposes a measure of translation costs based on the percentage of speakers who can communicate directly across countries. Translation costs do play a role, but the ability to communicate directly is much more important in trade relationships. In a more recent paper, Melitz and Toubal (2014) suggest that speaking the same language is crucial, and face-to-face interaction matters substantially in international trade. Finally, Falck et al. (2012) show that dialect similarity in the 19th century affects current regional migration flows in Germany. This suggests potential persistent effects of former borders.

In the Swiss context, Egger and Lassmann (2015) exploit the discontinuity along the Swiss language border to show that municipalities increase trade with neighboring countries speaking the same language by 19.6% after the conclusion of free trade agreements. Cultural differences also shape preferences and attitudes, notably toward work, social insurance, redistribution, and taxation (Eugster et al. 2011, 2017, Eugster and Parchet 2019). Language borders can shape patterns of economic exchange independently of political divisions. In particular, linguistic borders affect labor mobility. Workers are more likely to seek employment within their linguistic region, and firms recruiting across language borders face higher matching and training costs.

For the reasons outlined in Section 3.2.1, firms and directors decide to connect (i.e., a director accepts being a board member) if they both expect utility gains from the interactions. Distance increases connection costs continuously, and borders introduce discontinuous increases in the cost of forming links across them. To our knowledge, no study documents the effects of cantonal, linguistic, and religious borders on the corporate network. Using our network and spatial information, we can describe the structure and evolution of these connections. Using detailed spatial information on the universe of Swiss firms and directors over multiple decades, we can observe the evolution of distance and border effects over time.

3.3 History and institutional context

Switzerland's history led to the creation of different internal borders over time. Firstly, cantonal borders can abruptly change the institutional environment. Secondly, with its four national languages, the country has several language borders. They usually can match the cantonal borders, but sometimes they split cantons in half. This is also true for religion. Those three dimensions create a complex mix of borders in which the three can coexist simultaneously, thereby increasing institutional distance between municipalities. However, the context has significantly changed over the past century. While language regions remained remarkably stable over time, religion lost its significance, and reforms were implemented to establish a domestic market in Switzerland.

3.3.1 Federalism and cantonal borders as market boundaries

The Federal Constitution of 1848 grants each of the 26 cantons its own constitution, parliament, government, and courts, resulting in sharp differences in taxation, licensing, and market-access regulations across cantonal borders (Fankhauser 2009, 2011, Stüssi-Lauterburg 2012). Switzerland's decentralized organization differs from that of its neighbors in several respects. First, the lower levels of government, the cantons and municipalities, enjoy a particularly high degree of autonomy. Second, the control of political representatives through direct democracy allows the people to sanction abuses. Finally, it is the cantons that decide on the powers assigned to the federal level. Thus, all decisions on centralization must be justified and accepted in a democratic process.

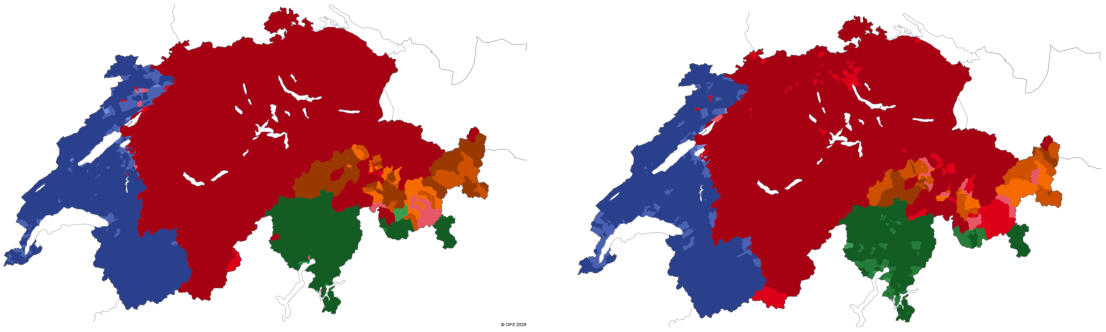
These institutional differences long functioned as genuine economic boundaries. In parallel with the debate over EEA accession, Switzerland took action against its weak anti-competition authorities and pervasive cartels. For decades, Swiss competition policy had been unusually permissive by international standards: cartels were legal unless deemed "*socially harmful*". The EEA debate brought Switzerland face-to-face with European competition norms, under which cartels were clearly prohibited, and competition policy was integral to the single market architecture. The result was a major overhaul of the Cartel Act in 1995, which fundamentally shifted Swiss competition policy from a permissive to a prohibitive framework. The reform was not simply technical. It was rather a broader effort to modernize the Swiss economy, reduce internal barriers, and align domestic market institutions with those of Europe. By reducing internal barriers and harmonizing market institutions across cantons, the reform lowered the costs of cross-cantonal economic interactions, which we expect will lead to a decline in cantonal border effects in our corporate network.

From an empirical perspective, the effect of cantonal borders as barriers to economic interaction should decline over time. In earlier decades, when regulatory autonomy was stronger and market access differed more sharply across cantons, we expect borders to impose larger frictions in cross-cantonal connections. As federal harmonization progressed and competition policy strengthened, these border frictions should attenuate. The historical narrative predicts a downward trend in estimated border effects, reflecting the gradual construction of a more unified Swiss internal market.

3.3.2 Language regions as social and market boundaries

Language differences raise transaction costs by complicating communication, reducing trust, and limiting information flows, both between firms and between directors and the firms they serve. Unlike cantonal borders, which reflect political sovereignty, language borders reflect cultural communities that do not align exactly with cantonal boundaries. Many cantons are monolingual, while others are officially bilingual (Bern, Fribourg, Valais) or trilingual (Grisons), meaning linguistic cleavages cut across institutional divisions.

Figure 3.1: Share of population speaking the language of the municipalities' majority



(a) Panel A: Shares in 1941 (b) Panel B: Shares in 2000

Notes: The darker the shade, the larger the share of the population that speaks the municipality's majority language as its main language. Colors indicate the majority language: dark red = German, blue = French, green = Italian, orange = Romansh. Sources: Swiss Federal Statistical Office (2025), based on Census Data (Swiss Federal Statistical Office 1941, 2000b).

Switzerland's language communities occupy geographically distinct areas whose internal borders have remained remarkably stable over centuries. Figure 3.1 shows census data on the language of all municipalities, from just before our first available year (1941) to the end of our data (2000), to illustrate its stability over time. These language borders can actually be traced back to the previous centuries and have proved to be very stable over time[3]. Preferences, however, change across the language border within cantons (Eugster et al. 2011, 2017, Eugster and Parchet 2019). This is an additional reason to think that economic and social interactions would be more

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[3] Figure 3.A.1 shows the share of the population speaking the municipality's majority language in 1860 and, for comparison, a map for 2000.

costly once they cross the language borders. From an economic geography perspective, linguistic segmentation also shapes labor mobility and market integration: workers tend to seek employment within their linguistic region, and firms recruiting across language borders face higher matching and communication costs, as shown for trade by Egger and Lassmann (2015), Melitz (2008), and Melitz and Toubal (2014).

Given this institutional and empirical background, one would expect language borders to exert a persistent downward effect on economic interactions, even within an otherwise highly integrated federal state. In earlier decades, when transport links were weaker and linguistic communities more isolated, these effects were likely stronger. Over time, as mobility improved and market integration improved, linguistic frictions may have diminished, but certainly not disappeared. For the purposes of our empirical analysis, this narrative suggests that linguistic borders should act as measurable frictions in municipality-to-municipality connections. While these frictions may vary over time, they are unlikely to disappear entirely given their remarkable stability.

3.3.3 Religions as social boundaries

Religion was a primary source of conflict in Swiss history. The tensions culminated in the Sonderbund War of 1847, when an alliance of Catholic cantons was ruled unconstitutional and dissolved by federal force (Sallmann 2011). Though brief, the war had profound consequences. The Federal Constitution of 1848 laid the foundation for modern Switzerland while preserving cantonal autonomy over church-state relations, resulting in varying degrees of separation across cantons (Roca 2012).

Figure 3.2 shows that the spatial distribution of religious majorities remained largely stable over our sample period, with almost no municipalities switching from one majority to the other. As highlighted in the right panel of Figure 3.2, some of the largest cities and their surroundings are populated by a majority of atheists since 2000[4]. While religion has slowly lost its social salience, the history of confessional conflict and associated cultural differences may still leave persistent traces in corporate networks. We include religious borders in our analysis to test whether past divisions generate persistent frictions in network formation even as their contemporary relevance fades.

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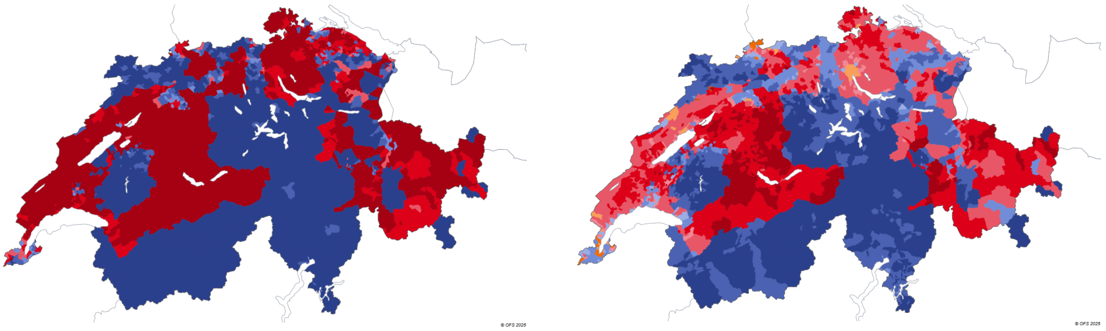
[4] Zürich, Geneva, and Basel, for example. In 2024, their total population accounts for around 9% of the total population of Switzerland.

Figure 3.2: Share of population affiliated with the religious majority



(a) Panel A: Shares in 1941 (b) Panel B: Shares in 2000

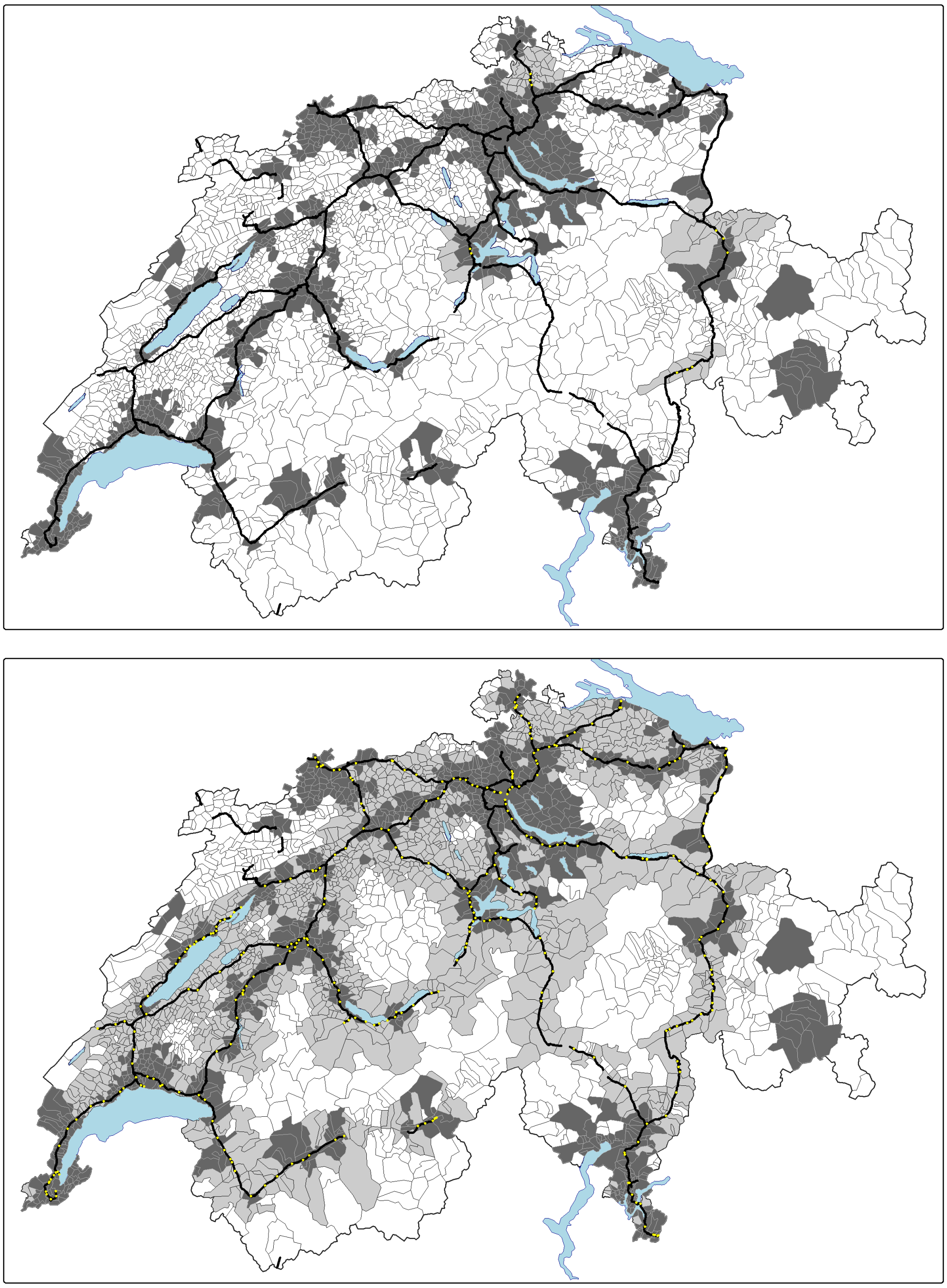
Notes: Darker shades indicate a larger share of the population affiliated with the local religious majority. In Panel A, red denotes Protestant-majority municipalities and blue denotes Catholic-majority municipalities. In Panel B, orange denotes municipalities in which atheists form the majority. In all cases, the lightest shade corresponds to a majority share below 40%, the intermediate shades to shares of 40-59.9% and 60-79.9%, and the darkest shade to a majority share above 80%. Sources: Swiss Federal Statistical Office (2025), based on Census Data (Swiss Federal Statistical Office 1941, 2000b).

3.3.4 Road development and infrastructure

Switzerland had no planned highways before the mid-1950s. Construction of a national highway network began in 1960 and progressively linked the largest cities and key regional centers, with most access points opening in the 1960s to 1980s. The network was largely completed by about 2010, with considerable delays and rerouting. This rollout reduced the effective distance between regions and deepened integration of the internal market. Firms near interchanges experience an immediate expansion of market reach (inputs, customers, and labor) and a reduction of transportation costs. In our context, the development of the road network reduces the distance between firms and directors, and favors the regional economic integration of local markets. In the meantime, the railroad policy over the same period focused on schedule optimization rather than the construction of new lines. Thus, the railroad network was remarkably stable over the period covered in our analysis.

The upper part of Figure 3.3 shows the access points already open in 1960 over the layout of the future complete highway network. The lower part shows all access points open by the end of our sample.

Figure 3.3: Location of all access opened until 1960 and 2003



Notes: Yellow dots show the highway access. Dark grey areas show cities & municipalities within agglomerations. Light grey areas are the rural municipalities with access within 15 km of their center. Based on Fretz et al. (2021).

3.4 Data

3.4.1 Firm and director data

The construction of the interlocking directorate networks and their municipal-level aggregations requires several steps. Firstly, we extract our data from the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*" for the years 1934, 1943, 1960, 1962-1966, 1969, 1972, 1975, and 1979-2003. We scan and digitize the volumes and then clean the resulting datasets year by year[5]. In our analysis, we will focus on a selection of years with (almost) stable 3-year intervals[6]. By focusing on 3-year cross-sections starting in 1960, we can more easily interpret effects over time.

We gather two linked datasets. The first is the annual universe of corporations (*"SA/AG"* in Switzerland). The second contains the annual universe of directors sitting on the boards of those companies. Our firm-level data include the firm name, municipal-level headquarters location, and nominal capital. For directors, we know their full names and the municipality in which they reside. Because we know which directors sit on which boards, we can construct the bipartite network that links firms and directors. We then aggregate all three networks to the municipal-level using harmonized municipal boundaries as of 2018 to account for municipal mergers over time.

3.4.2 Networks construction

We first construct the bipartite network of firms and directors for all corporations in Switzerland from 1960 to 2000. We then project the bipartite network onto the firm-to-firm network, the director-to-director network, and their municipal-level aggregations. Each network provides different information about the geography and structure of the corporate network. More formally, we construct a bipartite network $\mathcal{N}(\mathcal{V}, \mathcal{G}_{bi})$ in which $\mathcal{V} = \{n_1, \ldots, n_N\}$ is the set of nodes, where $N = |\mathcal{V}|$ is the total number of nodes, and $\mathcal{G}_{bi} = [g_{ij}]$ is the adjacency matrix characterizing all connections between firms and directors. A node can either be a firm or a director. The nodes are split into two disjoint subsets $\mathcal{V} = \mathcal{V}_F \cup \mathcal{V}_D$ and $\mathcal{V}_F \cap \mathcal{V}_D = \emptyset$. In the bipartite network, links exist only across the two subsets. A link between a director and a firm exists when this director serves on the firm's board.

We then project the bipartite network onto two different unimodal networks. In

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[5] We describe the complete data-cleaning and preparation procedure in Appendix 1.A.

[6] There is only one 4-year interval in our data, from 1975 to 1979.

our case, we use a weighted projection to account for cases in which firms share multiple directors (or directors sit on multiple boards together). Figure 3.4 illustrates a fictional bipartite network and the two resulting projections. In Panel A, Firms F_2 and F_3 share two directors, D_3 and D_4. When we project their connections onto the firm-to-firm network, we observe that they are connected, and the weight of the link between them is 2. Therefore, a link between two firms in this one-mode network means that they share at least one common director. Similarly, as D_3 and D_4 sit on two boards together (in F_2 and F_3), they are also connected in the director-to-director network. The weight in Panel C indicates the number of boards on which the connected directors sit together.

Because we geolocate firms and directors at the municipal level, we can attach municipal characteristics to both nodes and edges. We generate several of these variables after aggregating the networks at the municipal level. In Figure 3.5, the original bipartite network has firms and directors located in two distinct municipalities. This allows us to generate edge characteristics, indicating if the two connected municipalities are in the same canton. Similarly, we generate two dummy variables indicating whether the municipalities share the same majority language and religion. These edge characteristics will serve as boundary variables in our gravity analysis.

In the remainder of the chapter, we use the municipality-level aggregation of the undirected networks for two reasons. First, the networks are large, and we encounter computational issues rapidly when attempting to construct the adjacency matrix. In an n-node network, the number of possible connections is $n(n-1)/2$. The adjacency matrix explodes in size as n increases. In 1943, there are 14,193 firms and 100,713,528 possible connections. In 1962, there are 526,549,926 possible connections among the 32’452 firms. The number of firms continues to grow over time, making it impossible to estimate it without relying on further strategies[7].

Second, municipal aggregations retain all the information we need for the analysis[8]. Our empirical approach relies on municipal characteristics. The municipal-level network is an aggregation of microeconomic decisions by firms and directors. Municipalities are therefore not agents in a literal sense, but the same issue arises in the trade literature, where countries appear as trading units when firms actually trade (Bernard et al. 2007). For our purposes, the municipal-level network contains the

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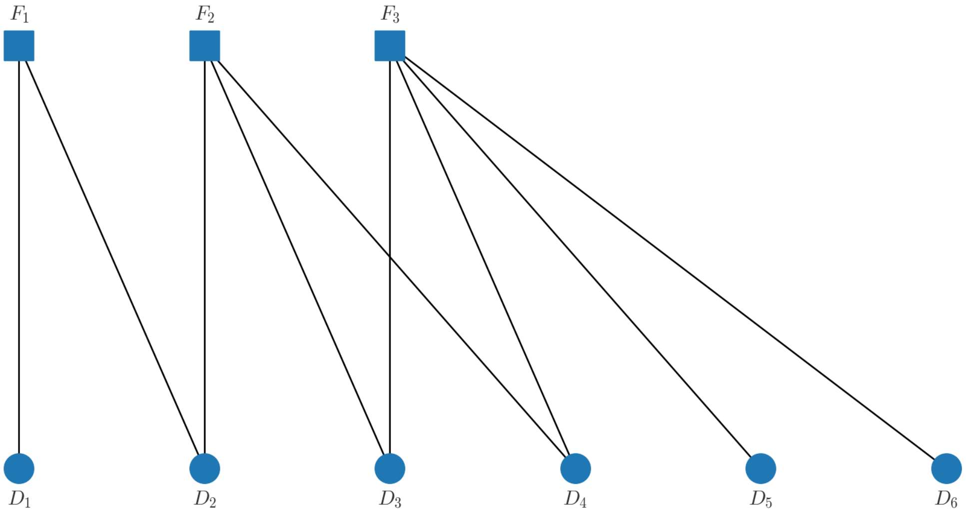
[7] For example, Büchel and von Ehrlich (2020) use stratified sampling to deal with the size of the adjacency matrix.

[8] A drawback of municipal aggregation is that it removes information about the exact connections between directors and firms. Figure 3.B.3 in the Appendix illustrates this issue. While we still know how many firms, directors, and connections exist between and within locations, we do not know the exact distribution of connections between them. However, since we are interested in distances and how borders affect connections, losing this distribution is not an issue.

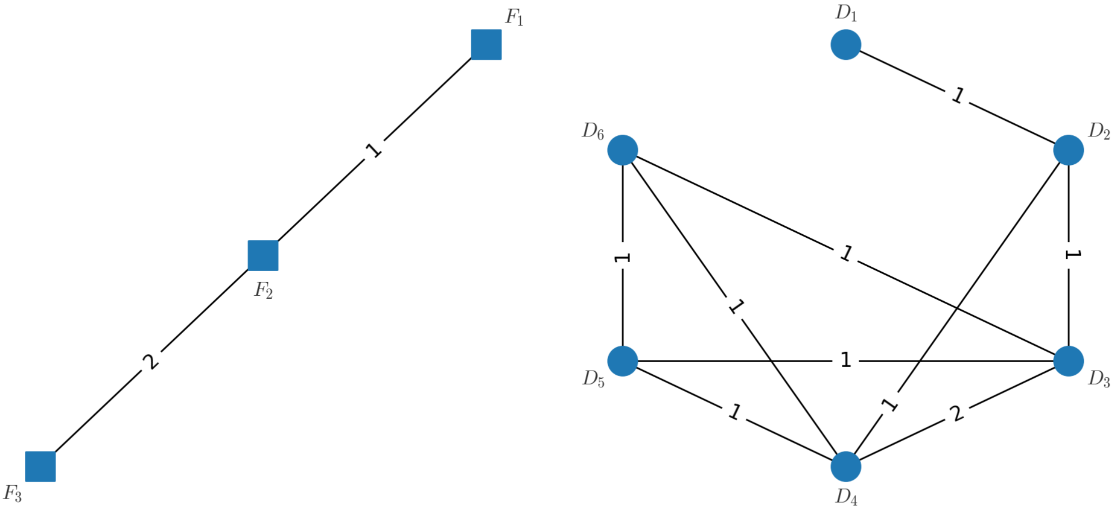
Figure 3.4: Illustration of the network projections

Panel A - Bipartite network



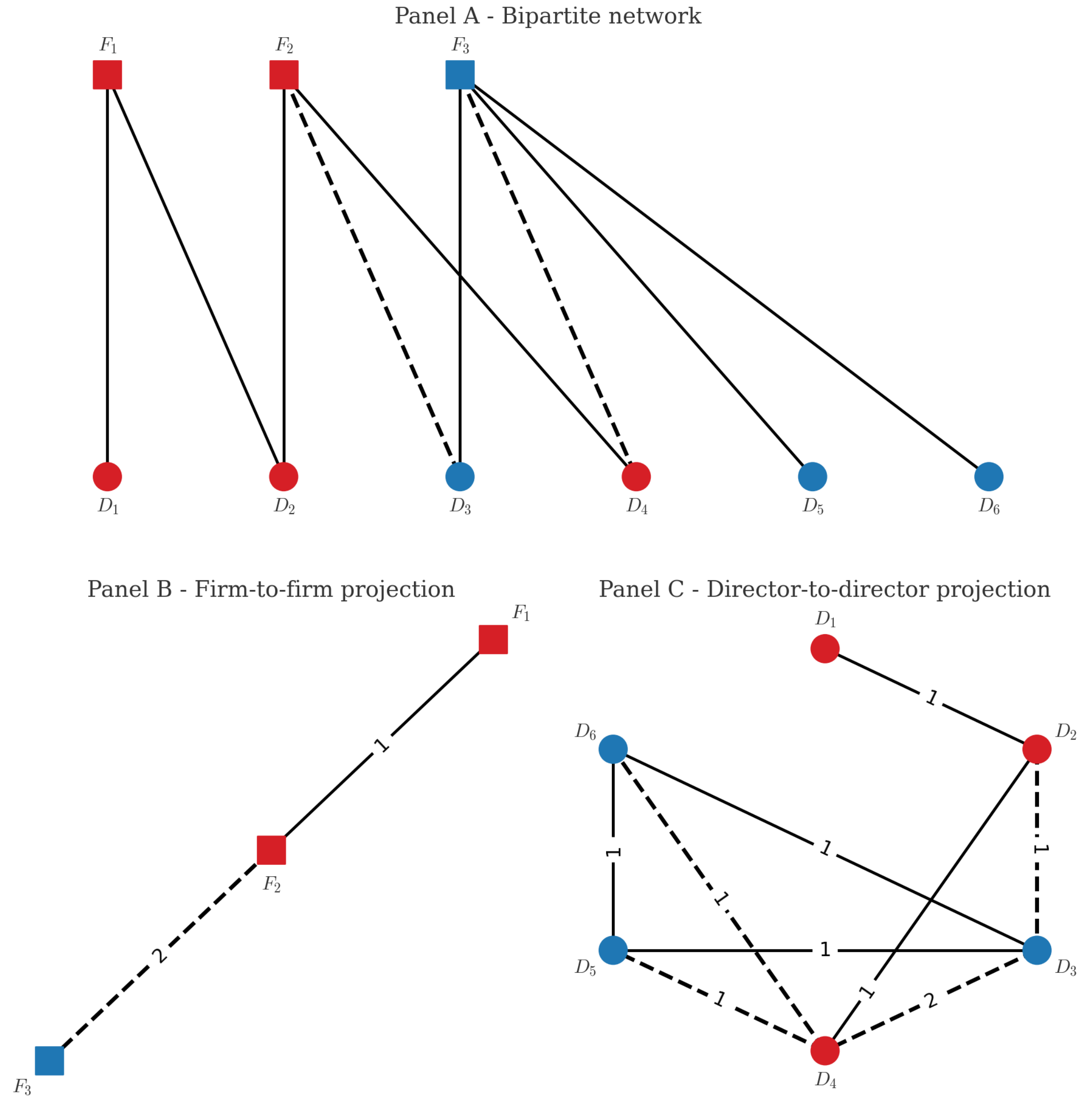
Panel B - Firm-to-firm projection

Panel C - Director-to-director projection



Notes: Panel A illustrates a bipartite network where six directors (denoted D_i and represented by circles) are members of the boards of three firms (denoted F_i and represented by squares). Panel B displays the resulting firm-to-firm network obtained by applying the same projection procedure used for our real networks. Similarly, Panel C shows the director-to-director projection of the original bipartite network in Panel A. The weight represents the number of connections existing between two agents. For example, F_2 and F_3 have two directors in common; hence, the weight of two is attributed to their connection, as shown in Panel B. Similarly, D_3 and D_4 are members of the same two boards, giving their connection a weight of two.

Figure 3.5: Illustration of the network projections, with nodes and edges attributes



Panel A - Bipartite network

Panel B - Firm-to-firm projection

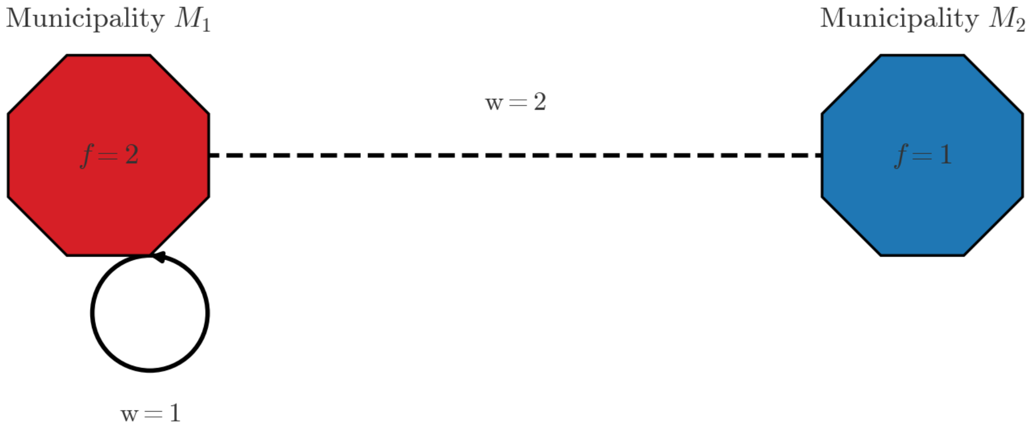
Panel C - Director-to-director projection

Notes: Similar to Figure 3.4, except that red and blue nodes are in two distinct municipalities in two different cantons (or language regions). The dashed lines represent connections between nodes in different cantons or language regions.

relevant information to study how geography and internal borders shape the intensity of interactions between firms and directors.

Figure 3.6 illustrates the aggregation process from the firm-to-firm projection (Figure 3.5 Panel B) to the municipal-level. As shown, we can attach the number of firms as a node characteristic. The number of inter-municipal connections is the sum of all firm-to-firm connections between the two municipalities. The dashed line represents them, and we attach an edge characteristic to the link to weight it. F_1 and F_2 are connected firms located in the same municipality. This connection appears in the self-loop below M_1. Since F_3 had no connections, there are no self-loops attached to M_2. The aggregation preserves all links, including those within municipalities[9].

Figure 3.6: Illustration of the municipal aggregation of the firm-to-firm projection



Notes: Red and blue nodes are two distinct municipalities in two different cantons (or language regions). The dashed line represents connections between nodes in different cantons or language regions.

More formally, in the aggregated network, a node M represents a municipality. Each municipality is denoted by $i \in \mathcal{M} = \{1, ..., M\}$. Each M has f firms located within its boundaries. Our network is $(\mathcal{M}, \mathcal{G}_{mun})$, with $\mathcal{G}_{mun} = [g_{ij}]$ is an adjacency matrix of size $M \times M$ representing all connections between municipalities. We have the network's adjacency matrix available for 35 different periods $t \in \{1, ..., 35\}$. This allows us to observe the evolution of the network over 40 years, albeit with some gaps.

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[9] Within-municipality ties serve as the "domestic trade" in gravity-style estimation. They serve as the baseline level of interaction in the absence of spatial frictions and allow distance and border effects to be interpreted relative to the baseline.

3.4.3 Municipal-level and pair-level data

We add data from several other sources. First, we use data from the Federal Population Census (Swiss Federal Statistical Office 2000a). Our municipal-level variables include the share of residents speaking one of the three main national languages. This allows the creation of a variable indicating the language spoken by the majority of residents. Similarly, we construct a variable indicating the religious majority in each municipality. Second, we exploit the highway access data from Fretz et al. (2021). Their dataset includes the distances from the centroid of each municipality to the nearest highway entry. Finally, our dataset also includes the urban typology as defined by the Federal Statistical Office (Swiss Federal Statistical Office 2005).

Pair-level variables include dummies equal to 1 if two municipalities are in different cantons or language regions, and if the majority of their residents are from a different religious majority. We measure the distance between two municipalities by computing the Euclidean distance between their centroids. Our distance and border variables are fixed over time because they represent the flight distance between municipalities. This distance measure is limited as it ignores important geographical features, such as mountains or lakes. However, this remains the best available approximation of the distance between units for the period covered in this study.

3.5 Descriptive statistics

3.5.1 Network descriptions

For the bipartite network aggregated at the municipal level,[10] nodes represent municipalities. The weighted links capture the number of director–firm connections between municipalities. In 1960, 1,480 municipalities host at least one director or one firm. The number increases to more than 2,100 in 1983, then stabilizes. By 2000, 87% of municipalities are connected. Over this period, the number of connections increases from 7,683 to 49,114. Nearly all municipalities are part of the largest connected component (LCC) of this aggregated network (96% to near 100%). The number of components drastically decreases from 59 to 4. However, the density remains small at 1% until 1987 and at 2% thereafter. The average municipality is connected to 10.38 other municipalities in 1960 and 45.06 in 2003. The maximum degree rises from 525 to 1,157. The large gap between the average and maximum degrees indicates a skewed degree distribution, with a small number of municipalities acting as hubs.

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[10] See Tables 3.C.1 to 3.C.3 in the Appendix for detailed descriptive statistics.

In the firm-to-firm projection aggregated at the municipal level, the number of municipalities hosting firms increases over time. The number of nodes rises from 998 in 1960 to approximately 2,000 by 1991. The share of Swiss municipalities with at least one firm increases from 40% to around 80%. Over the same period, the number of inter-municipal links increases from 3,866 to approximately 43,000, peaking at 48,644 in 1992, reflecting the densification of board interlocks. As in the bipartite case, almost all municipalities belong to the largest connected component, density remains low, and the degree distribution is highly skewed. The director-to-director projection has similar patterns. Here, nodes represent municipalities in which at least one director resides, and links count the number of boards shared by directors living in different municipalities.

3.5.2 Distance and borders: the structure of interlocking directorates in Switzerland

Figures 3.7 to 3.9 provide stylized facts on the spatial structure of the networks before turning to gravity equations. In each panel, the outcome is the link density within a distance bin. Link density is defined as the number of observed links between municipality pairs divided by the total number of links that could exist in that bin, excluding self-loops. The bars split dyads that are within a given border (e.g., same canton or language region) from dyads that it[11].

Two stylized facts stand out. First, all networks show strong distance decay. Link density is highest among close municipalities and declines rapidly as distance increases. At larger distances, densities approach zero, consistent with the sparsity of these networks. Second, there are visible within-border premiums in all networks. For nearly all distance bins, links are more frequent within cantons than across cantons, within language regions than across language borders, and within religion categories than across religion borders. This suggests that even conditional on geographic distance, border discontinuities reduce connections.

Importantly, these figures do not isolate the effect of a given border. Each figure conditions on only one border at a time, although the canton, language, and religion borders are correlated. As a result, part of what appears as language discontinuity may in fact reflect cantonal or religious differences. Gravity regressions allow us to address this issue.

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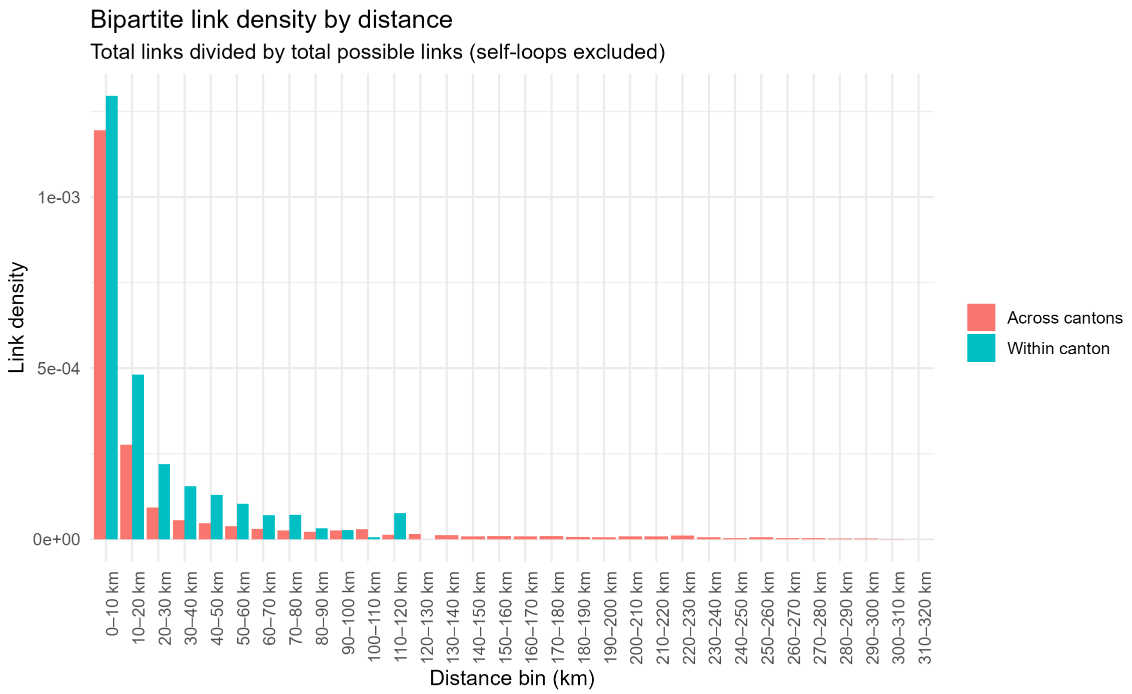
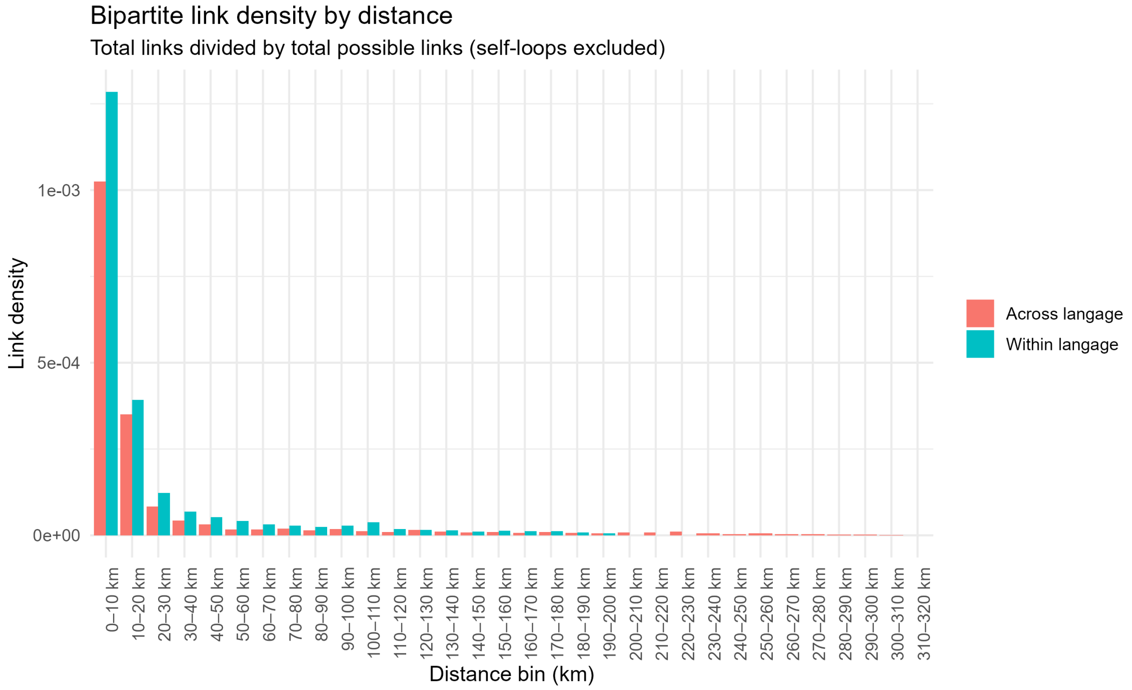
[11] The stylized figures focusing on religious borders are in Figure 3.C.4 in the Appendix

Figure 3.7: Density of connections - within vs. across borders - Bipartite network



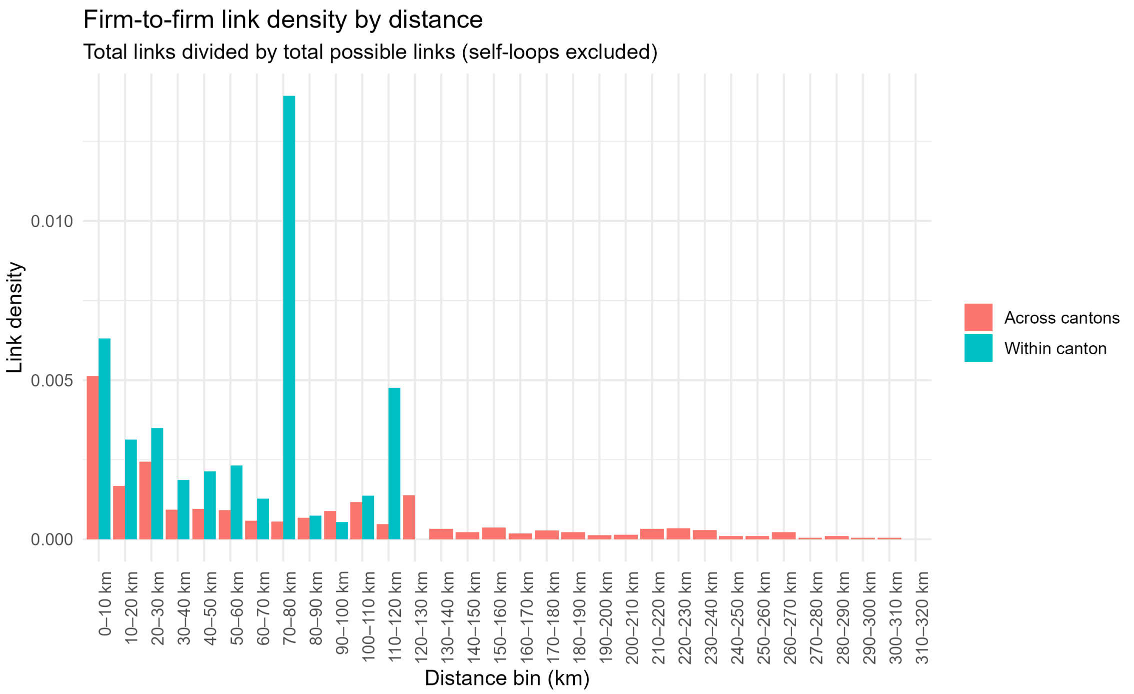
(a) Panel A: Cantonal borders



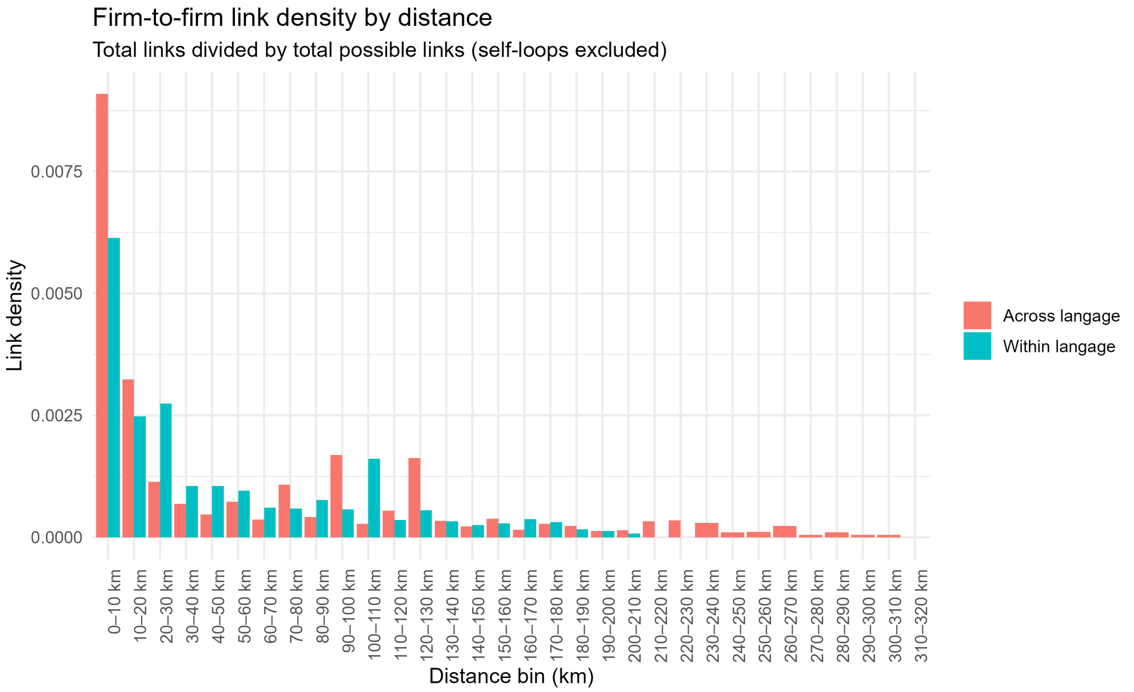
(b) Panel B: Language borders

Notes: Bars show link density by distance bin (observed links over possible links; self-loops excluded from the 0-10km bin), split into within-border and across-border dyads. Distances are between municipal centroids.

Figure 3.8: Density of connections - within vs. across borders - Firm-to-firm network



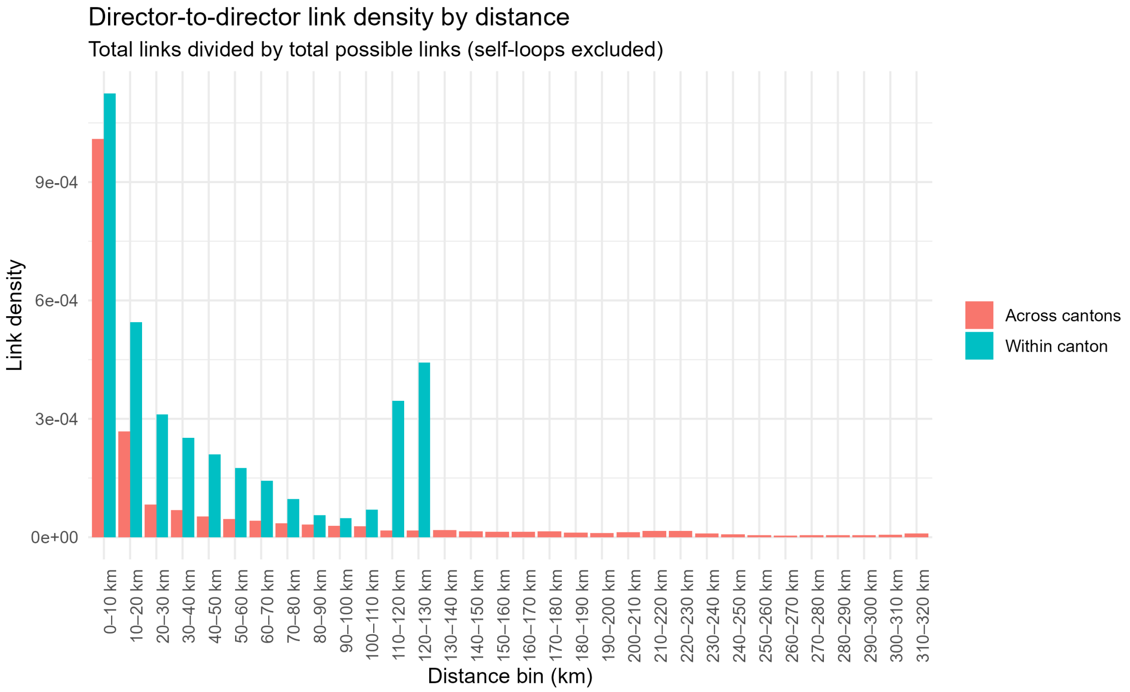
(a) Panel A: Cantonal borders



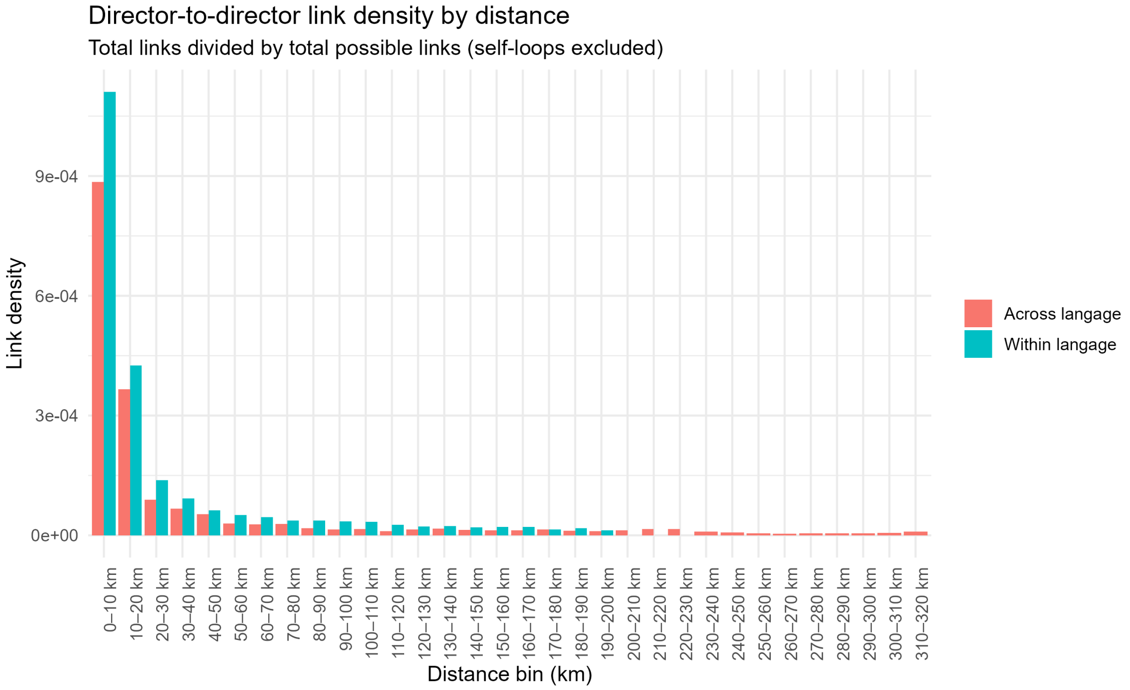
(b) Panel B: Language borders

Notes: Bars show link density by distance bin (observed links over possible links; self-loops excluded from the 0-10km bin), split into within-border and across-border dyads. Distances are between municipal centroids.

Figure 3.9: Density of connections - within vs. across borders - Director-to-directors network



(a) Panel A: Cantonal borders



(b) Panel B: Language borders

Notes: Bars show link density by distance bin (observed links over possible links; self-loops excluded from the 0-10km bin), split into within-border and across-border dyads. Distances are between municipal centroids. The figures are descriptive and do not isolate the partial effects of each border.

To complement the descriptive evidence on border effects, we examine the structure of the three networks using two standard network measures[12]. The E-I index compares cross-group ties with within-group ties and indicates that connections are more frequent within cantons and language regions than across them. This within-group bias persists across decades despite market integration and highway construction. The assortativity coefficients show the same pattern. Nodes are more likely to connect to nodes within the same borders. These patterns align with the visual evidence in Figures 3.7 to 3.9 and motivate the gravity framework, which quantifies the effects of distance, cantonal borders, language borders, and religion on connection intensity.

3.6 Empirical strategy

3.6.1 Two-way gravity estimates

The standard gravity model predicts that interaction intensity between two units increases with their size and declines with distance (Tinbergen 1962). However, Anderson and van Wincoop (2003) show that this raw gravity specification is biased due to omitted variables. To address this issue, we include origin and destination fixed effects[13] (Feenstra and Looi Kee 2004, Redding and Venables 2004). These fixed effects absorb multilateral resistance terms by controlling for time-invariant municipality-specific characteristics that may affect connections.

We start with an estimation of the following equation in each of our cross sections separately:

$$\begin{aligned} L_{ij} = \exp\Big[&\beta_0 + \beta_1 \ln Dist_{ij} + \beta_2 CtnBorder_{ij} \\ &+ \beta_3 LangBorder_{ij} + \beta_4 RelBorder_{ij} + \eta_i + \eta_j\Big]\nu_{ij}, \end{aligned} \tag{3.1}$$

where $CtnBorder_{ij}$ is a dummy taking the value 1 if i and j are in different cantons, and 0 otherwise. Similarly, $LangBorder_{ij}$ and $RelBorder_{ij}$ equal 1 when i and j are in different language regions and when they have different religious majorities, respectively. The municipal fixed effects for municipality i and j are η_i and η_j, respectively. ν_{ij} is the error term. Following the trade literature, we estimate the model

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[12] The two measures are formally described in the Appendix 3.C.5. Figures 3.C.5 to 3.C.10 present the evolution of both measures in each network over time.

[13] We will later use origin- and destination-year fixed effects as well. Note that the network is undirected. Technically, we do not have an origin or a destination. Here, we use the trade vocabulary for simplicity purposes, but we will sometimes call the origin- and destination-year fixed effects simply municipality-year fixed effects

using a Poisson Quasi-Maximum Likelihood (PQML) estimator[14]. PQML estimations allow zero values in L_{ij} and include unconnected pairs of municipalities. As a result, the dyadic panel contains two conceptually distinct types of zero observations. First, municipalities hosting firms or directors, but not connecting. Second, municipalities without firms or directors. In the first case, zeros represent genuine decisions not to connect and carry information about spatial frictions. Pairs from the second case switch to the first one over time, which affects the general interpretation of the zeros[15].

One limitation of our distance measure is that we locate all firms and directors at municipal centroids. The Euclidean distance between them is 0. This would be problematic in Equation (3.1) as we use the logarithm of the distance between all pairs of municipalities. In reality, connected firms or directors are distant from one another, even within municipalities. Therefore, we set the within-municipality distance to 1 km before applying the log transformation, so that $\ln(Dist_{ij})$ is well-defined for all pairs. For all other pairs, we use $\ln(Dist_{ij})$ directly, where $Dist_{ij}$ is the Euclidean distance in kilometres between municipal centroids.

This first step allows us to estimate the effects of cantonal, linguistic, and religious borders on the number of connections between municipalities in all periods. This allows the inclusion of the time-invariant distance variable in the regression. The main goal of using the two-way gravity approach in cross-sections is to estimate baseline magnitudes for the reference period we use in the three-way gravity estimations.

Finally, we estimate two-way gravity equations on the full panel and include municipality-year fixed effects. Pooling all years gives a single benchmark estimate of spatial frictions across the full sample, rather than a sequence of cross-sectional coefficients that vary with changes in sample composition. Municipality-year fixed effects absorb any year-specific changes in each municipality's propensity to form links. In practice, this improves precision by exploiting the full panel and yields a clean summary measure of the average distance decay and border penalties. However, it does not allow us to estimate how the distance and border effects evolve.

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[14] Estimating Equation (3.1) by OLS would exclude unconnected pairs of municipalities from the regression. This would be problematic, as most municipalities are unlinked, which could lead to estimation bias (Silva and Tenreyro 2006). PQML models with extensive fixed effects are estimated using *ppmlhdfe* (Correia et al. 2020).

[15] One robustness check would be to restrict the sample to a balanced panel of municipalities economically active in all periods between 1960 and 2000. This makes the composition of the zero structure constant and isolates the evolution of corporate network frictions from both entries and exits of new municipalities into the sample.

3.6.2 Three-way gravity estimates

When using cross-sections and two-way gravity estimations over all periods, we do not control for unobserved bilateral heterogeneity. As a result, our estimates still suffer from omitted-variable bias. As Baier and Bergstrand (2007) highlight, many factors can affect the number of connections, many of which are unobserved. The usual solution is to estimate three-way gravity models with municipality-year and municipality-pair fixed effects. Including the three different fixed effects absorbs all time-invariant characteristics at the individual and municipality-pair levels. This is particularly useful to estimate the impact of time-variant variables. In our case, however, all the border variables are time-invariant.

To address this issue, we follow Bergstrand et al. (2015) by interacting the time-invariant border dummies with year indicators D_t. By doing so, we can measure changes in bilateral distance and border effects. This approach allows us to account for average declines in unobservable bilateral connection costs (Bergstrand et al. 2015). We can apply their approach since we have data on connections within and across cantonal borders. Because municipality-pair fixed effects absorb all time-invariant bilateral characteristics, only the interactions of distance and border variables with year indicators are identified.

Concretely, we estimate Equation (3.2) using the full dyadic panel of municipalities:

$$\begin{aligned} L_{ij,t} = \exp \Big[& \beta_1 \Big(\ln Dist_{ij} \cdot D_t \Big) + \beta_2 \Big(CtnBorder_{ij} \cdot D_t \Big) + \beta_3 \Big(LangBorder_{ij} \cdot D_t \Big) \\ & + \beta_4 \Big(RelBorder_{ij} \cdot D_t \Big) + \eta_{i,t} + \eta_{j,t} + \phi_{ij} \Big] \nu_{ij,t}, \end{aligned} \tag{3.2}$$

where $\beta_1, \beta_2, \beta_3, \beta_4$ are vectors of year-specific coefficients, one for each period t, with a reference year normalization in 1960. Here, ϕ_{ij} represents the municipality-pair fixed effects, D_t is the year dummy, and the remainder of the equation is similar to Equation (3.1). Robust standard errors are clustered at the municipality-pair level.

3.6.3 Exogenous distance variation - Highway access

The gravity regressions in Sections 3.6.1 and 3.6.2 estimate how inter-municipal corporate connections vary with distance and border indicators, conditional on various fixed effects. This section exploits variation in highway access timing as an arguably exogenous source of variation in distance. We use highway openings to estimate the effects of reductions in road distance and travel time on inter-municipal connections.

We use gravity equations to estimate the effect of municipality-pair highway access on their connections. Highways affect bilateral connections by reducing transportation costs. This motivates a dyadic panel design with municipality-pair fixed effects that absorb all time-invariant bilateral frictions, thereby identifying the effect of highway access using within-pair changes over time.

Our main goal is to test whether highway access matters for corporate connections at all. We define a municipality-year indicator of highway access within a radius of 15 km from the municipal centroid:

$$A_{i,t}(r) \;=\; \mathbf{1}\{\text{municipality } i \text{ has access within } r \text{ km in period } t\} \tag{3.3}$$

From $A_{i,t}(r)$, we construct the pair-level indicator $Access_{ij,t}$ that indicates connectivity of either i or j, or both municipalities. This allows us to compare dyads with two connected municipality with unilaterally connected dyads[16]:

$$Access_{ij,t} \;=\; \mathbf{1}\{A_{it} = 1 \text{ or } A_{jt} = 1\}\,, \tag{3.4}$$

Let $L_{ij,t}$ denote the number of corporate connections between municipalities i and j in period t. We estimate a PQML gravity specification on the dyadic panel:

$$L_{ij,t} \;=\; \exp\Big[\beta_1\, Access_{ij,t} + \eta_{it} + \eta_{jt} + \phi_{ij}\Big]\nu_{ij,t}, \tag{3.5}$$

where ϕ_{ij} are municipality-pair fixed effects absorbing all time-invariant dyad characteristics (including physical distance and borders), and η_{it} and η_{jt} are municipality-by-time fixed effects. Standard errors are clustered at the municipality-pair level.

To estimate the potential evolution of the effect over time, we include several lags of the treatment variable. Equation (3.7) becomes[17]:

$$\begin{aligned} L_{ij,t} = \exp\Big[&\beta_1 Access_{ij,t} + \beta_2 Access_{ij,t-1} + \beta_3 Access_{ij,t-2} \\ &+ \beta_4 Access_{ij,t-3} + \eta_{i,t} + \eta_{j,t} + \phi_{ij}\Big]\nu_{ij,t}. \end{aligned} \tag{3.6}$$

Reverse causality remains an issue in our setting. Connections and economic interactions can drive the construction of highways. We account for this potential bias

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[16] Since $\mathbf{1}\{A_{it} = 1 \text{ or } A_{jt} = 1\} = A_{it} + A_{jt} - A_{it}A_{jt}$ and municipality-year fixed effects absorb the unilateral components A_{it} and A_{jt}, β_1 is identified off bilateral access $A_{it}A_{jt}$. Our estimates capture the effect of both endpoints having highway access, relative to pairs where only one endpoint is connected, rather than to fully unconnected pairs. This makes our estimates a conservative lower bound on the full bilateral highway access effect. Separating unilateral and bilateral access is left for future work.

[17] Each lag represents a 3-year gap.

by including $Access_{ij,t+1}$ in our regression equation (Wooldridge 2010, Bergstrand et al. 2015). If this lead is close to 0, we can be more confident in the exogeneity of the treatment:

$$\begin{aligned} L_{ij,t} = \exp \Big[& \beta_1 Access_{ij,t} + \beta_2 Access_{ij,t-1} + \beta_3 Access_{ij,t-2} \\ & + \beta_4 Access_{ij,t-3} + \beta_5 Access_{ij,t+1} + \eta_{i,t} + \eta_{j,t} + \phi_{ij} \Big] \nu_{ij,t}. \end{aligned} \tag{3.7}$$

As we expect borders to have an impact on inter-municipal connections, we also estimate Equation (3.7) including border dummies[18]:

$$\begin{aligned} L_{ij,t} = \exp \Big[& \beta_1 \, Access_{ij,t} + \beta_2 \Big(\ln Dist_{ij} \cdot D_t \Big) \\ & + \beta_3 \Big(CtnBorder_{ij} \cdot D_t \Big) + \beta_4 \Big(LangBorder_{ij} \cdot D_t \Big) \\ & + \beta_5 \Big(RelBorder_{ij} \cdot D_t \Big) + \eta_{i,t} + \eta_{j,t} + \phi_{ij} \Big] \nu_{ij,t}. \end{aligned} \tag{3.8}$$

3.7 Main results

For PQML regressions, we measure the effect of dummy variables change on municipality-pair connections as (Yotov et al. 2016):

$$\% \text{ change in } L_{ij} = \Big[e^{\hat{\beta}_{dummy}} - 1 \Big] \cdot 100 \tag{3.9}$$

For continuous variables, we use the standard elasticity approximation. For example, $\hat{\beta}_1 = -1$ implies that a 10% increase in distance decrease connections by 10%. Border coefficients capture the discrete drop (or increase) in connectivity when a municipality-pair is on different sides of a border, compared to pairs on the same side. They should be read as the border penalty (or premium) conditional on distance and the other borders included in the regression. A negative coefficient indicates that, for two municipalities at the same distance, crossing a border decreases connections.

3.7.1 Two-way gravity results

Bipartite aggregated network

Figure 3.10 presents the cross-sectional PQML estimates of Equation (3.1). We estimate the regressions separately for each cross-section and plot the coefficients

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[18] We also run Equation (3.8) including 3 lags of $Access_{ij,t}$. Finally, we modify Equation (3.8) to include one lead and three lags, as in Equation (3.9).

over time. This approach allows us to illustrate how the average importance of each friction evolves over the 1960–2000 sample.

The distance coefficient (Panel A) is negative across all cross-sections. Municipalities farther apart form fewer firm-to-director connections. Panels B and C show the negative effects of cantonal and linguistic borders on the number of connections. Cross-cantonal pairs have systematically fewer corporate ties than within-canton pairs at the same physical distance. Similarly, Panel C shows a negative effect of language borders on municipality connections. The patterns suggest that distance and cantonal border penalties are slightly more important in 2000 than in earlier periods. The effect of language borders on connections between firms and directors is stable over time, while religious borders do not seem to play a major role (Panel D).

The cross-sectional estimates should be interpreted with caution. In the absence of pair and municipal-year fixed effects, these estimates do not account for multilateral resistance terms, progressive entry of small municipalities (i.e., selection into the network), and time-invariant unobserved heterogeneity between municipality pairs. All three forces can amplify the estimated border coefficient over time.

Table 3.D.6 in the Appendix complements Figure 3.10 and reports the pooled two-way gravity estimates with municipality-year fixed effects. In the fully specified model (column 3), the distance elasticity is -1.356, implying that a 10% increase in inter-municipal distance is associated with roughly a 13.56% decrease in expected connections. Cantonal and language borders imply about 70% and 67% fewer links, respectively, while religious borders imply about 11% fewer links.

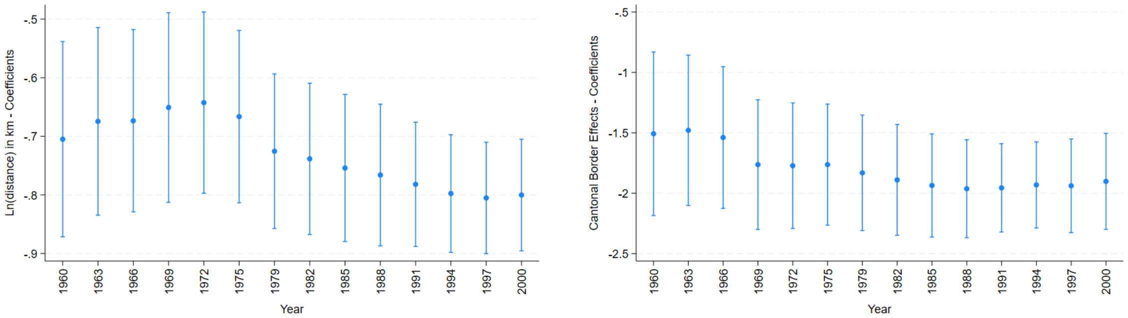
Firm-to-firm aggregated network

Figure 3.11 reports cross-sectional PQML estimates of Equation (3.1) for the firm-to-firm aggregated network. We estimate the model separately for each cross-section and plot the distance and border coefficients over time.

Panel A shows a negative distance gradient in all years. Municipalities farther apart form fewer inter-municipal firm-to-firm ties. Cross-cantonal municipality pairs form fewer firm connections than within-canton pairs (Panel B). The cantonal border penalty appears stable over time. Panel C shows strong negative language border effects. By contrast, religious borders matter less and vary more over time.

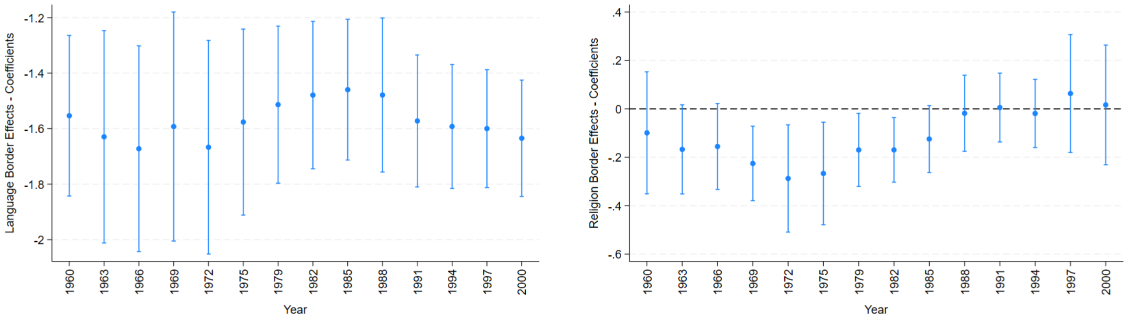
Appendix Table 3.D.8 reports the two-way gravity estimates including municipality-year fixed effects. In the fully specified model (column 3), the distance elasticity is -0.677, indicating a 6.77% decline in connections for a 10% increase in distance. Cantonal borders imply about 56% fewer links, language borders about 65% fewer

Figure 3.10: Cross-sectional results - Bipartite network



(a) Panel A: Distance

(b) Panel B: Cantonal borders

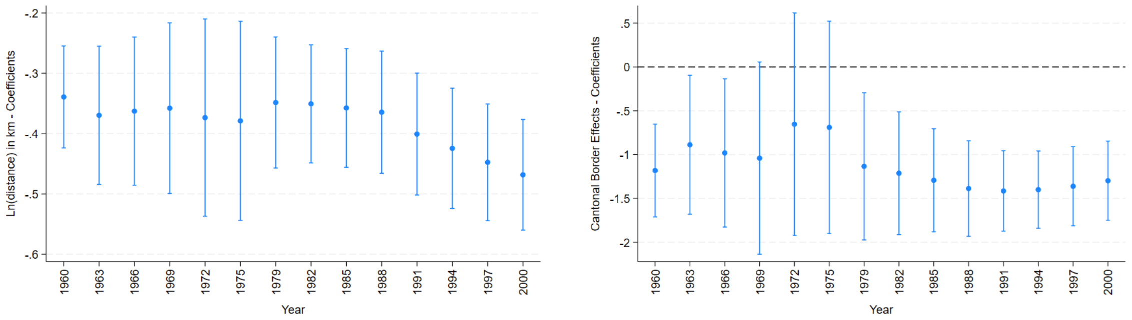


(c) Panel C: Language borders

(d) Panel D: Religion borders

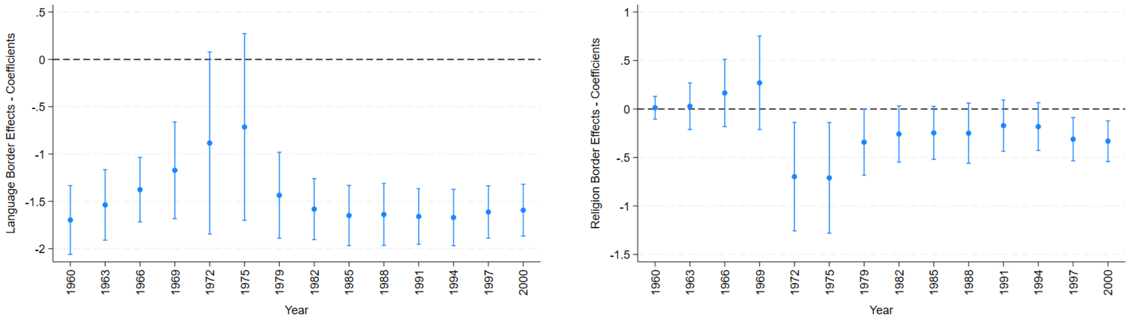
Notes: Each marker reports the coefficient estimate from separate cross-sectional PQML regressions of director-to-firm (bipartite) link counts between municipality pairs on log distance and border indicators, including origin and destination fixed effects (i.e., using Equation (3.1)). Vertical bars denote 95% confidence intervals, based on robust standard errors clustered by origin and destination. Table 3.D.5 provides the detailed results.

Figure 3.11: Cross-sectional results - Firm-to-firm network



(a) Panel A: Distance

(b) Panel B: Cantonal Borders



(c) Panel C: Language Borders

(d) Panel D: Religion Borders

Notes: Each marker reports the coefficient estimate from separate cross-sectional PQML regressions of firm-to-firm link counts between municipality pairs on log distance and border indicators, including origin and destination fixed effects (i.e., using Equation (3.1)). Vertical bars denote 95% confidence intervals, based on robust standard errors clustered by origin and destination. Table 3.D.7 provides the detailed results.

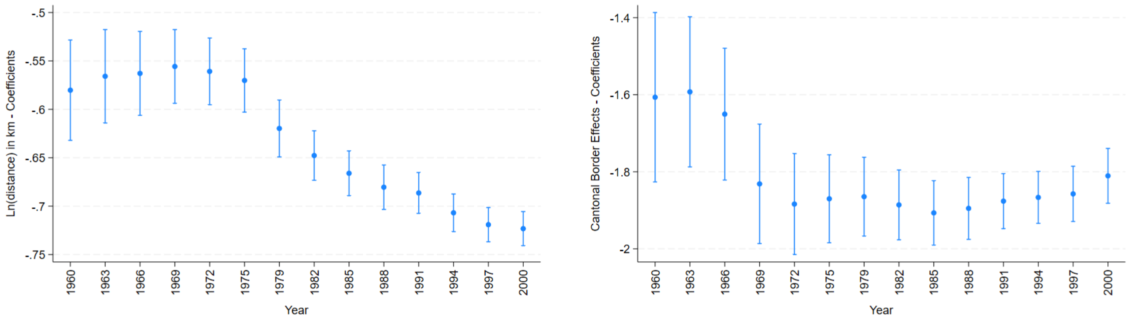
links, and religious borders about 25% fewer links. Relative to the bipartite network, firm-to-firm connections are less sensitive to distance but remain strongly segmented by cantonal and language borders.

Director-to-director aggregated network

Figure 3.12 reports cross-sectional PQML estimates of Equation (3.1) for the director-to-director aggregated network.

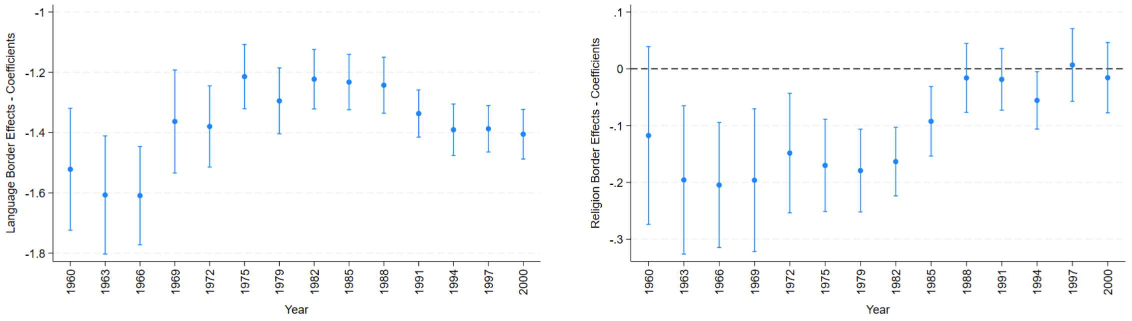
Again, Panel A shows fewer director-to-director connections between municipalities farther apart. Cross-cantonal municipality pairs also form fewer director connections than within-canton pairs (Panel B). Panel C shows strong negative language-border effects. By contrast, religious borders matter less and become negligible in later periods.

Figure 3.12: Cross-sectional results - Director-to-director network



(a) Panel A: Distance

(b) Panel B: Cantonal Borders



(c) Panel C: Language Borders

(d) Panel D: Religion Borders

Notes: Each marker reports the coefficient estimate from separate cross-sectional PQML regressions of director-to-director link counts between municipality pairs on log distance and border indicators, including origin and destination fixed effects (i.e., using Equation (3.1)). Vertical bars denote 95% confidence intervals, based on robust standard errors clustered by origin and destination.

In the fully specified model, the distance elasticity implies that a 10% increase in distance is associated with about 12% fewer director-to-director connections (see

Appendix Table 3.D.10). Cantonal borders display a large penalty, corresponding to roughly 72% fewer director ties for cross-cantonal municipality-pairs relative to within-canton pairs. Language borders are similarly important, with about 62% fewer links across them. Religious borders imply about 11% fewer links.

3.7.2 Three-way gravity results

The three-way gravity specification adds municipality-pair fixed effects. These fixed effects absorb all time-invariant bilateral characteristics. The coefficients are identified from within-pair changes over time and provide a cleaner picture of how distance and border frictions evolve.

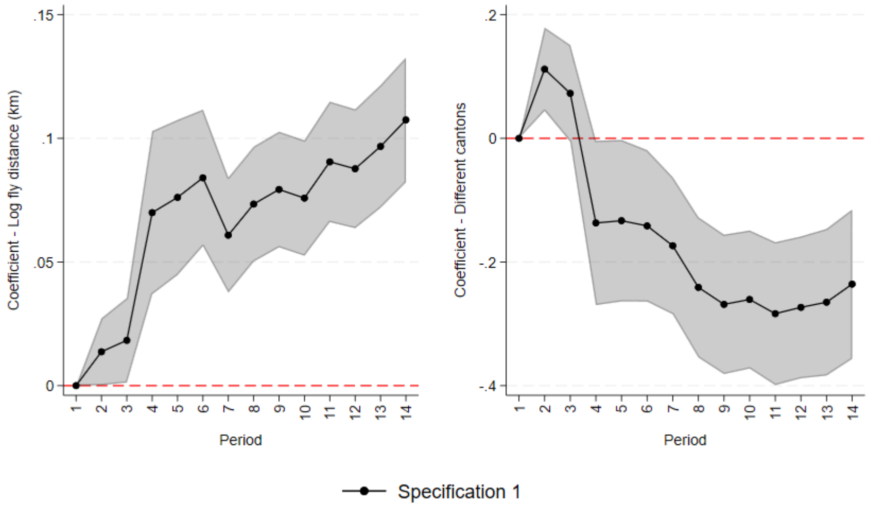
Bipartite aggregated network

Figure 3.13 presents the three-way gravity estimates from Equation (3.2) for the bipartite network. Each coefficient measures the change in a given friction relative to 1960, after absorbing all time-invariant pair heterogeneity through municipality-pair fixed effects ϕ_{ij} and all time-varying municipal characteristics through municipality-year fixed effects η_{it} and η_{jt}.

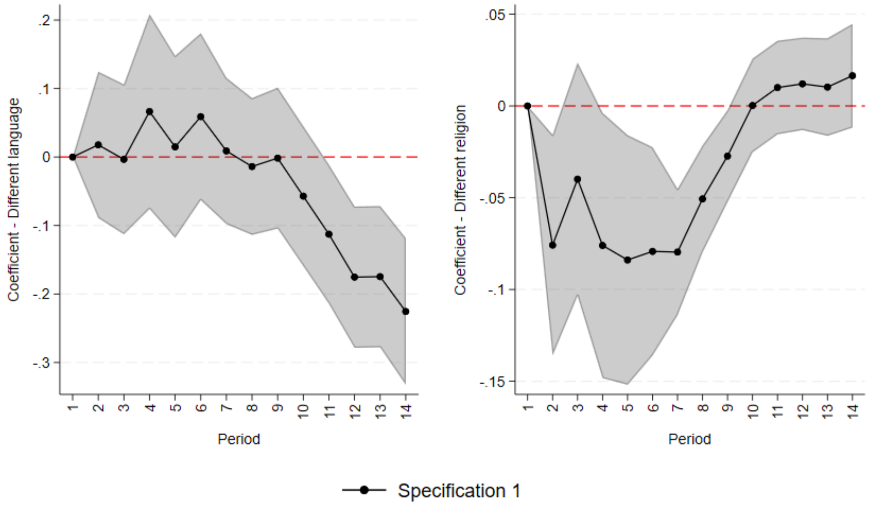
We observe three different patterns. First, the distance coefficients become increasingly positive (Panel A), implying that distance penalizes long connections less than in 1960. Second, the effect of cantonal and linguistic borders is increasing over time. Cross-cantonal and cross-language municipality pairs form fewer links in 2000 relative to 1960. Third, religious border coefficients are negative in early periods, but drift back toward zero in the last periods.

These estimates describe how the geography of director–firm ties evolves relative to 1960. The increase in the distance coefficient implies that long-distance appointments have become relatively more common. At the same time, the increasing cantonal-border friction indicates stronger within-canton concentration of mandates. For two municipality pairs at the same distance, connections are progressively less likely when they cross a cantonal border. The language-border coefficients indicate a growing linguistic segmentation of director–firm links relative to 1960. Directors have more mandate in firms located in municipalities within the same language region as their residence municipality.

Figure 3.13: Three-way gravity results - Bipartite network



(a) Panel A: Distance & Cantonal Borders

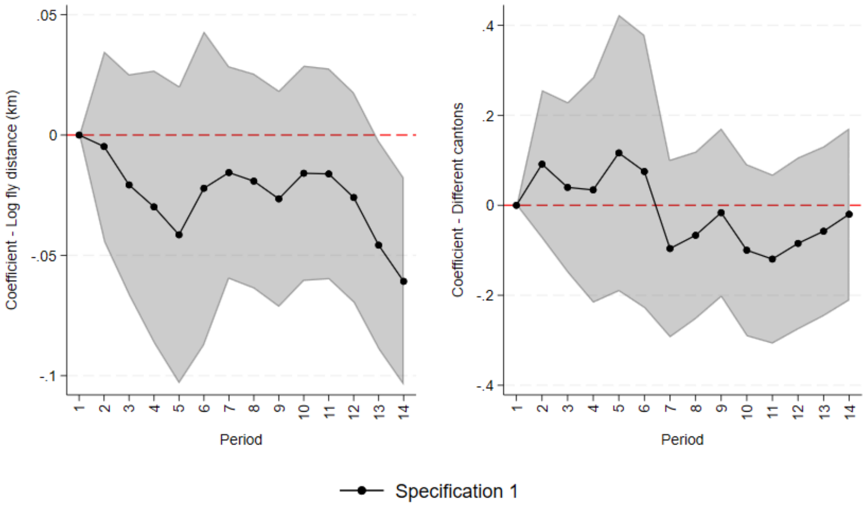


(b) Panel B: Language & Religion Borders

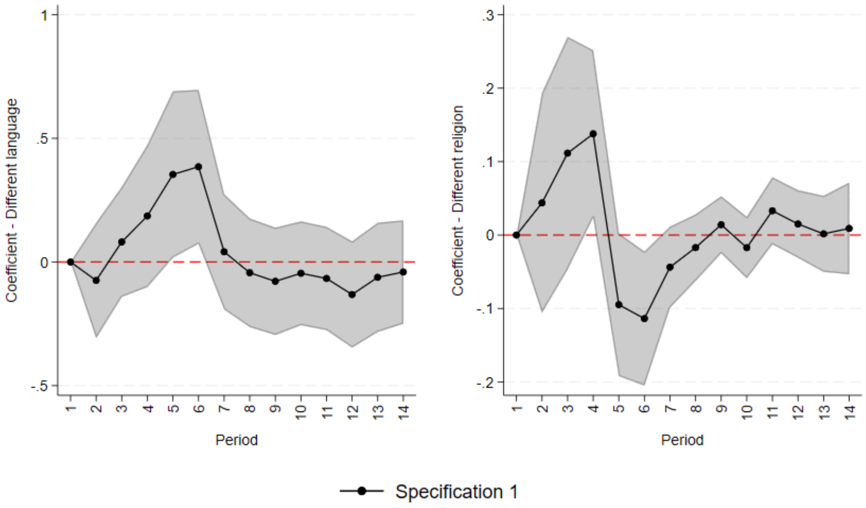
Notes: Period 1 corresponds to 1960 and serves as the reference period. Every period corresponds to 3 years, except between periods 6 and 7, where the gap is 4 years (1975 to 1979). Robust standard errors clustered by municipality pair.

Firm-to-firm aggregated network

Figure 3.14: Three-way gravity results - Firm-to-firm network



(a) Panel A: Distance & Cantonal Borders



(b) Panel B: Language & Religion Borders

Notes: Period 1 corresponds to 1960 and serves as the reference period. Every period corresponds to 3 years, except between periods 6 and 7, where the gap is 4 years (1975 to 1979). Robust standard errors clustered by municipality pair.

Figure 3.14 shows how frictions in the firm-to-firm network evolve relative to 1960. The distance estimates are mostly negative and become increasingly negative in the last periods, suggesting that long-distance firm connections are less frequent. Cantonal borders coefficients remain close to the reference period. There is a temporary increase in cross-language connections relative to 1960, but it returns to levels similar to those in 1960. Religious-border coefficients are more volatile, but remain mostly close to 0.

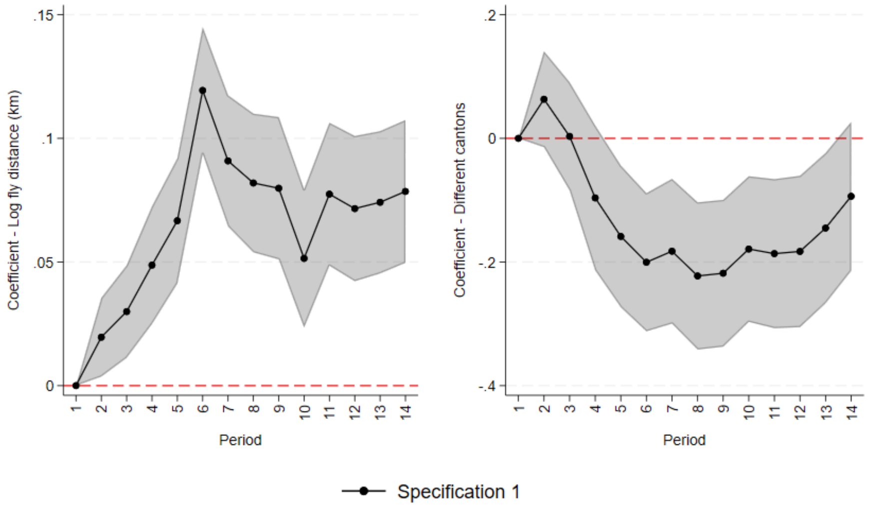
These estimates describe how the geography of inter-firm connections has shifted relative to 1960. In practical terms, firm additional ties over time increasingly come from closer municipalities. The border penalties appear broadly similar in 2000 relative to 1960, with some episodes of stronger or weaker effects for linguistic borders.

Director-to-director aggregated network

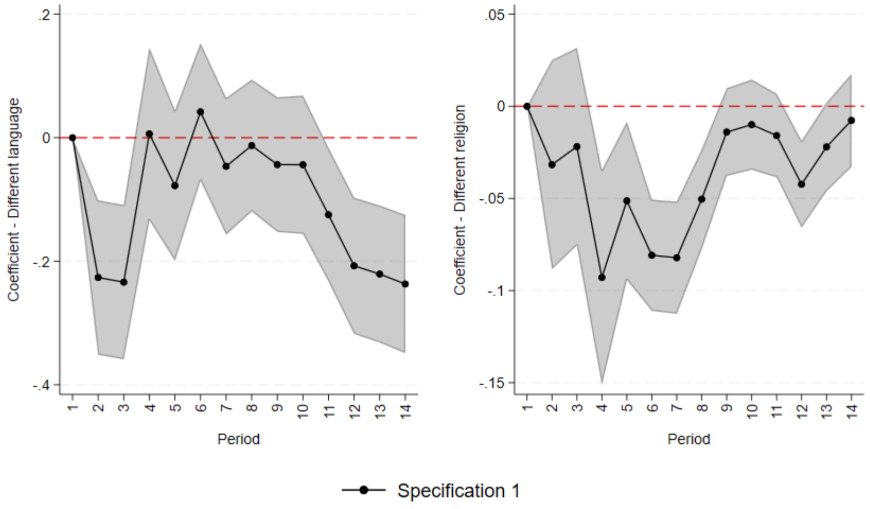
Figure 3.15 reports the three-way gravity estimates for the director-to-director network. The distance interaction increases up to around period 6, then declines slightly and stabilizes, indicating that long-distance director connections become relatively more frequent than in 1960. The cantonal border coefficients decrease in the early periods, then stabilize and recover in later periods. Language borders coefficients are initially negative, then move back toward zero, and become negative again in later decades. Finally, religious-border coefficients converge back toward zero in recent periods.

Overall, the director network shows an increase in long-distance connections in 2000 compared to 1960. By the end of the sample, they tend to reside more often in the same canton and within the same language region.

Figure 3.15: Three-way gravity results - Director-to-director network



(a) Panel A: Distance & Cantonal Borders



(b) Panel B: Language & Religion Borders

Notes: Period 1 corresponds to 1960 and serves as the reference period. Every period corresponds to 3 years, except between periods 6 and 7, where the gap is 4 years (1975 to 1979). Robust standard errors clustered by municipality pair.

3.7.3 Highway access results

This section examines whether reductions in road distance, induced by the construction of the Swiss highway network, affect connections between municipalities. We consider a pair of municipalities as jointly treated when both municipalities have highway access within 15 km of their centroids, as our estimates identify the effect of bilateral relative to unilateral access. We use the staggered timing of openings to estimate dynamic effects in a three-way gravity framework with municipality-pair and municipality-time fixed effects.

Bipartite aggregated network

Table 3.1 reports the main results for the municipal bipartite network. The table presents either the average effect of access opening ($Access_{ij,t}$) or dynamic leads and lags relative to the access year (e.g., $Access_{ij,t-1}$). Specifications (1) to (3) do not include the distance and borders variables. Specifications (4)-(6) include them. Table 3.D.15 reports the full set of estimates.

In specification (1), the coefficient of 0.035 indicates that when both municipalities in a pair have highway access, the expected number of connections increases by 3.56% relative to pairs where only one municipality is connected. Once we include lags in specification (2), we find an increase only in $Access_{ij,t-1}$. The estimates for the next periods are closer to 0 again. The total effect of the access opening is positive, and the gain from highway access suggests an increase of 7.47% in connections. The pre-treatment coefficient $Access_{ij,t+1}$ suggests no violation of the parallel trend assumption in specification (3).

We add distance and border variables in specifications (4)-(6). The estimated access effects remain small, and their signs and magnitudes stay close to the estimates in specifications (1)-(3). This suggests that the distance and internal borders do not drive the positive association between highway access and firm-director connections. The results rather show that highways are of second-order importance, relative to the large and persistent effects of distance and internal borders. In specification (6), the significant $Access_{ij,t+1}$ coefficient suggests that part of the post-treatment increase may reflect pre-existing dynamics and parallel trend assumption violations. In this setting, one possible interpretation is anticipation of openings by either firms or directors. Another is that our access measure may also understate travel time reductions. Even when neither municipality have an access within 15 km, newly constructed highway segments elsewhere may still substantially reduce travel time between them. Lead coefficients may therefore capture earlier travel-time reductions from highway openings elsewhere between the two municipalities, before either endpoint gains direct access.

Our results indicate that highway access slightly increases the firm-director connections between newly connected areas. Improved infrastructure reduces travel times between connected municipalities, facilitating board participation and expanding the reach of firms seeking directors. The estimated effects remain modest in magnitude relative to the baseline distance elasticity. This finding aligns with the broader evidence in this chapter that internal borders and spatial frictions persist despite large infrastructure investments.

Table 3.1: Three-way gravity results - Highway access (Bipartite network)

	(1)		(2)		(3)		(4)		(5)		(6)	
$Access_{ij,t}$	0.035	(0.024)	0.011	(0.021)	0.011	(0.017)	0.041*	(0.023)	0.022	(0.020)	0.003	(0.017)
$Access_{ij,t-1}$			0.039**	(0.018)	0.039**	(0.018)			0.032*	(0.018)	0.034*	(0.018)
$Access_{ij,t-2}$			0.006	(0.016)	0.006	(0.016)			0.002	(0.017)	0.004	(0.017)
$Access_{ij,t-3}$			0.017	(0.016)	0.017	(0.016)			0.019	(0.015)	0.021	(0.015)
$Access_{ij,t+1}$					-0.000	(0.022)					0.041*	(0.022)
Total *Access*			0.072	(0.041)	0.073	(0.041)			0.074	(0.040)	0.061	(0.040)
# Observations	1,337,379		1,337,379		1,337,379		1,337,379		1,337,379		1,337,379	
Pseudo R^2	0.900		0.900		0.900		0.900		0.900		0.900	

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of director-to-firm link counts between municipality pairs. All specifications include origin-time and destination-time fixed effects, as well as municipality-pair fixed effects. Specifications (4) to (6) include distance and border variables. Table 3.D.15 shows the complete results. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Firm-to-firm aggregated network

Table 3.2 presents the main results for the municipal firm-to-firm network. The specifications are similar as in Table 3.1. Specifications (1) to (3) estimate the access effect without frictions, while specifications (4) to (6) add them. Table 3.D.16 shows all estimates.

The results reveal larger effects than in the bipartite network. Specification (1) shows that the opening of a highway access within 15 km of both municipalities in a pair increases the number of firm-to-firm links between them by around 12% compared to pairs with only one municipality with an access. This effect disappears once we introduce lagged variables in specification (2). The first and second lags appear to be driving the effects in our first specification. The lead coefficient in specification (3) is negative and relatively small compared to the first lag. The total effect suggests that the cumulative increase in firm-to-firm links attributable to highway access is 35%.

The addition of distance and border controls alters the results of our estimations. The first lag remains the largest estimated coefficient, but the total effects are lower.

However, the lead in specification (6) gets larger (-0.191). This suggests that treated pairs already differ from untreated pairs before access opens. Therefore, interpretations of the results require caution, even though the first lag remains the largest coefficient across all specifications.

Table 3.2: Three-way gravity results - Highway access (Firm-to-firm network)

	(1)		(2)		(3)		(4)		(5)		(6)	
$Access_{ij,t}$	0.113*	(0.059)	-0.025	(0.047)	0.009	(0.034)	0.050	(0.060)	-0.073*	(0.044)	-0.005	(0.037)
$Access_{ij,t-1}$			0.282***	(0.087)	0.275***	(0.081)			0.261***	(0.092)	0.241***	(0.083)
$Access_{ij,t-2}$			0.053*	(0.030)	0.049*	(0.029)			0.059*	(0.032)	0.052	(0.032)
$Access_{ij,t-3}$			-0.010	(0.056)	-0.013	(0.056)			-0.065	(0.052)	-0.071	(0.053)
$Access_{ij,t+1}$					-0.087	(0.080)					-0.191**	(0.080)
Total *Access*			0.300	(0.113)	0.321	(0.118)			0.181	(0.122)	0.216	(0.126)
# Observations	1,305,738		1,305,738		1,305,738		1,305,738		1,305,738		1,305,738	
Pseudo R^2	0.978		0.978		0.978		0.978		0.978		0.978	

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of firm-to-firm link counts between municipality pairs. All specifications include origin-time and destination-time fixed effects, as well as municipality-pair fixed effects. Specifications (4) to (6) include distance and border variables. Table 3.D.16 shows the complete results. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Director-to-director aggregated network

Table 3.3 shows the main results for the municipal director-to-director network, with all the estimates available in Table 3.D.17.

The results contrast with the other networks. Specification (1) suggests that both municipalities in a pair having highway access increase director-to-director connections by around 4% relative to pairs where only one municipality is connected. Once we include lags, no dynamic effects appear. The contemporaneous effect and the first two lags are slightly positive, while the third lag is essentially zero. The total access effect indicates an increase in connections by around 6%. The lead in specification (3) shows no pre-trend or violation of the parallel trend assumption.

Adding distance and border variables does not affect the results. The lead in specification (6) is positive and small, but larger than in specification (3). This raises concern about differential pre-trends. Overall, the director-to-director results suggest that highway access openings have at most small effects on the connections between municipalities.

Table 3.3: Three-way gravity results - Highway access (Director-to-director network)

	(1)		(2)		(3)		(4)		(5)		(6)	
$Access_{ij,t}$	0.038	(0.024)	0.023	(0.023)	0.022	(0.021)	0.041*	(0.024)	0.029	(0.023)	0.019	(0.021)
$Access_{ij,t-1}$			0.020	(0.016)	0.020	(0.016)			0.020	(0.016)	0.021	(0.016)
$Access_{ij,t-2}$			0.013	(0.014)	0.013	(0.014)			0.008	(0.014)	0.008	(0.014)
$Access_{ij,t-3}$			0.001	(0.014)	0.002	(0.014)			-0.017	(0.014)	-0.015	(0.014)
$Access_{ij,t+1}$					0.001	(0.019)					0.021	(0.019)
Total *Access*			0.057	(0.033)	0.057	(0.032)			0.040	(0.033)	0.033	(0.032)
# Observations	1,833,416		1,833,416		1,833,416		1,833,416		1,833,416		1,833,416	
Pseudo R^2	0.830		0.830		0.830		0.830		0.830		0.830	

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of director-to-director link counts between municipality pairs. All specifications include origin-time and destination-time fixed effects, as well as municipality-pair fixed effects. Specifications (4) to (6) include distance and border variables. Table 3.D.17 shows the complete results. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

3.7.4 Network comparisons and interpretation

Table 3.4 shows differences across the three networks using the pooled two-way gravity equations. The bipartite estimates indicate a 13.56% decline in connections for a 10% increase in distance between municipalities. The corresponding decline is 11.67% in the director-to-director network and only 6.77% in the firm-to-firm network. By contrast, cantonal and language border effects remain large across all three networks. Religious borders are the weakest friction across all three networks.

The three-way gravity results of the evolution of the distance and border effects point in the same direction. In the bipartite and director-to-director networks, the distance interaction coefficients turn increasingly positive over time, indicating that connections span over longer distances than in 1960. Yet, cantonal and language border penalties simultaneously become more important across both networks. In contrast, the firm-to-firm network does not display a comparable reduction in distance frictions. The late coefficients even suggest that long-distance connections become less frequent. Border effects do not weaken over time, suggesting that cantonal and linguistic boundaries remain persistent features. These observations imply that even if directors become more mobile, firm-to-firm connections remain geographically clustered.

The three networks respond differently to the reduction in road distance, as the highway access results suggest. In the bipartite network, the opening of a highway access implies an increase of around 7% of connections. The firm-to-firm network shows the largest effect, with a cumulative effect of around 35%. By contrast, the estimates indicate an increase of approximately 6% in the director-to-director network. These results suggest that the main barrier to corporate network connections is not

Table 3.4: Two-way gravity results - All periods & networks

	Bipartite (1)	Firm-to-firm (2)	Director-to-director (3)
Ln(flight dist. - km)	-1.356***	-0.677***	-1.167***
	(0.022)	(0.066)	(0.018)
Cantonal borders	-1.189***	-0.823***	-1.288***
	(0.052)	(0.258)	(0.042)
Language borders	-1.104***	-1.064***	-0.956***
	(0.067)	(0.262)	(0.041)
Religious borders	-0.113***	-0.288***	-0.122***
	(0.026)	(0.100)	(0.018)
# Observations	47,474,748	30,668,619	38,282,037
Origin x year FE	Yes	Yes	Yes
Destination x year FE	Yes	Yes	Yes

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of link counts between municipality pairs on log distance and border indicators for all three networks and in the full specification. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

physical distance. The primary barriers are cultural and institutional, and they seem far more persistent.

The interaction between agglomeration economies and market expansion can help interpret the results. Large cities such as Zürich, Basel, and Geneva concentrate both firms and directors and act as hubs. As the Swiss economy integrates, firms in these hubs seek access to expand their markets and need local information to do so. Rather than relocating, they recruit directors from farther away. This process increases long-distance connections across all networks. At the same time, most firms in the sample are small and remain locally embedded. They expand first within local and regional markets, recruit nearby directors, and connect mainly to firms in their immediate surroundings. The aggregate firm-to-firm distance effect therefore reflects a combination of two types of firms. A small number of large hub firms produce long-distance ties across cities and agglomerations, and a much larger number of locally embedded firms generate short-distance connections.

Director appointment decisions depend on face-to-face interactions, which are costly to sustain across distance. The search-and-matching logic of Helsley and Strange (1990) and the peer-referral mechanism of Koskinen and Edling (2012) both predict a strong local concentration of board ties, because directors recommend acquaintances they personally know. Interlocks generate information, monitoring, and coordination benefits, which makes them valuable to firms even across space (Battaggion and Cerasi 2020, Loderer and Peyer 2002, Mizruchi 1996). However, the cost of these connections still increases with distance (Johnson and Gilles 2003, Jackson and Rogers 2005, Galeotti et al. 2006). These mechanisms fit the results well. Falling distance costs mainly affect directors, while cantonal and language borders remain persistent because they reflect cultural segmentation and increase communication and matching costs (Melitz and Toubal 2014, Egger and Lassmann 2015). Language barriers raise communication and matching costs, and their relative importance may even increase when transport costs fall (Schulze and Wolf 2009). This helps explain why language borders remain important even when distance declines. By contrast, religious borders appear weaker and less persistent, which is consistent with the broader evolution of religion in Switzerland.

Although the three-way gravity approach eliminates concerns from the two-way gravity equations, some limitations remain. The selection into network issue is only partly addressed. Border-year interaction coefficients in later periods are estimated on a progressively different composition of pairs. Late-entering pairs, if smaller and more locally concentrated, can create an upward bias in the estimated border friction in later years. Our results also include movers, which could explain the increasing distance penalty on connections and reflect the relocation of directors or firms in suburban and rural municipalities. Any transportation improvements can allow directors to commute farther than previously, while firms can relocate outside cities and still reach their large markets.

3.8 Conclusion

This chapter studies how geography and internal borders shape corporate connections in Switzerland between 1960 and 2000. Using data on the universe of Swiss corporations and their directors, we construct three related municipal-level networks: the bipartite network, the firm-to-firm projection, and the director-to-director projection. The empirical analysis combines descriptive network evidence with gravity-style dyadic regressions to estimate how distance, cantonal borders, language borders, and religious differences affect municipal connections in each network, and how these frictions evolve over time.

Three main findings emerge. First, geography strongly shapes all three networks. Distance reduces inter-municipal connectivity throughout, and cantonal and language borders are associated with substantially fewer links. Religious differences matter much less. Second, these frictions evolve differently across networks. In the bipartite and director-to-director networks, distance becomes less restrictive over time relative to 1960, which suggests that director matching gradually spans a broader geographic space. In the firm-to-firm projection, by contrast, distance does not weaken and may even become more restrictive in later periods. Third, the strong border effects persist over time. Cantonal and linguistic boundaries continue to segment corporate ties even as the national market integrates and transport infrastructure improves. Importantly, the finding that highway construction has limited effects on corporate network connectivity implies that physical infrastructure alone is insufficient to overcome the cultural and institutional segmentation of corporate labor markets and inter-firm governance ties.

We contribute to two different strands of the literature. It adds to the work on interlocking directorates by showing that these networks are strongly shaped by space and internal political borders. It also contributes to the literature on border effects by extending gravity methods to organizational networks within a country. The results show that internal borders matter not only for trade, migration, or commuting, but also for corporate governance and business connectivity. Even within a small and highly integrated federal economy, institutional and cultural boundaries remain powerful determinants of economic interaction.

The chapter has limitations, but it provides compelling and coherent descriptive evidence. The municipal aggregation is helpful for computability, but it abstracts from the exact micro-level matching structure between firms and directors. The three-way gravity equations, as implemented, do not solve issues of selection into networks or sorting, for example. The highway-access analysis should also be interpreted with caution, especially given the potential for differential pre-treatment dynamics. Finally, the chapter is descriptive and does not identify the causal effects of specific border changes or institutional reforms on corporate connections. However, these limitations do not undermine the main results and patterns, which are consistent across the chapter.

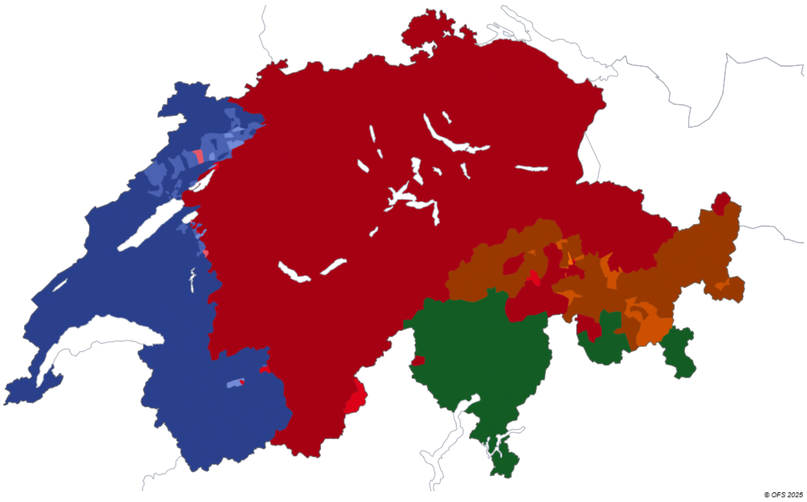
The findings of this chapter open several possibilities for future research. A natural extension would exploit quasi-experimental variation in institutional borders to identify causal effects of cantonal or linguistic boundaries on corporate network formation. Second, future work could move beyond municipal aggregation and directly model network formation at the firm or director level. Third, linking the corporate network to measures of economic performance, innovation, and productivity at the firm or municipal level would allow an assessment of the welfare consequences of interlocking directorates.

Overall, our results show that the Swiss corporate network becomes more connected over time and space, especially at the director level, but border effects are strong and persistent. Distance weakens in some layers, yet cantonal and language borders remain persistent barriers to connectivity. The chapter therefore suggests that market integration does not erase internal discontinuities.

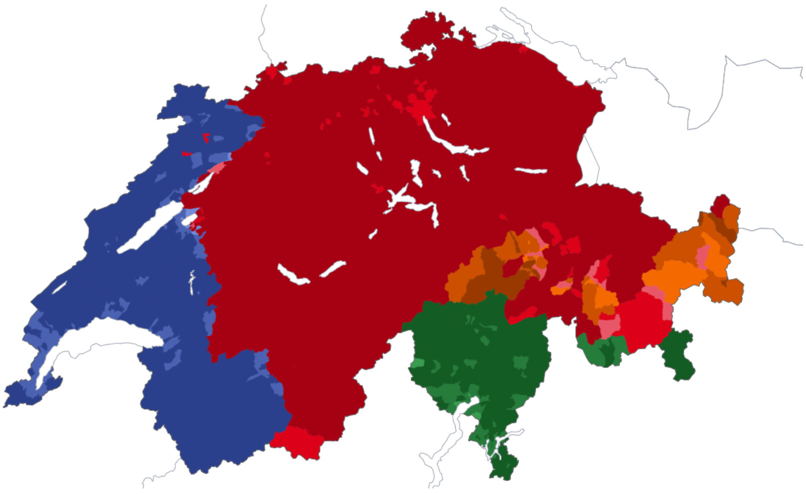
Appendices

3.A Maps

Figure 3.A.1: Share of population speaking the language of the municipalities' majority



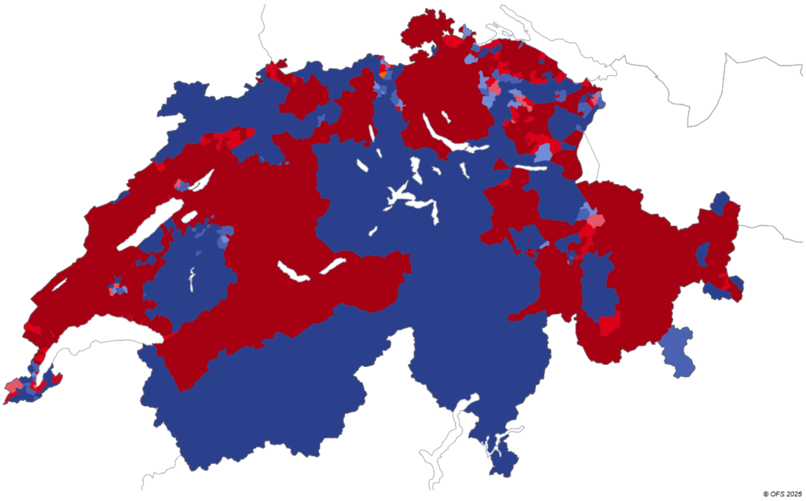
(a) Panel A: Shares in 1860



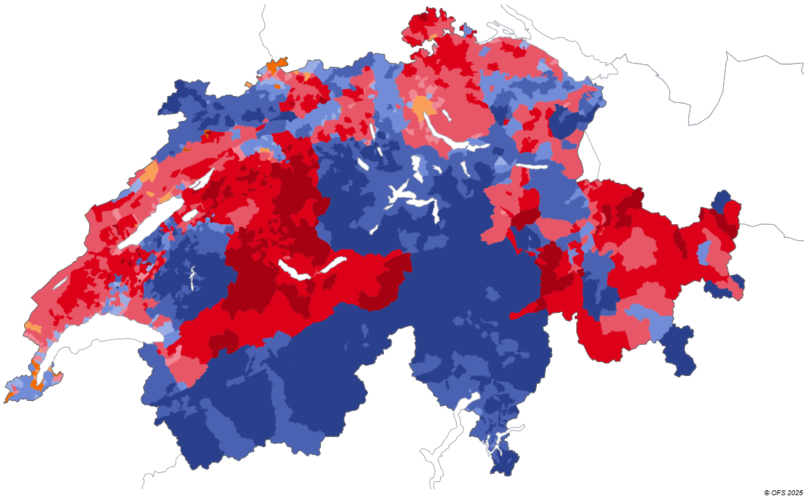
(b) Panel B: Shares in 2000

Notes: The darker the color, the higher the share of the population speaking the same language as the municipality's majority of residents. Colors indicate the majority language: dark red = German, blue = French, green = Italian, orange = Romansh. Sources: Swiss Federal Statistical Office (2025), based on Census Data (Swiss Federal Statistical Office 1860, 2000b).

Figure 3.A.2: Share of population affiliated with the religious majority



(a) Panel A: Shares in 1860

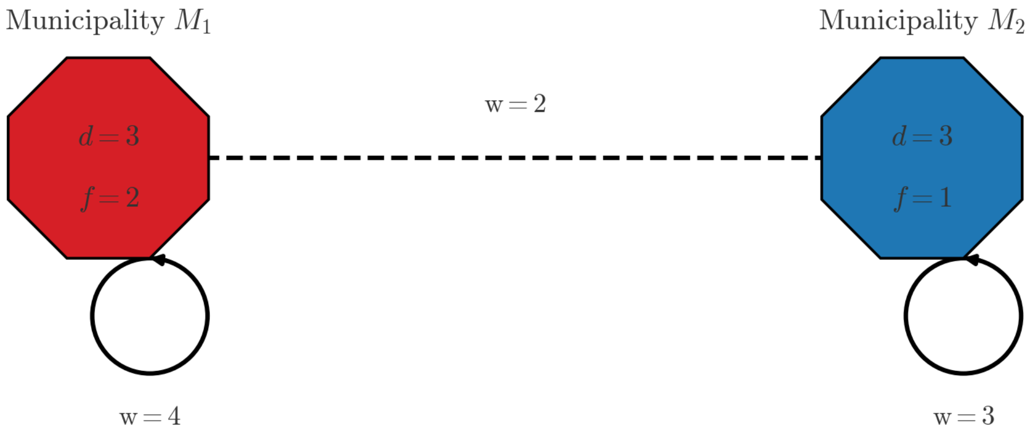


(b) Panel B: Shares in 2000

Notes: The darker the color, the higher the share of the population affiliated to the religious majority. The darkest red areas indicate municipalities with a protestant share of more than 80% of the total population. The lightest red areas are municipalities with a protestant majority but a share below 40%. The two categories in between highlight municipalities with a majority of protestants and shares ranging from 40% to 59.9% and from 60% to 79.9%. Blue areas are represented the same way and indicate the share of catholics in the total population in municipalities with a catholic majority. In Panel B, the orange areas indicate municipalities in which atheists are the majority. Similarly, the darker the orange, the higher the share of atheists living in this municipality. Sources: Swiss Federal Statistical Office (2025), based on Census Data (Swiss Federal Statistical Office 1860, 2000b).

3.B Municipal aggregation of the bipartite network

Figure 3.B.3: Illustration of the municipal aggregation of the bipartite network



Notes: Red and blue nodes are two distinct municipalities in two different cantons (or language regions). The dashed line represents connections between nodes in different cantons or language regions.

3.C Network descriptions

3.C.1 Bipartite network

Table 3.C.1: Descriptive statistics of the bipartite network

Year	# of edges	# of nodes (total)	# of firms	Avg. degree (firms)	Max degree (firms)	# of directors	Avg. degree (directors)	Max degree (directors)	Density	Connected?	# of components	Size of LCC	Share in LCC
1960	55,559	60,288	27,379	2.03	16	32,909	1.69	83	0.00	FALSE	10,551	26,285	0.44
1962	66,997	70,211	32,452	2.06	16	37,759	1.77	112	0.00	FALSE	11,403	33,302	0.47
1963	73,640	76,058	35,849	2.05	20	40,209	1.83	151	0.00	FALSE	11,940	37,329	0.49
1964	78,525	80,737	38,238	2.05	16	42,499	1.85	183	0.00	FALSE	12,512	40,096	0.50
1965	84,144	86,130	40,849	2.06	20	45,281	1.86	212	0.00	FALSE	13,213	43,124	0.50
1966	89,173	90,574	43,041	2.07	25	47,533	1.88	213	0.00	FALSE	13,636	45,965	0.51
1969	100,957	101,161	49,928	2.02	21	51,233	1.97	251	0.00	FALSE	14,556	53,546	0.53
1972	130,446	126,965	64,062	2.04	20	62,903	2.07	534	0.00	FALSE	16,408	72,774	0.57
1975	177,467	166,753	81,004	2.19	38	85,749	2.07	524	0.00	FALSE	19,585	100,922	0.61
1979	190,563	194,137	87,972	2.17	49	106,165	1.79	258	0.00	FALSE	29,099	95,999	0.49
1980	195,816	197,434	90,597	2.16	30	106,837	1.83	269	0.00	FALSE	29,429	97,468	0.49
1981	205,458	206,824	95,246	2.16	35	111,578	1.84	238	0.00	FALSE	31,120	100,908	0.49
1982	216,731	217,277	100,374	2.16	35	116,903	1.85	228	0.00	FALSE	32,542	106,197	0.49
1983	226,012	228,323	105,353	2.15	36	122,970	1.84	220	0.00	FALSE	34,926	109,542	0.48
1984	232,715	236,152	109,332	2.13	34	126,820	1.84	210	0.00	FALSE	36,649	112,083	0.47
1985	241,751	247,259	114,056	2.12	31	133,203	1.81	189	0.00	FALSE	38,969	115,763	0.47
1986	233,795	256,099	113,961	2.05	26	142,138	1.64	183	0.00	FALSE	46,372	100,191	0.39
1987	247,576	262,566	120,478	2.05	28	142,088	1.74	167	0.00	FALSE	45,429	108,411	0.41
1988	264,399	278,574	128,091	2.06	27	150,483	1.76	156	0.00	FALSE	48,188	115,226	0.41
1989	283,783	296,488	136,319	2.08	38	160,169	1.77	155	0.00	FALSE	50,606	125,335	0.42
1990	299,714	311,905	143,306	2.09	38	168,599	1.78	154	0.00	FALSE	53,047	132,204	0.42
1991	313,566	327,171	149,297	2.10	37	177,874	1.76	150	0.00	FALSE	56,284	136,338	0.42
1992	319,391	335,133	152,386	2.10	38	182,747	1.75	152	0.00	FALSE	58,764	136,195	0.41
1993	317,846	335,946	152,203	2.09	38	183,743	1.73	159	0.00	FALSE	60,044	132,364	0.39
1994	316,560	337,090	151,584	2.09	43	185,506	1.71	172	0.00	FALSE	61,235	129,156	0.38
1995	316,701	339,195	151,611	2.09	43	187,584	1.69	168	0.00	FALSE	62,493	128,049	0.38
1996	308,608	335,799	150,001	2.06	96	185,798	1.66	171	0.00	FALSE	63,871	120,947	0.36
1997	310,410	338,447	149,468	2.08	95	188,979	1.64	170	0.00	FALSE	64,609	120,243	0.36
1998	307,461	334,588	147,268	2.09	95	187,320	1.64	173	0.00	FALSE	63,533	119,331	0.36
1999	306,988	334,796	147,039	2.09	96	187,757	1.64	169	0.00	FALSE	63,936	117,325	0.35
2000	310,879	339,129	148,692	2.09	44	190,437	1.63	168	0.00	FALSE	64,767	118,158	0.35

Notes: A node is either a firm or a director; an edge represents a board membership link between a director and a firm. Avg. degree and Max degree are reported separately for firms and directors. Density is the share of realised edges over all possible edges. "Connected?" indicates whether the network forms a single connected component. The Largest Connected Component (LCC) is the largest subgraph in which all nodes are reachable from one another; "Size of LCC" denotes its number of nodes, and "Share of LCC" its fraction of the total number of nodes. The number of components counts the number of disconnected subgraphs. The data cover Swiss corporations from 1960 to 2000.

Table 3.C.2: Descriptive statistics of the bipartite network aggregated at municipal level

Year	# of edges	# of nodes	Connected?	Size of LCC	Share in LCC	Density	Avg degree	Max degree	# of components	Share of mun connected
1960	7,683	1,480	FALSE	1,420	0.96	0.01	10.38	525	59	0.59
1962	9,027	1,573	FALSE	1,517	0.96	0.01	11.48	571	54	0.63
1963	9,811	1,592	FALSE	1,541	0.97	0.01	12.33	594	50	0.64
1964	10,485	1,637	FALSE	1,583	0.97	0.01	12.81	605	51	0.66
1965	11,218	1,684	FALSE	1,636	0.97	0.01	13.32	626	47	0.67
1966	11,954	1,719	FALSE	1,670	0.97	0.01	13.91	665	49	0.69
1969	13,432	1,765	FALSE	1,727	0.98	0.01	15.22	673	38	0.71
1972	17,121	1,858	FALSE	1,823	0.98	0.01	18.43	764	35	0.74
1975	23,154	1,955	FALSE	1,930	0.99	0.01	23.69	885	26	0.78
1979	25,610	2,039	FALSE	2,023	0.99	0.01	25.12	903	15	0.82
1980	26,631	2,062	FALSE	2,045	0.99	0.01	25.83	920	17	0.83
1981	28,186	2,079	FALSE	2,060	0.99	0.01	27.11	948	19	0.83
1982	30,004	2,087	FALSE	2,072	0.99	0.01	28.75	988	15	0.84
1983	31,461	2,103	FALSE	2,088	0.99	0.01	29.92	1,017	15	0.84
1984	32,838	2,114	FALSE	2,100	0.99	0.01	31.07	1,028	14	0.85
1985	34,282	2,125	FALSE	2,112	0.99	0.01	32.27	1,036	13	0.85
1986	32,523	2,109	FALSE	2,088	0.99	0.01	30.84	1,027	18	0.85
1987	35,028	2,136	FALSE	2,112	0.99	0.01	32.80	1,061	22	0.86
1988	37,618	2,150	FALSE	2,131	0.99	0.02	34.99	1,082	19	0.86
1989	40,565	2,157	FALSE	2,145	0.99	0.02	37.61	1,121	13	0.86
1990	43,347	2,168	FALSE	2,158	1.00	0.02	39.99	1,142	11	0.87
1991	45,923	2,169	FALSE	2,161	1.00	0.02	42.34	1,158	9	0.87
1992	47,048	2,171	FALSE	2,160	0.99	0.02	43.34	1,169	12	0.87
1993	47,160	2,172	FALSE	2,163	1.00	0.02	43.43	1,165	10	0.87
1994	47,355	2,177	FALSE	2,169	1.00	0.02	43.50	1,153	9	0.87
1995	47,836	2,177	FALSE	2,171	1.00	0.02	43.95	1,145	7	0.87
1996	47,349	2,179	FALSE	2,173	1.00	0.02	43.46	1,151	7	0.87
1997	47,761	2,179	FALSE	2,175	1.00	0.02	43.84	1,149	5	0.87
1998	47,846	2,176	FALSE	2,171	1.00	0.02	43.98	1,149	5	0.87
1999	48,200	2,178	FALSE	2,173	1.00	0.02	44.26	1,158	5	0.87
2000	49,114	2,180	FALSE	2,176	1.00	0.02	45.06	1,157	4	0.87

Notes: A node represents a municipality; an edge between municipalities i and j represents the total number of director-firm links connecting firms located in i with directors residing in j (or vice versa). "Connected?" indicates whether the network forms a single connected component. The Largest Connected Component (LCC) is the largest subgraph in which all nodes are reachable from one another; "Size of LCC" denotes its number of nodes, and "Share of LCC" its fraction of the total number of nodes. Density is computed over all possible inter-municipal pairs. The number of components counts the number of disconnected subgraphs. "Share of mun connected" is the fraction of Swiss municipalities that host at least one director in a given year. The data cover Swiss corporations from 1960 to 2000.

3.C.2 Firm-to-firm network

Table 3.C.3: Descriptive statistics of the firm-to-firm network aggregated at municipal level

Year	# of edges	# of nodes	Connected?	Size of LCC	Share in LCC	Density	Avg degree	Max degree	# of components	Share of mun w/ firms
1960	7,585	998	FALSE	973	0.97	0.02	15.20	487	20	0.40
1962	9,564	1,095	FALSE	1,065	0.97	0.02	17.47	549	25	0.44
1963	10,883	1,142	FALSE	1,113	0.97	0.02	19.06	591	23	0.46
1964	11,802	1,177	FALSE	1,146	0.97	0.02	20.05	618	25	0.47
1965	12,878	1,229	FALSE	1,205	0.98	0.02	20.96	655	20	0.49
1966	13,737	1,266	FALSE	1,241	0.98	0.02	21.70	680	21	0.51
1969	15,947	1,364	FALSE	1,342	0.98	0.02	23.38	727	20	0.55
1972	20,544	1,500	FALSE	1,477	0.98	0.02	27.39	861	20	0.60
1975	28,976	1,661	FALSE	1,643	0.99	0.02	34.89	994	16	0.67
1979	27,095	1,712	FALSE	1,672	0.98	0.02	31.65	958	35	0.69
1980	29,121	1,731	FALSE	1,699	0.98	0.02	33.65	993	29	0.69
1981	31,047	1,759	FALSE	1,729	0.98	0.02	35.30	1,000	28	0.71
1982	33,422	1,806	FALSE	1,773	0.98	0.02	37.01	1,066	31	0.72
1983	34,432	1,830	FALSE	1,799	0.98	0.02	37.63	1,073	29	0.73
1984	36,105	1,859	FALSE	1,831	0.98	0.02	38.84	1,107	27	0.75
1985	37,418	1,875	FALSE	1,839	0.98	0.02	39.91	1,121	30	0.75
1986	30,909	1,792	FALSE	1,763	0.98	0.02	34.50	1,029	27	0.72
1987	35,963	1,866	FALSE	1,825	0.98	0.02	38.55	1,099	33	0.75
1988	38,727	1,907	FALSE	1,876	0.98	0.02	40.62	1,121	28	0.76
1989	42,343	1,933	FALSE	1,910	0.99	0.02	43.81	1,150	23	0.77
1990	45,353	1,971	FALSE	1,949	0.99	0.02	46.02	1,181	23	0.79
1991	48,239	1,999	FALSE	1,978	0.99	0.02	48.26	1,195	22	0.80
1992	48,644	2,022	FALSE	1,997	0.99	0.02	48.11	1,190	26	0.81
1993	47,288	2,022	FALSE	1,994	0.99	0.02	46.77	1,172	27	0.81
1994	46,672	2,024	FALSE	2,002	0.99	0.02	46.12	1,178	21	0.81
1995	46,401	2,021	FALSE	1,997	0.99	0.02	45.92	1,160	24	0.81
1996	44,799	2,002	FALSE	1,975	0.99	0.02	44.75	1,161	27	0.80
1997	43,755	2,006	FALSE	1,975	0.98	0.02	43.62	1,140	32	0.80
1998	43,205	1,999	FALSE	1,968	0.98	0.02	43.23	1,120	32	0.80
1999	42,919	1,996	FALSE	1,968	0.99	0.02	43.01	1,131	28	0.80
2000	43,248	2,006	FALSE	1,976	0.99	0.02	43.12	1,162	29	0.80

Notes: A node represents a municipality; an edge between municipalities i and j is weighted by the number of firm pairs sharing at least one common board member, where one firm is located in i and the other in j. "Connected?" indicates whether the network forms a single connected component. The Largest Connected Component (LCC) is the largest subgraph in which all nodes are reachable from one another; "Size of LCC" denotes its number of nodes, and "Share of LCC" its fraction of the total number of nodes. Density is computed over all possible inter-municipal pairs. The number of components counts the number of disconnected subgraphs. "Share of mun w/ firms" is the fraction of Swiss municipalities hosting at least one firm in a given year. The data cover Swiss corporations from 1960 to 2000.

3.C.3 Director-to-director network

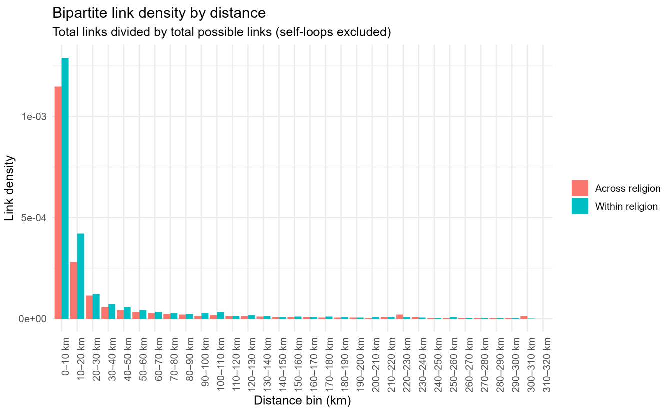
Table 3.C.4: Descriptive statistics of the director-to-director network aggregated at municipal level

Year	# of edges	# of nodes	Connected?	Size of LCC	Share in LCC	Density	Avg degree	Max degree	# of components	Share of mun w/ directors
1960	7,237	1,268	FALSE	1,189	0.94	0.01	11.41	438	73	0.51
1962	8,867	1,383	FALSE	1,311	0.95	0.01	12.82	501	66	0.55
1963	9,777	1,401	FALSE	1,340	0.96	0.01	13.96	524	56	0.56
1964	10,482	1,436	FALSE	1,370	0.95	0.01	14.60	546	61	0.58
1965	11,406	1,482	FALSE	1,426	0.96	0.01	15.39	574	54	0.59
1966	12,372	1,531	FALSE	1,477	0.96	0.01	16.16	594	54	0.61
1969	13,829	1,452	FALSE	1,397	0.96	0.01	19.05	576	54	0.58
1972	18,881	1,548	FALSE	1,499	0.97	0.02	24.39	683	49	0.62
1975	32,760	1,690	FALSE	1,664	0.98	0.02	38.77	876	27	0.68
1979	35,010	1,770	FALSE	1,743	0.98	0.02	39.56	849	28	0.71
1980	36,475	1,784	FALSE	1,759	0.99	0.02	40.89	835	26	0.72
1981	38,298	1,798	FALSE	1,770	0.98	0.02	42.60	843	29	0.72
1982	40,604	1,816	FALSE	1,794	0.99	0.02	44.72	900	23	0.73
1983	42,350	1,829	FALSE	1,805	0.99	0.03	46.31	893	24	0.73
1984	43,821	1,849	FALSE	1,830	0.99	0.03	47.40	897	20	0.74
1985	45,674	1,883	FALSE	1,865	0.99	0.03	48.51	928	18	0.75
1986	37,936	1,872	FALSE	1,838	0.98	0.02	40.53	894	31	0.75
1987	41,741	2,000	FALSE	1,971	0.99	0.02	41.74	947	27	0.80
1988	45,842	1,956	FALSE	1,932	0.99	0.02	46.87	982	25	0.78
1989	50,887	2,000	FALSE	1,982	0.99	0.03	50.89	1,040	19	0.80
1990	54,893	2,012	FALSE	2,000	0.99	0.03	54.57	1,069	13	0.81
1991	58,799	2,014	FALSE	2,002	0.99	0.03	58.39	1,105	13	0.81
1992	60,146	2,020	FALSE	2,013	1.00	0.03	59.55	1,102	8	0.81
1993	60,316	2,023	FALSE	2,013	1.00	0.03	59.63	1,100	10	0.81
1994	60,290	2,022	FALSE	2,012	1.00	0.03	59.63	1,088	11	0.81
1995	61,704	2,015	FALSE	2,007	1.00	0.03	61.24	1,084	9	0.81
1996	59,888	2,015	FALSE	2,001	0.99	0.03	59.44	1,075	15	0.81
1997	61,117	2,013	FALSE	2,002	0.99	0.03	60.72	1,103	12	0.81
1998	61,633	2,026	FALSE	2,016	1.00	0.03	60.84	1,125	11	0.81
1999	61,833	2,025	FALSE	2,016	1.00	0.03	61.07	1,129	10	0.81
2000	62,872	2,025	FALSE	2,017	1.00	0.03	62.10	1,143	9	0.81

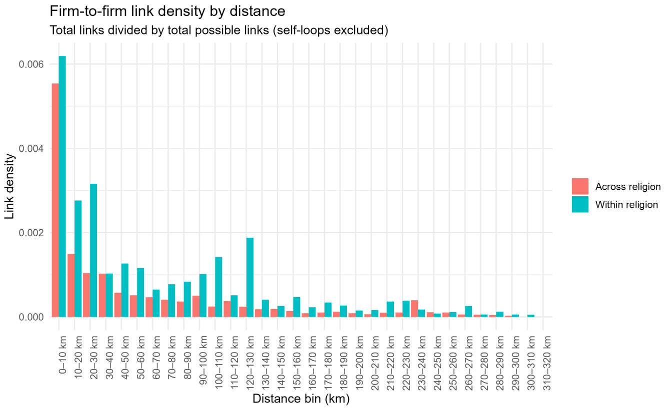
Notes: A node represents a municipality; an edge between municipalities i and j is weighted by the number of director pairs co-sitting on at least one common board, where one director resides in i and the other in j. "Connected?" indicates whether the network forms a single connected component. The Largest Connected Component (LCC) is the largest subgraph in which all nodes are reachable from one another; "Size of LCC" denotes its number of nodes, and "Share of LCC" its fraction of the total number of nodes. Density is computed over all possible inter-municipal pairs. The number of components counts the number of disconnected subgraphs. "Share of mun w/ directors" is the fraction of Swiss municipalities hosting at least one director in a given year. The data cover Swiss corporations from 1960 to 2000.

3.C.4 Stylized facts for religious borders

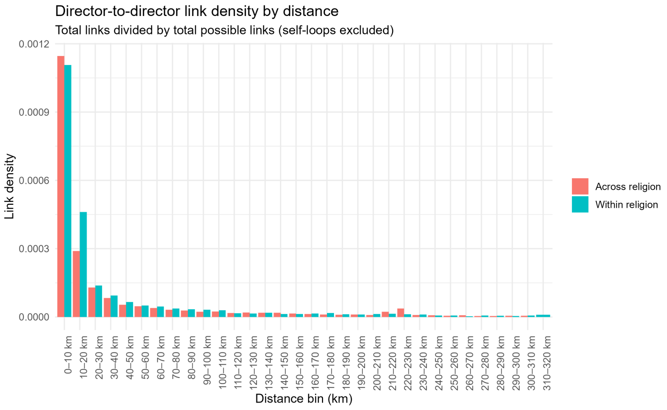
Figure 3.C.4: Density of connections - within vs. across religions



(a) Panel A: Bipartite Network



(b) Panel B: Firm-to-Firm Network



(c) Panel C: Director-to-director Network

Notes: Bars show link density by distance bin (observed links over possible links; self-loops excluded from the 0-10km bin), split into within-border and across-border dyads. Distances are between municipal centroids.

3.C.5 Network analysis

We complement our descriptive statistics with standard metrics from the network science literature. Network structure can be used to analyze and describe which municipalities are connected to which others, depending on their characteristics. It abstracts from spatial concerns, but can provide supporting evidence that cantonal, language, and religion borders matter. Similar to Figures 3.7-3.9, the metrics in this section consider only one border, and not all simultaneously. However, we can calculate the metrics for each year and plot their evolution.

E-I Index

The E-I index is an intuitive measure of in- and out-group connectivity (Krackhardt and Stern 1988). To obtain the index, we simply subtract the number of out-group ties from the number of in-group ties, divided by the total number of ties.

$$E - I = \frac{AcrossCanton - WithinCanton}{AcrossCanton + WithinCanton} \tag{3.10}$$

The index ranges from -1 to 1, with -1 indicating that all links are within cantonal borders and none cross them. The opposite case, in which all links cross cantonal borders, gives a score of 1. An index of 0 means there are as many links across borders as within borders.

Figures 3.C.5 to 3.C.7 in the Appendix plot the evolution of the E–I index computed on the three municipal-level networks. In all three networks, the index is negative throughout the years, indicating that interlocking directorates are predominantly formed within cantonal, language or religious borders rather than across them. The dynamics in the E-I index in the bipartite network are not very telling. Across the three networks, the cantonal borders are the most frequently crossed, and their importance remain stable over time. The E-I index using the language borders shows large negative values. Even though the values remain stable, the language divide seems to be more pronounced in recent years. The religious borders appear to be quite important when measured by the index, however, they are the only ones with values getting closer to 0 over time (except in the firm-to-firm network).

Assortativity

The patters observed using the E-I index are somewhat confirmed using an assortativity measure. Selection into connections based on characteristics is known as assortative mixing, or homophily (Newman 2002, 2003). We measure assortativity using the coefficient proposed by Newman (2003). It measures the fraction of edges connecting municipalities within the same group, minus the expected fraction in a uniformly random graph with the same degree sequence. Consider an undirected network of connected municipalities $\mathcal{M}$ in which each node belongs to a set T. The coefficient is defined as:

$$r = \frac{\sum_{t \in T} x_{tt} - \sum_{t \in T} y_t^2}{1 - \sum_{t \in T} y_t^2}, \tag{3.11}$$

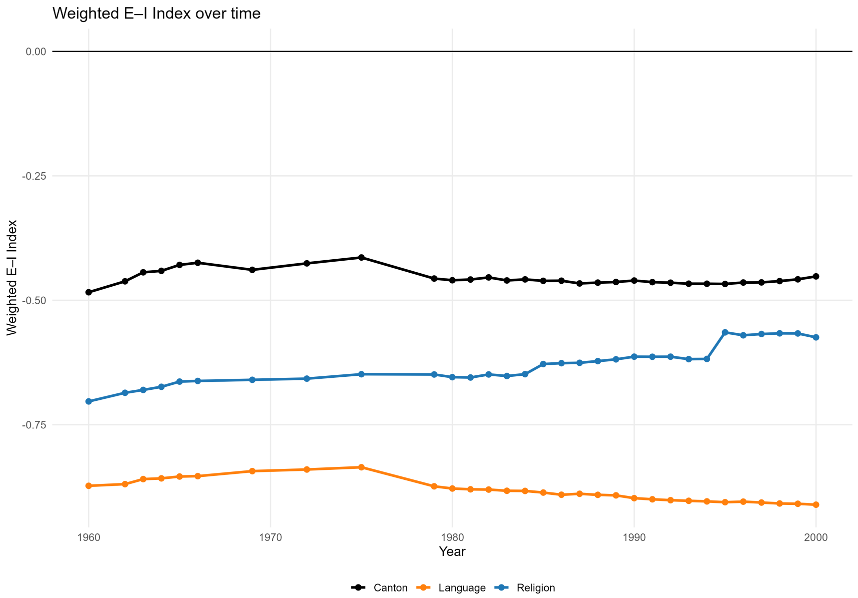
With $y_t = \sum_{s \in T} x_{st}$, where x_{st} is the share of links between nodes of the different types. The resulting r ranges from -1 to 1, with larger values indicating greater assortative mixing of the network. If $r > 0$, $\mathcal{M}$ is positively sorted, and negatively sorted if $r < 0$. Nodes in the positively sorted network tend to have links with nodes of the same type, while nodes in the negatively sorted network tend to have neighbors with a different type.

Figures 3.C.8 to 3.C.10 display assortativity coefficients computed with respect to cantonal, language, and religious borders in the bipartite, firm-to-firm, and director-to-director networks. Unsurprisingly, all values of r are positive: nodes tend to connect with other nodes with similar characteristics. In our context, this means that connections are within borders. Across all three networks, cantonal and language borders generate substantially higher assortativity than religious borders.

In terms of dynamics, three trends stand out. First, assortativity by religion tends to weaken steadily (but slowly), indicating a gradual erosion of confessional and cantonal clustering in corporate ties. Second, assortativity by language decreases in the 1970s, then rises again in later decades. Finally, assortativity by canton is much more stable, although the values are systematically lower in later years than in earlier years. Overall, the assortativity figures reinforce the conclusion that internal administrative and linguistic borders remain first-order structuring forces in the Swiss corporate network.

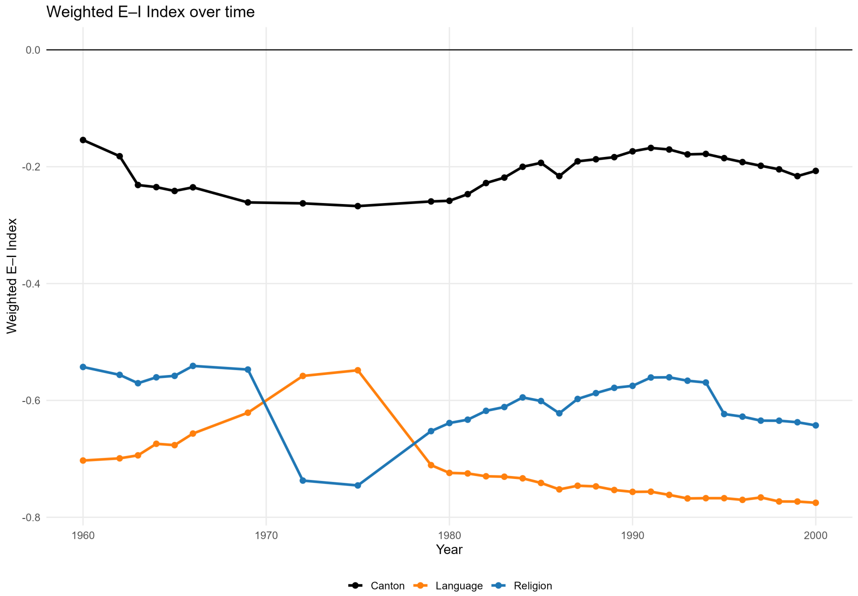
E-I index figures

Figure 3.C.5: Evolution of the E-I index in the bipartite network



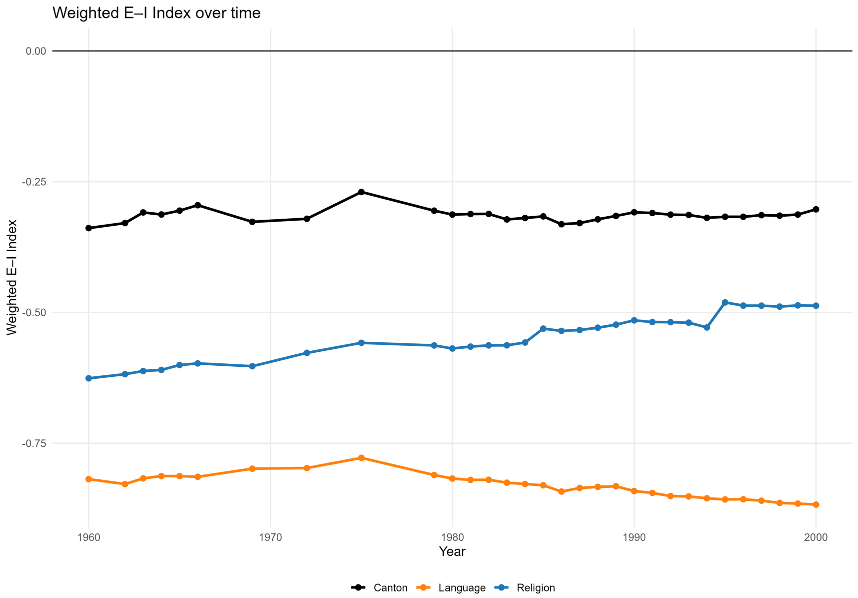
Notes: E-I index computed on the municipal-level bipartite network, where nodes are municipalities and weighted links measure director–firm connections between municipality pairs. The index ranges from -1 (all links within borders) to 1 (all links across borders).

Figure 3.C.6: Evolution of the E-I index in the firm-to-firm network



Notes: E-I index computed on the municipal-level firm-to-firm projection, where weighted links measure the number of interlocking directorate ties between firms located in two municipalities. The index ranges from -1 (all links within borders) to 1 (all links across borders).

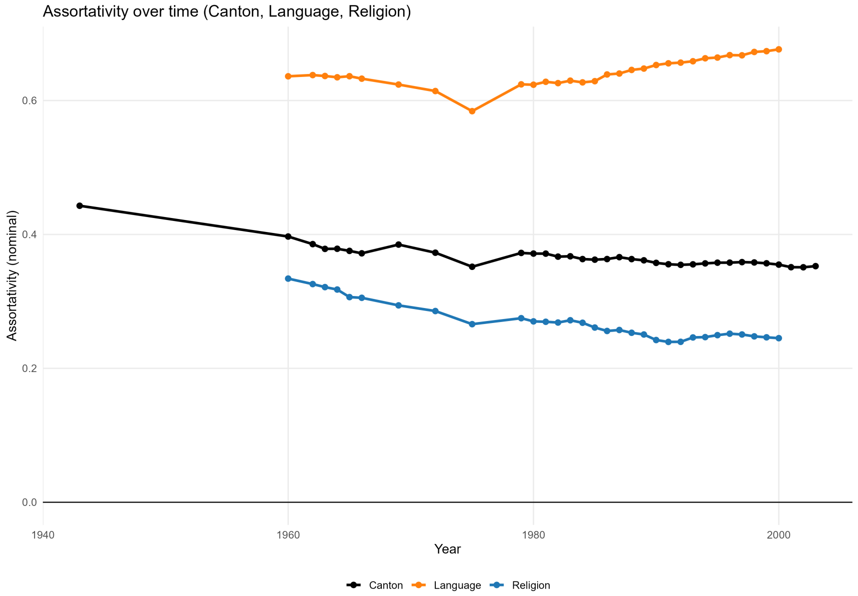
Figure 3.C.7: Evolution of the E-I index in the director-to-director network



Notes: E-I index computed on the municipal-level director-to-director projection, where weighted links measure the number of persons with mandates on the same board of directors between municipalities. The index ranges from -1 (all links within borders) to 1 (all links across borders).

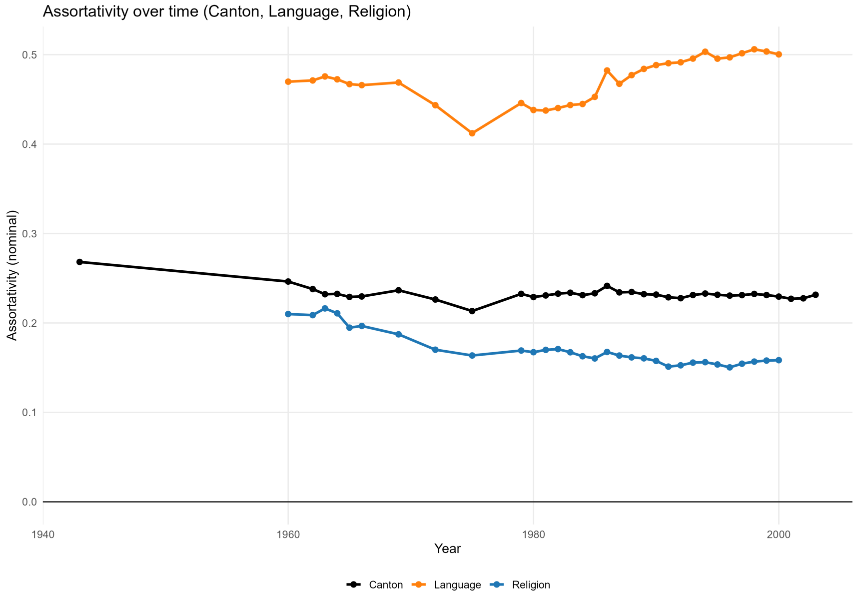
Assortativity figures

Figure 3.C.8: Evolution of Assortativity in the bipartite network



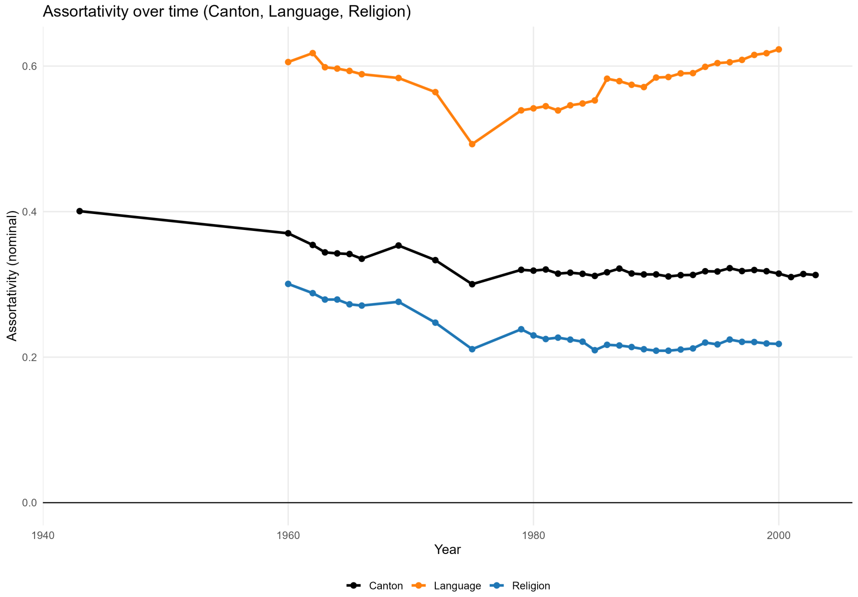
Notes: Assortativity coefficient (Newman 2003) computed on the municipal-level bipartite network by grouping municipalities by border category. Values range from -1 (disassortative mixing) to 1 (assortative mixing).

Figure 3.C.9: Evolution of Assortativity in the firm-to-firm network



Notes: Assortativity coefficient (Newman 2003) computed on the municipal-level firm-to-firm projection by grouping municipalities by border category. Values range from -1 (disassortative mixing) to 1 (assortative mixing).

Figure 3.C.10: Evolution of Assortativity in the director-to-director network



Notes: Assortativity coefficient (Newman 2003) computed on the municipal-level director-to-director projection by grouping municipalities by border category. Values range from -1 (disassortative mixing) to 1 (assortative mixing).

3.D Results

3.D.1 Two-way gravity estimations

Table 3.D.5: Two-way gravity estimates (separate cross-sections - Bipartite)

Period	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Year	1960	1963	1966	1969	1972	1975	1979	1982	1985	1988	1991	1994	1997	2000
Log flight distance (km)	-0.705***	-0.675***	-0.673***	-0.651***	-0.642***	-0.666***	-0.725***	-0.738***	-0.754***	-0.766***	-0.782***	-0.798***	-0.805***	-0.800***
	(0.085)	(0.082)	(0.079)	(0.083)	(0.079)	(0.075)	(0.067)	(0.066)	(0.064)	(0.062)	(0.054)	(0.051)	(0.049)	(0.049)
Cantonal borders	-1.508***	-1.479***	-1.539***	-1.763***	-1.772***	-1.763***	-1.831***	-1.890***	-1.936***	-1.963***	-1.955***	-1.931***	-1.938***	-1.902***
	(0.345)	(0.317)	(0.299)	(0.274)	(0.265)	(0.256)	(0.244)	(0.234)	(0.218)	(0.207)	(0.186)	(0.182)	(0.198)	(0.203)
Language borders	-1.553***	-1.629***	-1.672***	-1.592***	-1.667***	-1.576***	-1.513***	-1.479***	-1.460***	-1.479***	-1.572***	-1.592***	-1.600***	-1.635***
	(0.148)	(0.195)	(0.189)	(0.211)	(0.196)	(0.171)	(0.144)	(0.136)	(0.129)	(0.142)	(0.121)	(0.114)	(0.108)	(0.107)
Religious borders	-0.099	-0.167*	-0.155*	-0.226***	-0.288**	-0.267**	-0.170**	-0.170**	-0.125*	-0.018	0.005	-0.019	0.063	0.016
	(0.129)	(0.094)	(0.091)	(0.079)	(0.113)	(0.108)	(0.077)	(0.068)	(0.071)	(0.080)	(0.072)	(0.072)	(0.124)	(0.126)
# Observations	1,768,750	2,068,560	2,473,648	2,255,920	2,540,552	3,038,548	3,267,900	3,441,012	3,661,048	3,902,440	4,062,052	4,113,420	4,076,600	4,098,776
Pseudo R^2	0.876	0.869	0.864	0.860	0.856	0.852	0.846	0.841	0.837	0.834	0.831	0.828	0.825	0.822

Notes: Estimates from PQML regressions of bipartite link counts between municipality pairs on log flight distance and border variables, estimated separately for each of the 14 cross-sections. Each cross-section includes fixed effects for the origin and destination. Robust standard errors, clustered by origin and destination municipality, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.D.6: Two-way gravity results - All periods (Bipartite network)

	(1)	(2)	(3)
Ln(flight dist. - km)	-1.399***	-1.361***	-1.356***
	(0.021)	(0.022)	(0.022)
Cantonal borders	-1.257***	-1.223***	-1.189***
	(0.051)	(0.050)	(0.052)
Language borders		-1.105***	-1.104***
		(0.067)	(0.067)
Religious borders			-0.113***
			(0.026)
# Observations	47,474,748	47,474,748	47,474,748
Origin x year FE	Yes	Yes	Yes
Destination x year FE	Yes	Yes	Yes

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of the weighted number of director-to-firm links between municipality pairs on log distance and border variables. All specifications include municipality-year fixed effects. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.D.7: Two-way gravity estimates (separate cross-sections - Firm-to-firm)

Period	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Year	1960	1963	1966	1969	1972	1975	1979	1982	1985	1988	1991	1994	1997	2000
Log flight distance (km)	-0.339***	-0.370***	-0.363***	-0.358***	-0.373***	-0.379***	-0.348***	-0.351***	-0.357***	-0.365***	-0.401***	-0.424***	-0.448***	-0.468***
	(0.043)	(0.058)	(0.063)	(0.072)	(0.083)	(0.084)	(0.055)	(0.050)	(0.050)	(0.052)	(0.052)	(0.051)	(0.049)	(0.047)
Cantonal borders	-1.182***	-0.888**	-0.981**	-1.041*	-0.653	-0.689	-1.134***	-1.212***	-1.293***	-1.387***	-1.415***	-1.400***	-1.361***	-1.306***
	(0.270)	(0.355)	(0.400)	(0.535)	(0.428)	(0.425)	(0.287)	(0.243)	(0.245)	(0.265)	(0.276)	(0.280)	(0.273)	(0.269)
Language borders	-1.697***	-1.390***	-1.211***	-1.147***	-0.971***	-0.864***	-1.047***	-1.040***	-0.989***	-1.007***	-0.999***	-0.958***	-0.915***	-0.891***
	(0.185)	(0.201)	(0.223)	(0.281)	(0.216)	(0.218)	(0.140)	(0.138)	(0.134)	(0.142)	(0.145)	(0.144)	(0.139)	(0.135)
Religious borders	0.013	0.029	-0.019	-0.030	-0.053	-0.030	-0.006	0.011	0.000	-0.009	-0.010	-0.016	-0.021	-0.021
	(0.060)	(0.073)	(0.090)	(0.098)	(0.086)	(0.083)	(0.052)	(0.047)	(0.047)	(0.048)	(0.047)	(0.046)	(0.044)	(0.043)
# Observations	720,630	731,456	744,102	756,883	769,921	783,144	804,552	818,003	833,229	848,991	864,330	879,112	892,774	905,881
Pseudo R^2	0.922	0.921	0.919	0.918	0.917	0.916	0.918	0.919	0.920	0.921	0.922	0.923	0.924	0.925

Notes: Estimates from PQML regressions of firm-to-firm link counts between municipality pairs on log flight distance and border variables, estimated separately for each of the 14 cross-sections. Each cross-section includes fixed effects for the origin and destination. Robust standard errors, clustered by origin and destination municipality, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.D.8: Two-way gravity results - All periods (Firm-to-firm network)

	(1)	(2)	(3)
Log flight distance (km)	-0.792***	-0.684***	-0.677***
	(0.065)	(0.066)	(0.066)
Cantonal borders	-0.837***	-0.903***	-0.823***
	(0.262)	(0.255)	(0.258)
Language borders		-1.068***	-1.064***
		(0.260)	(0.262)
Religious borders			-0.288***
			(0.100)
# Observations	30,668,619	30,668,619	30,668,619
Origin x year FE	Yes	Yes	Yes
Destination x year FE	Yes	Yes	Yes

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of the weighted number of firm-to-firm links between municipality pairs on log distance and border variables. All specifications include municipality-year fixed effects. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.D.9: Two-way gravity estimates (separate cross-sections - Director-to-director)

Period	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Year	1960	1963	1966	1969	1972	1975	1979	1982	1985	1988	1991	1994	1997	2000
Log flight distance (km)	-0.580***	-0.566***	-0.563***	-0.556***	-0.561***	-0.570***	-0.620***	-0.648***	-0.666***	-0.680***	-0.686***	-0.707***	-0.719***	-0.723***
	(0.026)	(0.025)	(0.022)	(0.019)	(0.018)	(0.017)	(0.015)	(0.013)	(0.012)	(0.012)	(0.011)	(0.010)	(0.009)	(0.009)
Cantonal borders	-1.607***	-1.593***	-1.650***	-1.831***	-1.884***	-1.870***	-1.865***	-1.886***	-1.907***	-1.895***	-1.876***	-1.867***	-1.857***	-1.811***
	(0.112)	(0.099)	(0.087)	(0.079)	(0.067)	(0.058)	(0.052)	(0.046)	(0.043)	(0.041)	(0.036)	(0.034)	(0.037)	(0.036)
Language borders	-1.522***	-1.607***	-1.609***	-1.363***	-1.380***	-1.214***	-1.295***	-1.223***	-1.232***	-1.243***	-1.337***	-1.391***	-1.387***	-1.405***
	(0.103)	(0.100)	(0.083)	(0.087)	(0.069)	(0.054)	(0.056)	(0.050)	(0.047)	(0.047)	(0.040)	(0.044)	(0.039)	(0.042)
Religious borders	-0.117	-0.196***	-0.205***	-0.196***	-0.148***	-0.170***	-0.179***	-0.163***	-0.092***	-0.016	-0.019	-0.056**	0.007	-0.016
	(0.080)	(0.067)	(0.056)	(0.064)	(0.054)	(0.041)	(0.037)	(0.031)	(0.031)	(0.031)	(0.028)	(0.026)	(0.033)	(0.032)
# Observations	1,299,675	1,608,276	1,957,240	1,800,050	2,064,846	2,512,754	2,789,970	2,952,822	3,175,124	3,414,575	3,643,957	3,693,984	3,669,068	3,699,696
Pseudo R^2	0.833	0.826	0.820	0.814	0.805	0.793	0.788	0.779	0.772	0.770	0.763	0.756	0.746	0.739

Notes: Estimates from PQML regressions of director-to-director link counts between municipality pairs on log flight distance and border variables, estimated separately for each of the 14 cross-sections. Each cross-section includes fixed effects for the origin and destination. Robust standard errors, clustered by origin and destination municipality, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3.D.10: Two-way gravity results - All periods (Director-to-director network)

	(1)	(2)	(3)
Ln(flight dist. - km)	-1.209***	-1.173***	-1.167***
	(0.018)	(0.018)	(0.018)
Cantonal borders	-1.383***	-1.327***	-1.288***
	(0.041)	(0.040)	(0.042)
Language borders		-0.954***	-0.956***
		(0.041)	(0.041)
Religious borders			-0.122***
			(0.018)
# Observations	38,282,037	38,282,037	38,282,037
Origin x year FE	Yes	Yes	Yes
Destination x year FE	Yes	Yes	Yes

Notes: This table reports the coefficient estimates from panel gravity PQML regressions of the weighted number of director-to-director links between municipality pairs on log distance and border variables. All specifications include municipality-year fixed effects. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

3.D.2 Three-way gravity estimations

Distances

Table 3.D.11: Distances, in all three networks

	Bipartite (1)		Firm-to-Firm (2)		Director-to-Director (3)	
1 × log Dist	0.000	(.)	0.000	(.)	0.000	(.)
2 × log Dist	0.014**	(0.007)	-0.005	(0.020)	0.020**	(0.008)
3 × log Dist	0.018**	(0.009)	-0.021	(0.023)	0.030***	(0.010)
4 × log Dist	0.070***	(0.017)	-0.030	(0.029)	0.049***	(0.012)
5 × log Dist	0.076***	(0.016)	-0.041	(0.032)	0.067***	(0.013)
6 × log Dist	0.084***	(0.014)	-0.022	(0.033)	0.119***	(0.013)
7 × log Dist	0.061***	(0.012)	-0.016	(0.023)	0.091***	(0.014)
8 × log Dist	0.073***	(0.012)	-0.019	(0.023)	0.082***	(0.014)
9 × log Dist	0.079***	(0.012)	-0.027	(0.023)	0.080***	(0.015)
10 × log Dist	0.076***	(0.012)	-0.016	(0.023)	0.051***	(0.014)
11 × log Dist	0.091***	(0.012)	-0.016	(0.022)	0.077***	(0.015)
12 × log Dist	0.088***	(0.012)	-0.026	(0.022)	0.072***	(0.015)
13 × log Dist	0.097***	(0.013)	-0.046**	(0.022)	0.074***	(0.015)
14 × log Dist	0.107***	(0.013)	-0.061***	(0.022)	0.079***	(0.015)
Observations	1,337,379		1,305,738		1,833,416	
Pseudo R^2	0.900		0.978		0.830	

Notes: This table reports the three-way gravity PQML estimates of Equation (3.2) for the distance friction, across all three networks. Period 1 (1960) is the reference period. Coefficients measure the change in the distance effect relative to 1960, controlling for municipality-pair and municipality-year fixed effects. Columns (1), (2), and (3) report estimates for the bipartite, firm-to-firm, and director-to-director networks, respectively. These results underlie Panel A of Figures 3.13-3.15. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Cantonal borders

Table 3.D.12: Cantonal borders, in all three networks

	Bipartite (1)		Firm-to-Firm (2)		Director-to-Director (3)	
1 × CtnBorder	0.000	(.)	0.000	(.)	0.000	(.)
2 × CtnBorder	0.112***	(0.034)	0.091	(0.084)	0.063	(0.040)
3 × CtnBorder	0.073*	(0.040)	0.040	(0.097)	0.003	(0.045)
4 × CtnBorder	-0.137**	(0.068)	0.034	(0.128)	-0.096	(0.060)
5 × CtnBorder	-0.133**	(0.067)	0.116	(0.157)	-0.159***	(0.058)
6 × CtnBorder	-0.142**	(0.062)	0.084	(0.159)	-0.183***	(0.053)
7 × CtnBorder	-0.174***	(0.056)	0.043	(0.132)	-0.202***	(0.048)
8 × CtnBorder	-0.241***	(0.058)	-0.090	(0.122)	-0.199***	(0.044)
9 × CtnBorder	-0.269***	(0.058)	-0.162	(0.123)	-0.205***	(0.046)
10 × CtnBorder	-0.261***	(0.057)	-0.062	(0.134)	-0.177***	(0.044)
11 × CtnBorder	-0.284***	(0.059)	-0.175	(0.134)	-0.208***	(0.045)
12 × CtnBorder	-0.273***	(0.059)	-0.169	(0.136)	-0.207***	(0.046)
13 × CtnBorder	-0.265***	(0.060)	-0.302**	(0.136)	-0.190***	(0.047)
14 × CtnBorder	-0.236***	(0.062)	-0.278**	(0.136)	-0.192***	(0.046)
Observations	1,337,379		1,305,738		1,833,416	
Pseudo R^2	0.900		0.978		0.830	

Notes: This table reports the three-way gravity PQML estimates of Equation (3.2) for the cantonal border friction, across all three networks. Period 1 (1960) is the reference period. Coefficients measure the change in the cantonal border effect relative to 1960, controlling for municipality-pair and municipality-year fixed effects. Columns (1), (2), and (3) report estimates for the bipartite, firm-to-firm, and director-to-director networks, respectively. These results underlie Panel A of Figures 3.13-3.15. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Language borders

Table 3.D.13: Language borders, in all three networks

	Bipartite (1)		Firm-to-Firm (2)		Director-to-Director (3)	
1 × LangBorder	0.000	(.)	0.000	(.)	0.000	(.)
2 × LangBorder	0.018	(0.054)	-0.000	(0.046)	-0.037	(0.037)
3 × LangBorder	-0.003	(0.056)	-0.072	(0.050)	-0.055	(0.041)
4 × LangBorder	0.066	(0.072)	-0.009	(0.058)	0.104**	(0.047)
5 × LangBorder	0.015	(0.068)	0.042	(0.059)	0.012	(0.044)
6 × LangBorder	0.059	(0.062)	0.103*	(0.059)	0.040	(0.042)
7 × LangBorder	0.009	(0.054)	0.060	(0.052)	0.035	(0.036)
8 × LangBorder	-0.014	(0.051)	0.000	(0.051)	-0.023	(0.033)
9 × LangBorder	-0.002	(0.052)	-0.039	(0.051)	-0.034	(0.032)
10 × LangBorder	-0.057	(0.052)	-0.070	(0.051)	-0.073**	(0.032)
11 × LangBorder	-0.113**	(0.052)	-0.132**	(0.051)	-0.078**	(0.032)
12 × LangBorder	-0.175***	(0.053)	-0.194***	(0.052)	-0.122***	(0.033)
13 × LangBorder	-0.175***	(0.053)	-0.201***	(0.052)	-0.145***	(0.033)
14 × LangBorder	-0.226***	(0.055)	-0.236***	(0.053)	-0.120***	(0.034)
Observations	1,337,379		1,305,738		1,833,416	
Pseudo R^2	0.900		0.978		0.830	

Notes: This table reports the three-way gravity PQML estimates of Equation (3.2) for the language border friction, across all three networks. Period 1 (1960) is the reference period. Coefficients measure the change in the language border effect relative to 1960, controlling for municipality-pair and municipality-year fixed effects. Columns (1), (2), and (3) report estimates for the bipartite, firm-to-firm, and director-to-director networks, respectively. These results underlie Panel B of Figures 3.13-3.15. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Religion borders

Table 3.D.14: Religion borders, in all three networks

	Bipartite		Firm-to-Firm		Director-to-Director	
	(1)		(2)		(3)	
1 × RelBorder	0.000	(.)	0.000	(.)	0.000	(.)
2 × RelBorder	-0.076**	(0.031)	-0.023	(0.077)	-0.087***	(0.032)
3 × RelBorder	-0.040	(0.032)	-0.056	(0.082)	-0.064**	(0.033)
4 × RelBorder	-0.076**	(0.037)	-0.087	(0.105)	-0.102***	(0.037)
5 × RelBorder	-0.084**	(0.035)	-0.148	(0.114)	-0.116***	(0.035)
6 × RelBorder	-0.079***	(0.029)	-0.124	(0.103)	-0.085***	(0.030)
7 × RelBorder	-0.080***	(0.018)	-0.074	(0.072)	-0.064***	(0.022)
8 × RelBorder	-0.051***	(0.015)	-0.014	(0.060)	-0.048***	(0.020)
9 × RelBorder	-0.027**	(0.013)	0.011	(0.057)	-0.026	(0.019)
10 × RelBorder	0.000	(0.013)	0.026	(0.058)	-0.000	(0.019)
11 × RelBorder	0.010	(0.013)	0.018	(0.057)	0.009	(0.019)
12 × RelBorder	0.012	(0.013)	-0.037	(0.057)	0.002	(0.019)
13 × RelBorder	0.010	(0.014)	-0.015	(0.057)	0.003	(0.019)
14 × RelBorder	0.016	(0.014)	0.020	(0.058)	0.002	(0.019)
Observations	1,337,379		1,305,738		1,833,416	
Pseudo R^2	0.900		0.978		0.830	

Notes: This table reports the three-way gravity PQML estimates of Equation (3.2) for the religion border friction, across all three networks. Period 1 (1960) is the reference period. Coefficients measure the change in the religion border effect relative to 1960, controlling for municipality-pair and municipality-year fixed effects. Columns (1), (2), and (3) report estimates for the bipartite, firm-to-firm, and director-to-director networks, respectively. These results underlie Panel B of Figures 3.13-3.15. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

3.D.3 Highways

Bipartite aggregated network

Table 3.D.15: Three-way gravity results - Highway access (Bipartite network)

	(1)		(2)		(3)		(4)		(5)		(6)	
$Access_{ij,t}$	0.035	(0.024)	0.011	(0.021)	0.011	(0.017)	0.041*	(0.023)	0.022	(0.020)	0.003	(0.017)
$Access_{ij,t-1}$			0.039**	(0.018)	0.039**	(0.018)			0.032*	(0.018)	0.034*	(0.018)
$Access_{ij,t-2}$			0.006	(0.016)	0.006	(0.016)			0.002	(0.017)	0.004	(0.017)
$Access_{ij,t-3}$			0.017	(0.016)	0.017	(0.016)			0.019	(0.015)	0.021	(0.015)
$Access_{ij,t+1}$					-0.000	(0.022)					0.041*	(0.022)
Total *Access*			0.072	(0.041)	0.073	(0.041)			0.074	(0.040)	0.061	(0.040)
1 × log Dist							0.000	(.)	0.000	(.)	0.000	(.)
2 × log Dist							0.012*	(0.007)	0.012*	(0.007)	0.013*	(0.007)
3 × log Dist							0.016*	(0.009)	0.016*	(0.009)	0.017*	(0.009)
4 × log Dist							0.069***	(0.017)	0.067***	(0.017)	0.068***	(0.017)
5 × log Dist							0.075***	(0.016)	0.073***	(0.016)	0.074***	(0.016)
6 × log Dist							0.083***	(0.014)	0.081***	(0.014)	0.082***	(0.014)
7 × log Dist							0.060***	(0.012)	0.059***	(0.012)	0.059***	(0.012)
8 × log Dist							0.073***	(0.012)	0.071***	(0.012)	0.072***	(0.012)
9 × log Dist							0.079***	(0.012)	0.077***	(0.012)	0.078***	(0.012)
10 × log Dist							0.075***	(0.012)	0.074***	(0.012)	0.075***	(0.012)
11 × log Dist							0.090***	(0.012)	0.089***	(0.012)	0.090***	(0.012)
12 × log Dist							0.087***	(0.012)	0.086***	(0.012)	0.087***	(0.012)
13 × log Dist							0.096***	(0.013)	0.095***	(0.013)	0.096***	(0.013)
14 × log Dist							0.107***	(0.013)	0.106***	(0.013)	0.108***	(0.013)
1 × CtnBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × CtnBorder							0.112***	(0.034)	0.111***	(0.034)	0.111***	(0.034)
3 × CtnBorder							0.074*	(0.040)	0.072*	(0.040)	0.071*	(0.040)
4 × CtnBorder							-0.137**	(0.068)	-0.138**	(0.068)	-0.139**	(0.067)
5 × CtnBorder							-0.134**	(0.066)	-0.136**	(0.066)	-0.134**	(0.066)
6 × CtnBorder							-0.139**	(0.062)	-0.142**	(0.062)	-0.141**	(0.062)
7 × CtnBorder							-0.171***	(0.056)	-0.172***	(0.056)	-0.171***	(0.056)
8 × CtnBorder							-0.238***	(0.058)	-0.239***	(0.058)	-0.238***	(0.057)
9 × CtnBorder							-0.265***	(0.057)	-0.265***	(0.057)	-0.263***	(0.057)
10 × CtnBorder							-0.257***	(0.057)	-0.257***	(0.057)	-0.255***	(0.057)
11 × CtnBorder							-0.280***	(0.059)	-0.280***	(0.059)	-0.278***	(0.059)
12 × CtnBorder							-0.270***	(0.058)	-0.270***	(0.058)	-0.268***	(0.058)
13 × CtnBorder							-0.262***	(0.060)	-0.261***	(0.060)	-0.260***	(0.060)
14 × CtnBorder							-0.233***	(0.061)	-0.232***	(0.061)	-0.232***	(0.061)
1 × LangBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × LangBorder							0.020	(0.054)	0.018	(0.054)	0.018	(0.054)
3 × LangBorder							-0.001	(0.056)	-0.001	(0.056)	-0.003	(0.056)
4 × LangBorder							0.068	(0.072)	0.069	(0.072)	0.067	(0.072)
5 × LangBorder							0.015	(0.068)	0.016	(0.068)	0.015	(0.067)
6 × LangBorder							0.059	(0.062)	0.060	(0.062)	0.058	(0.062)
7 × LangBorder							0.008	(0.054)	0.009	(0.054)	0.007	(0.055)
8 × LangBorder							-0.015	(0.051)	-0.015	(0.051)	-0.017	(0.051)
9 × LangBorder							-0.003	(0.053)	-0.003	(0.052)	-0.006	(0.053)
10 × LangBorder							-0.058	(0.052)	-0.059	(0.052)	-0.061	(0.052)
11 × LangBorder							-0.114**	(0.052)	-0.115**	(0.052)	-0.117**	(0.052)
12 × LangBorder							-0.176***	(0.053)	-0.178***	(0.053)	-0.180***	(0.053)
13 × LangBorder							-0.176***	(0.053)	-0.177***	(0.053)	-0.179***	(0.053)
14 × LangBorder							-0.227***	(0.055)	-0.228***	(0.055)	-0.230***	(0.055)
1 × RelBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × RelBorder							-0.075**	(0.031)	-0.076**	(0.031)	-0.077**	(0.031)
3 × RelBorder							-0.041	(0.032)	-0.040	(0.032)	-0.041	(0.032)
4 × RelBorder							-0.077**	(0.037)	-0.078**	(0.037)	-0.078**	(0.037)
5 × RelBorder							-0.085**	(0.035)	-0.085**	(0.035)	-0.085**	(0.035)
6 × RelBorder							-0.079***	(0.029)	-0.080***	(0.029)	-0.081***	(0.029)
7 × RelBorder							-0.080***	(0.018)	-0.080***	(0.017)	-0.080***	(0.017)
8 × RelBorder							-0.051***	(0.015)	-0.051***	(0.015)	-0.051***	(0.015)
9 × RelBorder							-0.027**	(0.013)	-0.027**	(0.013)	-0.027**	(0.013)
10 × RelBorder							0.000	(0.013)	0.000	(0.013)	-0.000	(0.013)
11 × RelBorder							0.010	(0.013)	0.010	(0.013)	0.010	(0.013)
12 × RelBorder							0.012	(0.013)	0.012	(0.013)	0.012	(0.013)
13 × RelBorder							0.010	(0.014)	0.010	(0.014)	0.010	(0.014)
14 × RelBorder							0.016	(0.014)	0.016	(0.014)	0.016	(0.014)
# Observations	1,337,379		1,337,379		1,337,379		1,337,379		1,337,379		1,337,379	
Pseudo R^2	0.900		0.900		0.900		0.900		0.900		0.900	

Notes: This table reports the estimates from panel gravity PQML regressions of director-to-firm link counts between municipality pairs. All specifications include municipality-year and municipality-pair fixed effects. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Firm-to-firm aggregated network

Table 3.D.16: Three-way gravity results - Highway access (Firm-to-firm network)

	(1)		(2)		(3)		(4)		(5)		(6)	
$Access_{ij,t}$	0.113*	(0.059)	-0.025	(0.047)	0.009	(0.034)	0.050	(0.060)	-0.073*	(0.044)	-0.005	(0.037)
$Access_{ij,t-1}$			0.282***	(0.087)	0.275***	(0.081)			0.261***	(0.092)	0.241***	(0.083)
$Access_{ij,t-2}$			0.053*	(0.030)	0.049*	(0.029)			0.059*	(0.032)	0.052	(0.032)
$Access_{ij,t-3}$			-0.010	(0.056)	-0.013	(0.056)			-0.065	(0.052)	-0.071	(0.053)
$Access_{ij,t+1}$					-0.087	(0.080)					-0.191**	(0.080)
Total *Access*			0.300	(0.113)	0.321	(0.118)			0.181	(0.122)	0.216	(0.126)
1 × log Dist							0.000	(.)	0.000	(.)	0.000	(.)
2 × log Dist							-0.007	(0.020)	-0.010	(0.020)	-0.009	(0.019)
3 × log Dist							-0.025	(0.024)	-0.031	(0.022)	-0.042*	(0.023)
4 × log Dist							-0.030	(0.029)	-0.058*	(0.032)	-0.064**	(0.032)
5 × log Dist							-0.042	(0.031)	-0.053*	(0.030)	-0.061**	(0.030)
6 × log Dist							-0.022	(0.033)	-0.024	(0.030)	-0.033	(0.030)
7 × log Dist							-0.015	(0.023)	-0.020	(0.022)	-0.029	(0.022)
8 × log Dist							-0.019	(0.023)	-0.023	(0.023)	-0.032	(0.023)
9 × log Dist							-0.026	(0.023)	-0.031	(0.023)	-0.040*	(0.023)
10 × log Dist							-0.016	(0.023)	-0.020	(0.023)	-0.030	(0.023)
11 × log Dist							-0.016	(0.022)	-0.020	(0.022)	-0.030	(0.023)
12 × log Dist							-0.025	(0.022)	-0.030	(0.022)	-0.040*	(0.022)
13 × log Dist							-0.045**	(0.022)	-0.049**	(0.022)	-0.059***	(0.022)
14 × log Dist							-0.060***	(0.022)	-0.064***	(0.022)	-0.078***	(0.022)
1 × CtnBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × CtnBorder							0.095	(0.082)	0.093	(0.081)	0.096	(0.078)
3 × CtnBorder							0.047	(0.096)	0.041	(0.087)	0.084	(0.089)
4 × CtnBorder							0.029	(0.129)	0.088	(0.136)	0.118	(0.138)
5 × CtnBorder							0.110	(0.157)	0.104	(0.157)	0.103	(0.155)
6 × CtnBorder							0.078	(0.155)	0.018	(0.148)	0.032	(0.147)
7 × CtnBorder							-0.093	(0.101)	-0.087	(0.101)	-0.077	(0.100)
8 × CtnBorder							-0.063	(0.095)	-0.048	(0.095)	-0.038	(0.095)
9 × CtnBorder							-0.013	(0.095)	-0.007	(0.095)	0.002	(0.094)
10 × CtnBorder							-0.097	(0.097)	-0.090	(0.097)	-0.081	(0.096)
11 × CtnBorder							-0.116	(0.096)	-0.111	(0.095)	-0.101	(0.095)
12 × CtnBorder							-0.082	(0.097)	-0.076	(0.097)	-0.067	(0.096)
13 × CtnBorder							-0.055	(0.096)	-0.050	(0.095)	-0.041	(0.095)
14 × CtnBorder							-0.017	(0.098)	-0.012	(0.097)	0.002	(0.097)
1 × LangBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × LangBorder							-0.074	(0.119)	-0.090	(0.117)	-0.086	(0.117)
3 × LangBorder							0.086	(0.112)	0.060	(0.108)	0.063	(0.106)
4 × LangBorder							0.191	(0.148)	0.196	(0.144)	0.201	(0.144)
5 × LangBorder							0.357**	(0.173)	0.367**	(0.172)	0.377**	(0.170)
6 × LangBorder							0.388**	(0.160)	0.391**	(0.156)	0.399**	(0.156)
7 × LangBorder							0.044	(0.120)	0.043	(0.119)	0.052	(0.119)
8 × LangBorder							-0.041	(0.114)	-0.043	(0.113)	-0.033	(0.113)
9 × LangBorder							-0.076	(0.112)	-0.079	(0.111)	-0.069	(0.110)
10 × LangBorder							-0.043	(0.108)	-0.046	(0.107)	-0.036	(0.107)
11 × LangBorder							-0.065	(0.108)	-0.068	(0.107)	-0.057	(0.106)
12 × LangBorder							-0.130	(0.111)	-0.133	(0.110)	-0.122	(0.109)
13 × LangBorder							-0.060	(0.114)	-0.065	(0.112)	-0.054	(0.112)
14 × LangBorder							-0.039	(0.108)	-0.043	(0.107)	-0.031	(0.106)
1 × RelBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × RelBorder							0.043	(0.075)	0.030	(0.071)	0.039	(0.073)
3 × RelBorder							0.108	(0.079)	0.096	(0.073)	0.101	(0.072)
4 × RelBorder							0.136**	(0.058)	0.113**	(0.050)	0.118**	(0.048)
5 × RelBorder							-0.096*	(0.049)	-0.110**	(0.049)	-0.111**	(0.048)
6 × RelBorder							-0.115**	(0.047)	-0.130***	(0.045)	-0.128***	(0.045)
7 × RelBorder							-0.044	(0.028)	-0.050*	(0.029)	-0.048*	(0.029)
8 × RelBorder							-0.018	(0.023)	-0.021	(0.023)	-0.021	(0.023)
9 × RelBorder							0.014	(0.020)	0.009	(0.019)	0.009	(0.019)
10 × RelBorder							-0.018	(0.021)	-0.022	(0.021)	-0.021	(0.021)
11 × RelBorder							0.033	(0.023)	0.028	(0.023)	0.029	(0.023)
12 × RelBorder							0.015	(0.023)	0.010	(0.023)	0.011	(0.023)
13 × RelBorder							0.001	(0.026)	-0.004	(0.026)	-0.003	(0.026)
14 × RelBorder							0.008	(0.032)	0.004	(0.031)	0.005	(0.031)
# Observations	1,305,738		1,305,738		1,305,738		1,305,738		1,305,738		1,305,738	
Pseudo R^2	0.978		0.978		0.978		0.978		0.978		0.978	

Notes: This table reports the estimates from panel gravity PQML regressions of director-to-firm link counts between municipality pairs. All specifications include municipality-year and municipality-pair fixed effects. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Director-to-director aggregated network

Table 3.D.17: Three-way gravity results - Highway access (Director-to-director network)

	(1)		(2)		(3)		(4)		(5)		(6)	
$Access_{ij,t}$	0.038	(0.024)	0.023	(0.023)	0.022	(0.021)	0.041*	(0.024)	0.029	(0.023)	0.019	(0.021)
$Access_{ij,t-1}$			0.020	(0.016)	0.020	(0.016)			0.020	(0.016)	0.021	(0.016)
$Access_{ij,t-2}$			0.013	(0.014)	0.013	(0.014)			0.008	(0.014)	0.008	(0.014)
$Access_{ij,t-3}$			0.001	(0.014)	0.002	(0.014)			-0.017	(0.014)	-0.015	(0.014)
$Access_{ij,t+1}$					0.001	(0.019)					0.021	(0.019)
Total *Access*			0.057	(0.033)	0.057	(0.032)			0.040	(0.033)	0.033	(0.032)
1 × log Dist							0.000	(.)	0.000	(.)	0.000	(.)
2 × log Dist							0.018**	(0.008)	0.018**	(0.008)	0.018**	(0.008)
3 × log Dist							0.028***	(0.010)	0.028***	(0.010)	0.028***	(0.010)
4 × log Dist							0.048***	(0.012)	0.047***	(0.012)	0.047***	(0.012)
5 × log Dist							0.066***	(0.013)	0.066***	(0.013)	0.066***	(0.013)
6 × log Dist							0.119***	(0.013)	0.119***	(0.013)	0.119***	(0.013)
7 × log Dist							0.090***	(0.013)	0.090***	(0.014)	0.091***	(0.014)
8 × log Dist							0.081***	(0.014)	0.081***	(0.014)	0.082***	(0.014)
9 × log Dist							0.079***	(0.015)	0.079***	(0.015)	0.080***	(0.015)
10 × log Dist							0.051***	(0.014)	0.051***	(0.014)	0.052***	(0.014)
11 × log Dist							0.077***	(0.015)	0.077***	(0.015)	0.078***	(0.015)
12 × log Dist							0.071***	(0.015)	0.071***	(0.015)	0.072***	(0.015)
13 × log Dist							0.074***	(0.015)	0.074***	(0.015)	0.074***	(0.015)
14 × log Dist							0.078***	(0.015)	0.078***	(0.015)	0.079***	(0.015)
1 × CtnBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × CtnBorder							0.063	(0.040)	0.063	(0.040)	0.063	(0.039)
3 × CtnBorder							0.003	(0.044)	0.002	(0.044)	0.002	(0.044)
4 × CtnBorder							-0.097	(0.060)	-0.097	(0.060)	-0.097	(0.060)
5 × CtnBorder							-0.159***	(0.058)	-0.159***	(0.058)	-0.158***	(0.058)
6 × CtnBorder							-0.198***	(0.057)	-0.199***	(0.057)	-0.198***	(0.057)
7 × CtnBorder							-0.180***	(0.060)	-0.180***	(0.060)	-0.180***	(0.060)
8 × CtnBorder							-0.220***	(0.061)	-0.220***	(0.061)	-0.219***	(0.061)
9 × CtnBorder							-0.215***	(0.061)	-0.216***	(0.061)	-0.215***	(0.060)
10 × CtnBorder							-0.176***	(0.060)	-0.176***	(0.060)	-0.176***	(0.060)
11 × CtnBorder							-0.184***	(0.061)	-0.184***	(0.061)	-0.183***	(0.061)
12 × CtnBorder							-0.180***	(0.062)	-0.181***	(0.062)	-0.180***	(0.062)
13 × CtnBorder							-0.143**	(0.062)	-0.143**	(0.062)	-0.142**	(0.062)
14 × CtnBorder							-0.091	(0.061)	-0.091	(0.061)	-0.091	(0.061)
1 × LangBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × LangBorder							-0.225***	(0.064)	-0.224***	(0.064)	-0.225***	(0.064)
3 × LangBorder							-0.233***	(0.064)	-0.232***	(0.064)	-0.233***	(0.064)
4 × LangBorder							0.006	(0.071)	0.007	(0.071)	0.007	(0.071)
5 × LangBorder							-0.078	(0.062)	-0.078	(0.062)	-0.078	(0.062)
6 × LangBorder							0.041	(0.057)	0.041	(0.057)	0.040	(0.057)
7 × LangBorder							-0.047	(0.056)	-0.047	(0.056)	-0.048	(0.056)
8 × LangBorder							-0.014	(0.054)	-0.014	(0.054)	-0.015	(0.054)
9 × LangBorder							-0.045	(0.056)	-0.045	(0.056)	-0.046	(0.056)
10 × LangBorder							-0.045	(0.057)	-0.045	(0.057)	-0.046	(0.057)
11 × LangBorder							-0.126**	(0.055)	-0.126**	(0.055)	-0.127**	(0.055)
12 × LangBorder							-0.209***	(0.056)	-0.208***	(0.056)	-0.210***	(0.056)
13 × LangBorder							-0.222***	(0.057)	-0.222***	(0.057)	-0.223***	(0.057)
14 × LangBorder							-0.238***	(0.057)	-0.238***	(0.057)	-0.239***	(0.057)
1 × RelBorder							0.000	(.)	0.000	(.)	0.000	(.)
2 × RelBorder							-0.031	(0.029)	-0.031	(0.029)	-0.032	(0.029)
3 × RelBorder							-0.022	(0.027)	-0.022	(0.027)	-0.022	(0.027)
4 × RelBorder							-0.094***	(0.030)	-0.093***	(0.030)	-0.093***	(0.030)
5 × RelBorder							-0.051**	(0.022)	-0.052**	(0.022)	-0.052**	(0.022)
6 × RelBorder							-0.081***	(0.015)	-0.081***	(0.015)	-0.081***	(0.015)
7 × RelBorder							-0.082***	(0.016)	-0.082***	(0.016)	-0.082***	(0.016)
8 × RelBorder							-0.050***	(0.014)	-0.050***	(0.014)	-0.050***	(0.014)
9 × RelBorder							-0.014	(0.012)	-0.014	(0.012)	-0.014	(0.012)
10 × RelBorder							-0.010	(0.012)	-0.010	(0.012)	-0.010	(0.012)
11 × RelBorder							-0.016	(0.012)	-0.016	(0.012)	-0.016	(0.012)
12 × RelBorder							-0.042***	(0.012)	-0.042***	(0.012)	-0.042***	(0.012)
13 × RelBorder							-0.022*	(0.012)	-0.022*	(0.012)	-0.022*	(0.012)
14 × RelBorder							-0.008	(0.013)	-0.008	(0.013)	-0.008	(0.013)
# Observations	1,833,416		1,833,416		1,833,416		1,833,416		1,833,416		1,833,416	
Pseudo R^2	0.830		0.830		0.830		0.830		0.830		0.830	

Notes: This table reports the estimates from panel gravity PQML regressions of director-to-firm link counts between municipality pairs. All specifications include municipality-year and municipality-pair fixed effects. Robust standard errors, clustered by municipality pair, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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