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

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Abstract

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 the variation in the timing of the access openings and use a staggered difference-in-differences approach to measure 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 an effect on the relocation of firms. 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 robust to various robustness checks.

1 Introduction

Infrastructures impact our lives, exchanges, and economy in many ways. Railroads, highways, and air travel are especially central in shaping today's spatial economic activity. They do so by alleviating transportation costs. Enhanced infrastructure can increase the attractiveness of a region to businesses by facilitating the movement of goods, market accessibility, and the procurement of inputs. Highways and railroads cause an increase in economic activity in rural areas near highways compared to regions further away from the road network (Redding & Turner, 2015). In this sense, transportation infrastructure serves as a means to improve local economic integration by reducing transportation costs.

This paper utilizes the development of the highway network in Switzerland to investigate its impact on the number of firms at the municipal level. We study this push in Swiss domestic market integration through reductions of transportation costs. We also investigate the births, deaths, and movements of firms following the construction of new highway access. Switzerland had no highways before WWII. The project was discussed immediately after the war ended, but it took several additional years for concrete advances. The highway network was largely defined in the 1950s. The plans were agreed on at the end of the decade and then almost entirely constructed between 1960 and 2010. Although the main connections were clearly defined, the details on the exact route were still discussed long after.

We rely on the inconsequential unit approach pioneered by Chandra & Thompson (2000) and on data from Fretz et al. (2021) on access to highways in Switzerland. They collected data on the specific timing of highway openings, which connected different regions with a much faster alternative transport route. More specifically, we evaluate the spatial distribution of the number of firms at the municipal level after the opening of access to the highway network. We compare municipalities that had early access to highways nearby with those that received similar access 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 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 a proxy for the size of the companies. Third, we track the movements of firms and determine whether the effects come from the creation of new firms or the relocation of existing ones. This also allows us to learn more about firms' location decisions. Our results shed light on the effect of transportation on the spatial inequality of economic activities. In addition, we examine whether the timing of highway access matters by using an extended two-way fixed effects (ETWFE) estimator for staggered difference-in-differences, which allows for non-negative count data (Wooldridge 2021, 2023).

To do so, we created an original and historical dataset. We collected information on all "*Aktiengesellschaften*" (Henceforth "AG", a form of limited companies) in Switzerland between 1934 and 2003 from the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*" (or "*Répertoire des administrateurs des sociétés anonymes suisses*" in French). This publication lists all individuals with at least one mandate on a board of directors in the country and includes other information on the firms. To construct the panel data of firms, we had to match firms across cross-sections. We used the geolocalisations of the firms and their names to attribute a unique ID over all periods (using a fuzzy matching algorithm).

For this paper, the geographical units of interest are the Swiss municipalities. Thus, we aggregate the firm-level data into a municipal-level panel, containing the number of firms, information on the board of directors, and nominal capital, as well as the distribution of these variables in Switzerland. We merged our data with publicly available data on highway access constructed by Fretz et al. (2021). In addition to the stock of firms, we measure the number of newly created firms every year for each municipality. We also know how many ceased to exist and how many relocated. This allows us to investigate what drives the change in the number of firms. Are the changes coming from newly created firms, those that are dying, or simply the result of relocation between locations?

We contribute to the existing literature in the following ways. Firstly, we aim to investigate the question of whether diminishing transportation costs increase economic activity or reorganize existing activities (Fogel 1964, Redding & Turner 2015). Using our firm-level panel data, we can track firms' movements and creations (or deaths), which allows us to investigate the "growth versus reorganization" problem. Secondly, we observe heterogeneous effects depending on the size of the firms. Finally, we test whether the results observed vary when allowing treatment effects to differ across cohorts.

In our baseline specifications, we quantify the dynamic and long-run effects of getting highway access on the variables of interest at time t relative to getting it later. For the number of firms, we observe a positive effect in the year following the opening of access in the specification that includes the full set of fixed effects and municipal time trends. The stock of companies continues to accumulate 15 years after the construction of the highways. These results are robust to other definitions of the treatment group and the inclusion of never-connected municipalities in the control group. We do find large and significant long-term effects for our preferred specification.

The more detailed outcome variables show that the increase in the number of firms comes from large selection effects. New firms are being created, but incumbent ones are also being closed. First, the number of newly created firms in the treated municipalities does not increase before the opening of the access. We find positive and statistically significant effects starting from the year following the opening of highways. The highways also increase the number of firms created in the long run. Second, the number of firms disappearing also increased significantly. Finally, the results on the relocation of firms from and to the treated municipalities are inconclusive. We do find positive coefficients in the number of in- and out-migrations of firms, but they are statistically insignificant. In conclusion, the construction of highways in Switzerland led to an increase in firm creation when access to the highways became available. The increase in competition (both locally and from cities) forced existing firms to close. This suggests that the increase in the number of firms comes from the creation of new firms, rather than from the relocation of existing economic activities. The long-lasting increase in the stock of firms suggests that the creation of firms surpasses both the deaths and out-migrations of existing firms. Although the data requirements of Wooldridge's approach (2021, 2023) limit our options, we obtain similar results in cases where it can be applied.

We complement the main analysis with a variety of robustness checks and extensions. We first include municipalities further away in the control group and find similar results. Then, we run the analysis on a subsample that excludes firms existing for only one period in our panel. This approach addresses potential artificial increases in the birth of firms resulting from the fuzzy matching of observations across periods. Another way to control for whether the effects come from unmatched observations across periods is to create a "net-birth" variable, which involves subtracting the deaths of the previous period from the births in the current period. In both cases, we find comparable results to those in our main analysis. We also run placebos, where we randomize the timing of the treatment, and the results suggest that the effects come from the highways, not chance.

The structure of this paper is as follows. In the next section, we review the related literature and set the theoretical framework. Section 3 describes the context in which the development of the highways in Switzerland occurred. Section 4 presents our data and provides descriptive statistics. In Section 5, we describe our empirical approach. Section 6 presents our empirical results, while Section 7 contains different robustness checks. Section 8 concludes the paper.

2 Theoretical framework and related literature

2.1 Theoretical framework

This paper is related to previous research in different aspects. We focus on investments in Switzerland's national high-speed road network and their effects on economic activity, specifically firm creation and relocation.

Market access is improved for firms close to transportation networks, resulting in comparative advantages in terms of access to workers, managers, consumers, suppliers, or resources compared to competitors located further away from these networks (Eberts & McMillen 1999, Bernard & Moxnes 2018). The results in Bernard et al. (2019), for example, provide evidence that comparative advantage occurs across firms within a country through reductions in search costs, not only through transportation costs. A reduction in the distance between seller and buyer also leads to a more than proportional reduction in transport costs (Atkin & Donaldson 2015). In addition, transportation networks have long-lasting effects on economic integration and trade between regions (Barjamovic et al. 2019, Flückiger et al. 2021). Consequently, firms should be attracted to locations close to highway access and the long lasting advantages they provide in addition to the short-term benefits.

However, attracting new firms means stronger competition in those municipalities. Increasing competition could lead to present firms exiting or closing if they are not productive enough. We expect an increase in aggregate productivity as more productive firms enter the market and less productive ones leave (Melitz 2003). The newly connected municipalities are now more tightly connected to cities where the highways pass. Thus, depending on the distance between them and the cities, rural municipalities might face competition from urban firms. This can negatively affect incumbent firms in rural connected municipalities, as in Qin (2017) in the context of railway development in China.

To summarize, on the one hand, there is a better market reach (for inputs, consumers, workers, and directors) and reduced transportation costs, improving regional economic integration and attracting firms. This will generate the positive spillovers generated by agglomeration economies, and highway access will lead to an increase in the number of firms. On the other hand, stronger competition can push firms to exit or close. The two effects go in opposite directions, and the stock of firms might increase or decrease. At the same time, remote locations now face competition from the centers as lowering the transportation costs reduce the advantages of being isolated in smaller regions (Proost & Thisse 2019).

2.2 Related literature

Investment in road infrastructure is a well-studied topic in the economic literature. Previous works have investigated the various effects of infrastructure development, with a focus on railroads or highways. Many outcomes have been studied, including demographic outcomes such as population growth, population sorting, and segregation, as well as taxpayers, commuters, and workers sorting (Levkovich et al. 2019, Fretz et al. 2021). The relationship between highways and segregation is studied by Brinkman & Lin (2022) and Mahajan (2024). Real estate and land prices, and urban development, as in Atack et al. (2010), Duranton & Turner (2012), Hornung (2015), Donaldson & Hornbeck (2016), and Levkovich et al. (2019). Political outcomes such as political participation (Huet-Vaughn 2019) or geographic polarization (Nall 2015). Volpe Martincus & Blyde (2013), Duranton et al. (2014), and Donaldson (2018) are examples of works interested in 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 results find positive effects after highway developments (Duranton & 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 were interested in the effects of investments in transportation infrastructures on various outcomes in developing countries (Datta 2012, Faber 2014, Ghani et al. 2016, Jedwab & 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 & Játiva 2024).

Other studies investigated the effects on firms. Datta (2012) studies the impact of upgraded roads in India on the inventory management of firms along the Golden Quadrilateral (GQ) program. Firms located near the improved highways decreased their inventory and had more efficient supplier turnover compared to 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 the 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 closing rate, despite higher competition brought about by the increase in entrants. Duranton & Turner (2012) find similar 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%.

In a series of papers, Holl (2004a, 2004b, 2004c) focuses on manufacturing firms at the plant level. In her first study, she found 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) confirms that the positive effect is concentrated in this 10 km corridor near the highways for a variety of industry. 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).

Even more related are the papers from Percoco (2016), Gibbons et al. (2019), and Busso & Fentanes (2024). 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. 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) found 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 & Fentanes (2024) show that new highways in Mexico led to higher local labor productivity primarily through changes in firm dynamics. In their causal estimations, they find an increase in firm entries and a decrease in firm exits (by 1.6 and 3.3 percentage points, respectively). While methodologically related, our study differs in key respects. However, the context differs vastly between Busso & Fentanes (2024) and our research. Their study examines Mexico, a developing economy with infrastructure gaps, between 1998 and 2018, while our study focuses on Switzerland between 1960 and 2003, a high-income country with mature infrastructure (railways and roads), closely located urban centers, and different firm dynamics.

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 located within 10 km of road 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, thanks to the easier commute via the new roads.

In this paper, 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 providing labor to workers and commuters near the cities. Their findings align with the comparative advantage argument presented by Bernard et al. (2019). Even in rural areas, highways will enhance the agglomeration economies and attract firms in already relatively more agglomerated markets.

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, an ambitious program was launched in 1951. On March 21, 1958, the Swiss population approved a popular initiative[1] 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 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 of Figure 1 shows the progress made in 2003.

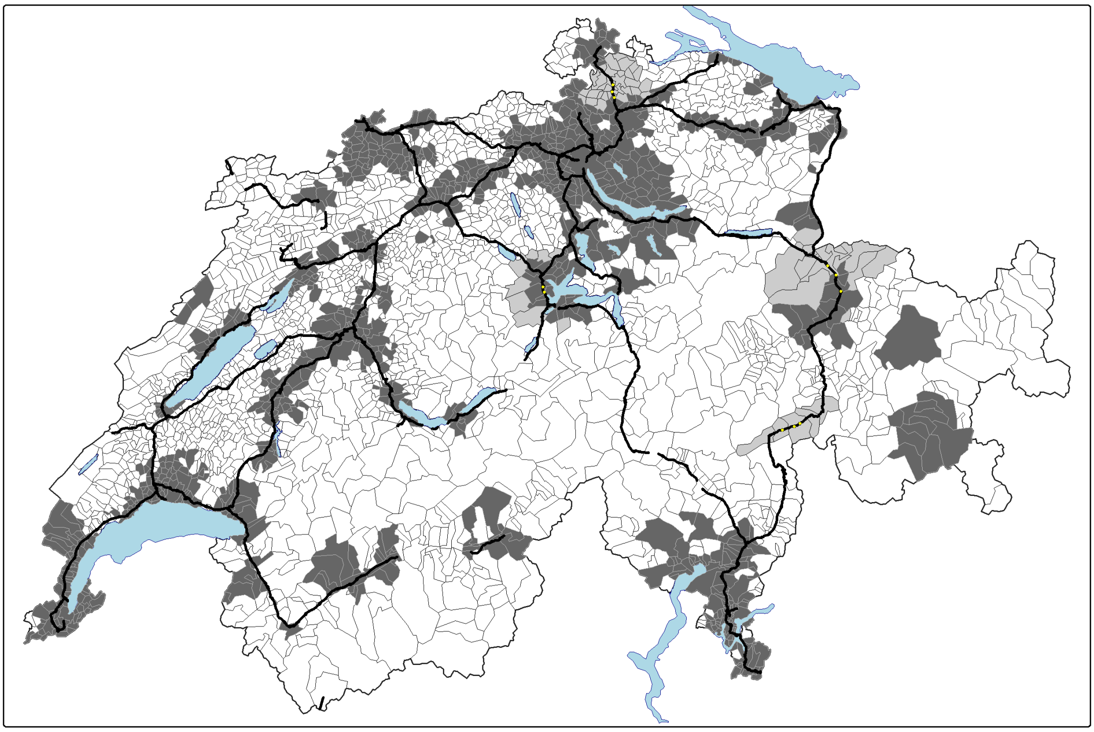
The program aimed to link the largest cities (Basel, Bern, Geneva, Lausanne, and Zurich) and connect other important ones (Baden, Biel, Fribourg, Luzern, Neuchâtel, Olten, Sankt-Gallen, Solothurn, Winterthur, and Yverdon). Between those big cities, various options were considered. For example, the highway linking Bern to Lausanne had nine possible layouts before construction[2]. Ultimately, the highway passes next to three important regional cities in addition to Fribourg: Bulle, Châtel-Saint-Denis, and Vevey. Their regional political influence and economic importance compared to other municipalities likely played a significant role in the planning. 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 in anticipation of the 1964 National Exhibition.

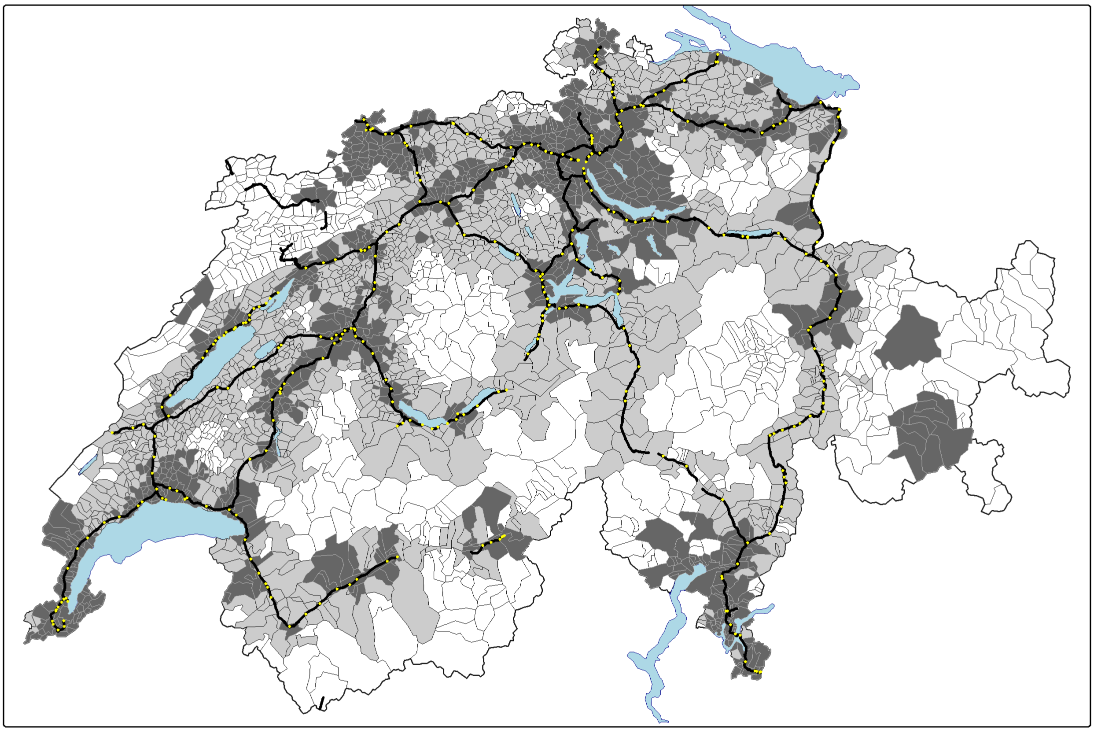
The construction of the network was planned over a period of 20 years, with an estimated cost of 2.93 billion CHF. However, these estimates proved overly optimistic. In 1963, already 700'000 cars were circulating on Swiss roads. By 1965, motor vehicle numbers had already exceeded the projections made for 1980, necessitating an upward revision of the

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

[2] See Figure 7 in Appendix A.3.

Figure 1: Location of all access built until 1960 & 2003



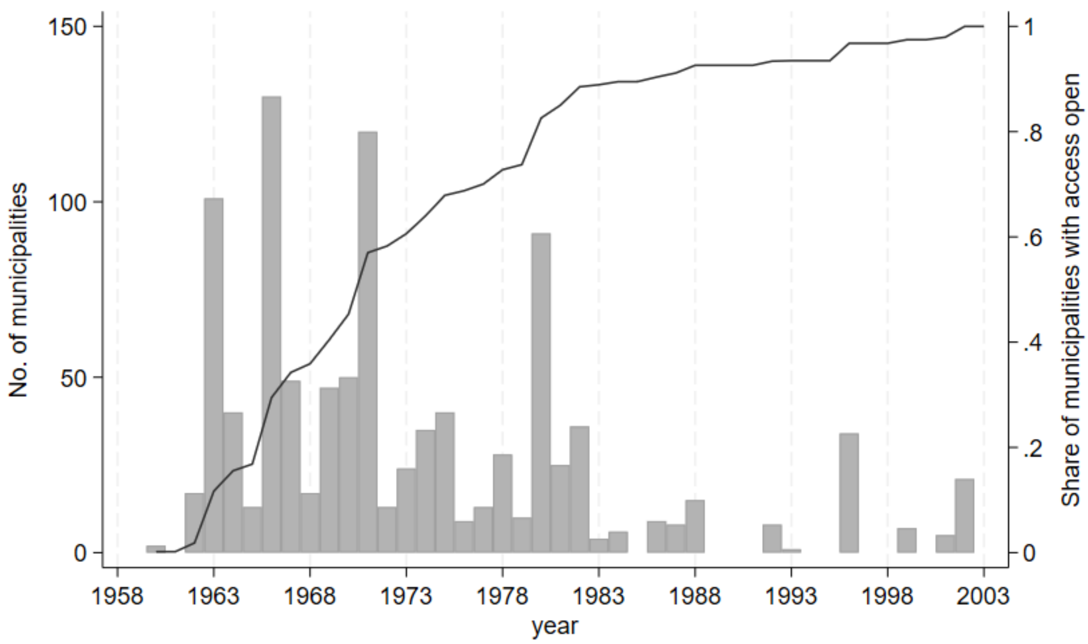


Note: Yellow dots show the highway access. Dark grey areas show cities & municipalities within agglomerations. Light grey areas are the rural municipalities with access within 15 km distance from their center. Based on Fretz et al. (2021).

national road capacity. The increase in costs led to project delays, ensuring that funding could continue to be supported by fuel taxes. The estimations of the cost were even more inaccurate: in 1997, a report from the Control Committee of the National Council rang 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 Cantons. They estimated the remaining costs at around 25 billion CHF, highlighting the complexity of the project.

The development of the Swiss highway system was not without its share of opposition. Like many large-scale infrastructure projects, highway development faced delays, but also criticism from various quarters. For example, the A1 highway was originally planned to be built over swamps, which were initially selected due to their low agricultural value. Diverse ecological groups formed an opposition to this layout, as they sought 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. Tired of the numerous oppositions delaying the A1 construction, the focus shifted toward the A12, linking Fribourg to Vevey. The bill for the A1 began to balloon in the mid-1980s when the wishes of opponents - environmentalists, farmers, and local authorities - were taken into account, necessitating major changes in the planned layouts.

Figure 2: Number of municipalities gaining access by year and share of municipalities with access (15 km or less)



Note: This figure shows the number of municipalities receiving an access by year. The right axis shows the share of treated municipalities, which add up to 100% as we limit the sample to the eventually treated municipalities.

This example, coupled with the financial delays mentioned earlier, is one of the many cases where the initial plans and the timing of its opening did not occur 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 from its initial planned duration. In 2010, the network was almost entirely built with 94.6% of the initial project completed. Figure 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. Around 60% of the access were built in 1971. More than 80% of the network was completed before the 1980s.

Of course, transport is not limited to roads. The Swiss railway network was developed long before the 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, such as the reorganization of train schedules, the modernization of the rolling stock, and the improvement of the regular interval timetable. Even though some changes occurred, the focus was clearly on improving the commuting lines, and no major new rails were constructed over our sample period (Swiss Federal Office of Transport 2007).

4 Data

4.1 Firms data

We have digitalized all existing editions of the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften*" (or "*Répertoire des administrateurs des sociétés* anonymes suisses" in French) between 1934 and 2003. Figure 6 in Appendix A.1 shows an example of a page from the publication. These collections are based on the 27 commercial registries and contain every person who holds a mandate on the board of directors of a type of limited company (AG) in a specific year, with information such as the name, address, function in 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[3]. Since we can track firms

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

across periods, we can 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 is 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, 1962-1966, 1969, 1975, and 1979-2003. We explain how we dealt with the gaps in our different outcome variables in Appendix A.2.

Our dataset also includes the category of municipalities as defined by the Federal Statistical Office (Swiss Federal Statistical Office 2005). There are 27 ″urban centers″, 865 ″suburban municipalities″, and 1469 ″non-urban municipalities″ in our sample. In our analysis, we use the non-urban ones to create our treatment and control groups. As expected, most firms are located in cities and suburban municipalities. Across all years in our sample, cities host an average of 1′977.05 firms. The number of firms located in suburban municipalities is way lower, with an average of 39.67 firms. Unsurprisingly, there are even fewer firms in non-urban municipalities. The non-urban municipalities located within 15 km of a highway access have, on average, 11.63 firms, while those further away have 8.6 firms. Table 11 in Appendix B.1 provides summary statistics by type of municipality.

Regarding the evolution over the considered period, Table 12 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 increased to 12%. The last line indicates the change in importance of each type of municipality when comparing before and after the construction of highways. One caveat in our case is that we limit the sample period to 1960-2003. Which means that our *Before* measures already had some treatment occurring (around 40% of municipalities were treated in 1970). However, the goal of this Table is to illustrate trends, not to run causal analysis.

The interesting aspect of Table 12 is that the urban centers experienced a decline in national importance across all variables: the number, creations, deaths, relocations of firms, as well as employment-related variables. Note that there are only 27 urban centers, so even if the share of firms dropped, the share remains high for such a small number of municipalities. All three other types of municipality saw an increase in our firm variables. Suburban municipalities experienced the largest increase. This finding aligns with those of Fretz et al. (2021), who also report effects of residential urban sprawl. Economic activities, as measured by our firm variables, also appear to spread outside of the cities.

However, when we look at agglomerations as a single group, we see that there are almost no changes. We show results suggesting this phenomenon, as shown in Table 8 in Appendix C.2.2.

The firms are geolocalized where the headquarters are officially registered. This could be an issue if the headquarters were located at a different location than the production sites. However, most companies are small in size, making it more likely for them to have both their headquarters and production facilities in the same location. Regarding larger companies, they tend to have several distinct registered firms (e.g., holdings, management entity, etc.), making them more likely to have their headquarters location and production sites 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). Percentiles are calculated based on the nationwide distribution of capital for each year. We also proxy the size of firms using the size of their board of directors, with the hypothesis that larger firm requires more board members. Unfortunately, extensive and systematic firm data is quasi-inexistent for Switzerland, even aggregated at the municipal level. This limits the analysis due to the lack of labor market outcomes.

The count of firms in each municipality is a non-negative integer value. A common way to model count data is as a Poisson-distributed variable. The Poisson distribution explicitly handles the integer property of the dependent variable and includes zero observations as a natural outcome. An analysis focusing on small rural municipalities implies zero counts for certain years. The proportion of zero observations is large in our sample. In the full sample, observations with no firm represent 20.60% of all observations. This percentage increases to 27.69% when we consider only non-urban municipalities. Unsurprisingly, in the subsample of non-urban connected municipalities, the percentage of observations without any firm is slightly lower (26.91%). Figures 8 and 9, also in Appendix 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), where zeros are even more prevalent.

4.2 Other data sources

As previously mentioned, 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 distance between the centroid of all municipalities and the nearest entry to the high-speed network. We define the treatment group as the municipalities with access within a 15 km distance. We allow for this definition to vary in our robustness checks. A more common distance used is 10 km (Holl 2004a, 2004b, 2004c, Faber 2014, Fretz et al. 2021), but our results (see Table 7 in our Extension section) suggest that part of the effect also occurs between 10 and 15 km. Their access data not only indicates the distance to the closest access, but it also counts the number of access within different distance thresholds. From their dataset, we also utilize the variable indicating whether a railway passes through a municipality's territory.

We also make use of data from the Federal Population Census (Swiss Federal Statistical Office 2000), the Business Census (Swiss Federal Statistical Office 1955), and the distance to the closest urban center from Fretz et al. (2021). We also compute our distance measure, which simply consists of the straight line distance. We primarily use that data for robustness checks and extensions of the main analysis.

5 Empirical methodology

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. Those accesses connected different municipalities with a much faster alternative transport route than previously. We exploit the timing-based data structure and restrict the analysis to ever-treated units only to limit selection bias. The timing of the opening of new highway access varies across the treated municipalities. As in Hornung (2015), Donaldson (2018), or Fretz et al. (2021), all municipalities are included in our main analysis at the end of the panel. However, they get this treatment in different years. This staggered treatment allows us to use the municipalities that will get access in later periods as controls for the already-treated ones.

To deal with potential omitted variable bias, we include unit fixed effects, year fixed effects, and municipality-specific time trends in our regressions. This approach allows us to account for nationwide shocks and variations in growth rates across individual unobserved variables. Additionally, we investigate whether there are any differences in railway accessibility between municipalities treated early and those treated later. Railways influence firms' location decisions and could act as a confounding variable. Fortunately, as noted in section 3, nearly all railroads were already built at the beginning of the twentieth century, long before our panel starts (Büchel & Kyburz 2018). Additionally, our panel dataset ends in 2003, just prior to the expansion of the rail network in 2004. Therefore, there were no major railway constructions during the years covered by our data.

Of course, the positions of the access are not decided randomly and depend on the specific socio-economic contexts. On the one hand, it is not by chance that the main cities of Switzerland are well-connected and close to several highway entrances. The purpose of such high-speed roads is precisely to connect large cities and facilitate exchanges between them. On the other hand, there are extremely remote municipalities where it makes little economic sense to build such infrastructure. For these two reasons, treatment is not random. Thus, selection bias poses major threat to identification. Never-treated municipalities might be incomparable to treated ones, but so are treated ones between themselves. Important municipalities may be the focus of the network's early development.

To limit this bias, we need to carefully build treatment and control groups for which the treatment is exogenous. First, we exclude remote municipalities from our control group. Considering only ever-treated municipalities deals with part of the endogeneity caused by the non-random location of highway access. Second, we rely on the "inconsequential place approach" used by Chandra & Thompson (2000).We restrict our analysis to non-urban municipalities, as they receive the treatment mostly based on their locations between major cities. Due to their small size, these municipalities were unlikely to influence highway routing decisions. As we have discussed in the context section, the main goal was precisely to link big cities and not to develop economic activities in randomly connected remote municipalities.

We will estimate the cumulative effect of having an entry to the highway network within 15 km of the municipalities. Anticipation is crucial in preventing the internalization of transportation cost reductions into land value and the real estate market. Those areas around highways are becoming more attractive for firms, and being the first to seize the opportunity allows them to avoid facing increased prices or rents (Duncan 2011). Following Miller's (2023) recommendation, we avoid setting the period we want to normalize to zero mindlessly. There is no straightforward solution in our context, and, therefore, we need to rely on a judgment call. Thus, to account for possible anticipatory effects, we will set the reference period 4 years prior to the treatment. We decide to use this reference period because, as stated by Fretz et al. (2021), data from specialized websites suggest that the construction time of large infrastructure, such as bridges or tunnels, is approximately 4 years on average. Note that we only observe the moment the 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 moved to the location yet.

Staggered treatment occurs when treatment timing varies across units or groups. When multiple periods and groups are available, and the treatment has been implemented over several periods, the standard two-way fixed effects method might struggle to identify the average treatment effects. 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 avoid biases in estimation when implemented correctly. TWFE can estimate treatment effects efficiently, yielding results similar to those of Borusyak et al. (2024) or Gardner (2022). Although TWFE may be sufficient, we will implement Wooldridge's approach as a robustness check. Wooldridge (2021, 2023) proposes that the extended two-way fixed effects (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 will rely on his method to allow all treatment effects to vary by cohort and time.

5.2 Specifications

5.2.1 Standard Staggered Difference-in-Differences

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

$$y_{it} = \sum_{\tau=-8}^{\tau=15} \beta_\tau Access_{i,t+\tau} + +\xi_i + \xi_t + \theta_i t + \epsilon_{it} \tag{1}$$

In our baseline regressions, y_{it} represents the different outcome variables (stock, births, deaths, and relocations of firms) located in municipality i at time t. $Access_{i,t+\tau}$ is a dummy variable that takes on a value of 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 $\theta_i t$. The municipality-time error term is ϵ_{it} and is clustered at the district level to account for possible autocorrelation over time and units as suggested by Bertrand et al. (2004)[4].

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

The event-study equivalent of the previous regression in equation (1) is:

$$\gamma_p = \begin{cases} -\sum_{\tau=p+1}^{r} \beta_\tau & \text{for } -8 \leq p \leq r < -1 \\ 0 & \text{for } p = r = -4 \\ \sum_{\tau=r+1}^{p} \beta_\tau & \text{for } r+1 \leq p \leq 15 \end{cases} \tag{2}$$

The long-term cumulative effect is calculated following Davidson & MacKinnon (2004). The reference year is -4, to match the reference year in our dynamic estimations. Thus, equation (1) becomes:

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

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

$$y_{it} = \gamma Access_{it} + \sum_{\tau=-3,\tau\neq 0}^{15} \beta_\tau (Access_{it-\tau} - Access_{it}) + \xi_i + \xi_t + \theta_i t + \epsilon_{it} \tag{4}$$

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

5.2.2 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 two-way fixed effects as thoroughly shown in the recent literature on difference-in-differences (e.g., de Chaisemartin & 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. However, like in our case with count and non-negative dependent variables, this assumption may not hold. Thankfully, Wooldridge (2023) demonstrates that the linear ETWFE model can be adapted to accommodate non-linear models. We use the *jwdid* command on Stata to apply Wooldridge's (2023) approach[5].

[5] Section 3 of Nagengast et al. (2024) provides a detailed explanation of the command

More formally, the baseline equation in Wooldridge (2021) is as follows:

$$Y_{i,t} = \alpha + \sum_{g \in G} \sum_{t=g}^{T} \theta_{g,t} D_{i,g,t} + \xi_i + \xi_t + \epsilon_{i,t} \quad (5)$$

$Y_{i,t}$ is the outcome variable, $D_{i,g,t}$ is a dummy that takes the value of 1 if the observation 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 (5) becomes :

$$E(Y_{i,t}|X, \xi_i, \xi_t) = H\left(\alpha + \sum_{g \in G} \sum_{t=g}^{T} \theta_{g,t} D_{i,g,t} + \xi_i + \xi_t\right) \quad (6)$$

With $H(\cdot) = H(\exp)$. If we assume $H(\cdot)$ to be the identity function, we go back to the standard ETWFE. It is worth noting that the approach is particularly demanding in our case. We have 43 periods t and 26 different cohorts g. Therefore, we need to estimate one coefficient for every combination of t and g (1118 different $\theta_{g,t}$). The number of estimated parameters increases rapidly with the number of cohorts and analysis periods. Adding municipality time trends, as in our standard staggered difference-in-differences, poses additional issues for 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.

The approach allows for flexible aggregations of the coefficients. After estimating the model, under the default options, 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 on the overall data, across groups or periods, or the dynamic effects. We calculate the dynamic treatment effects relative to the reference period and replicate our analysis by including never-treated observations located within a distance of 15 to 30 km from an access point.

6 Results

6.1 Standard Staggered Difference-in-Differences

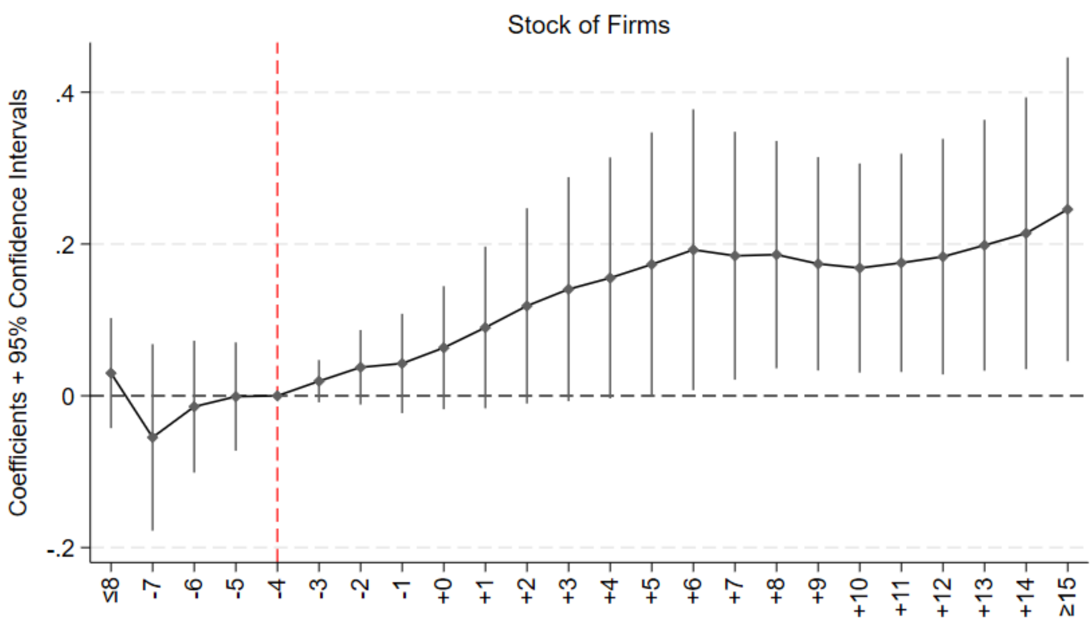
6.1.1 Stock of firms

We start by presenting the results for our stock variable, the number of firms, in Table 1. The first part of the table 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. After introducing municipal-specific time trends in the specification (2), our estimations are now statistically significant, with quasi-identical coefficients compared to those in (1). The number of firms starts to increase in period 1. Figure 3 illustrates the accumulation of the effect over time in our full specification. We observe that, prior to the reference period, the parallel trends assumption holds. The number of firms increases over time, with a slight break between periods 6 and 10, before continuing to increase even 15 years after the opening of the access.

The long-term effects measured following Davidson & MacKinnon (2004) are shown at the bottom of the table. We find a 30.34% long-term increase in the number of firms in specifications (2)[6].

Figure 3: Cumulative effect j years before/after access (<15 km) - Number of firms



[6]The effect is calculated as follows: $100 \cdot \left(e^{0.265} - 1\right) = 30.34\%$

Table 1: Cumulative effect j years 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	

Note: 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 * $p < 0.10$.

The results align with our expectations. The comparative advantage of municipalities having access before others leads to an increase in the number of firms compared to those that are yet to receive access. This increase comes from the decrease in transportation costs resulting from the construction of new roads. The positive effects are also consistent with the existing literature on the impact of road development on plants and establishments. However, similarly to the literature, we are still blind to the ”growth versus reorganization” problem when using the stock of firms. We contribute by utilizing the panel structure of our dataset, which enables us to track firms over time and distinguish between creations and relocations of firms.

6.1.2 Creation, death, and relocation of firms

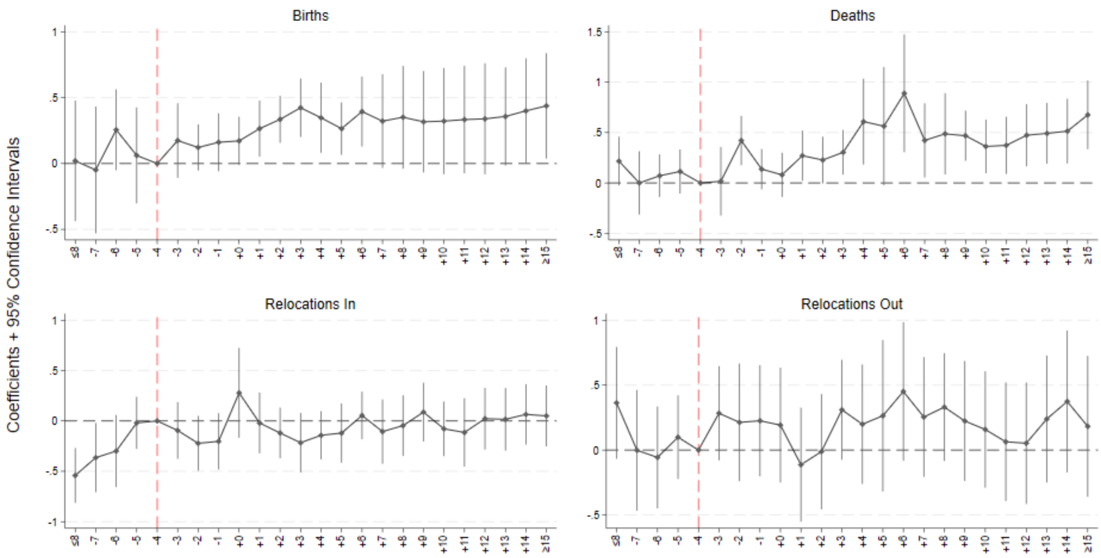
To do so, we now turn to our ”flow” outcome variables. We estimate the same regressions but using the number of newly created, dying, and relocating firms from and to the treated municipalities. Table 1 also shows the cumulative effects for each outcome variable. We present the results with our full specification, which includes municipality-specific time trends. The complete tables with our two different specifications are in Appendix C.1 Figure 4 visually presents the effects for all flow variables.

Column (3) shows the number of newly created firms in the treatment group. We find a positive effect starting at period 0, and all subsequent periods show that the highway construction induced a large increase in the number of firms created. Again, the increasing trend slows down around period 6. The coefficients are also more unstable due to the flow nature of the variable (while the number of firms is a stock). The long-term effect is 75.59%. Note that the average number of yearly firm creations in the treatment group across all periods is 1.46. The large effect in percentage also comes from the very few creations in our treated sample in general.

We also observe an increase in the number of closures in the treatment municipalities, as shown in column (4). In this case, we observe an effect before the reference year (in period -8, which is binned), suggesting that the assumption of parallel trends might not hold. The cumulative effect starts to increase after period 1. The effect fluctuates more for the closure of firms, but again, the long-term effect shows a substantial increase in the number of firm deaths (63.39%). The results in columns (5) and (6) are inconclusive. The effects on the number of firms relocating in and out of the treated municipalities are positive, but statistically insignificant and noisy. This is good news for our stock estimates, as the new road in the treated municipalities does not lead to migration of economic activity to those municipalities. Finding no effect on relocations reinforces our estimation in the baseline results, as this suggests that the Stable Unit Treatment Value Assumption (SUTVA) is not a major threat to identification. More specifically, if firms were moving from untreated (even not-yet-treated) to treated municipalities, then the assumption would be violated. Here, the effect on the stock in treated municipalities comes from the treatment to which they are assigned, not the treatments of others.

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 the creation of firms that exceeds the number of deaths. In brief, the development of highways in Switzerland has induced growth rather than a simple reorganization of the economy in the treated municipalities. We investigate the potential channels behind these results in Section 7.2.

Figure 4: Cumulative effect j years before/after access (<15 km) - Details



7 Robustness Checks & Extensions

7.1 Robustness Checks

7.1.1 Variation of treatment and control groups

We start by simply running our main regressions using 10 km as the distance defining the treatment group. As described before, this is too restrictive in our context (see Table 7). The results are almost identical, as shown in Table 21 in Appendix C.3 This is true for all variables except for relocations. For those two, the coefficients change, but we still do not find any conclusive results.

In Table 2, we include rural non-connected municipalities in the control group. Instead of having only treated municipalities in our sample, we also include municipalities that have never been treated. More specifically, municipalities with a distance ranging from 15 km to 30 km from a highway access are also included in the control group. We find close results. The number of firms accumulates from period 0 up to period 8 for specifications (1). Specification (2) has a similar trend compared to the results in the main analysis. There are two main differences overall. First, there is one positive and statistically significant coefficient in period -2. Second, all coefficients are slightly larger. Including municipalities that we considered non-treated increases the effects. The long-term effect increased from 0.265 to 0.305 using those never-treated municipalities.

Interestingly, as shown in Appendix C.2 Table 14, adding only non-connected municipalities lying 15 to 20 km from the highways in the control group do not change the coefficients compared to the main results. The long-term effects increase from 0.265 to 0.279, while the cumulative effect after 15 years increasing from 0.246 to 0.264. 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 and a cumulative effect after 15 periods of 0.273 (see 15, also in Appendix C.2).

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 Table 1. Interestingly, even if statistically insignificant, we find a lower long-term effect for relocations in the treated municipalities. On the contrary, we observe higher relocations out of the treated group, which means that the larger increase in the stock of firms comes only from births. The only way an increase in stock is possible when we observe fewer firms moving in and more firms dying and relocating out is because the increase in firm creations surpasses the other flow variables.

Table 2: Cumulative effect j years 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	

Note: 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.

7.1.2 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 direct highway access, but located near those with access, experience negative spillovers. If this were the case, the observed positive effects of highways could partially reflect displacement rather than pure growth. Moreover, this would constitute a violation of the SUTVA, which underlies 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 not-yet-treated themselves (Debarsy & Le Gallo, 2025). Specifically, we exclude municipalities that lie 5 km away from a treated municipality, but keep those further away. We refer to those excluded municipalities as "*close-controls*". If negative spillovers were present, we would expect the estimated treatment effects to increase when these close-controls are dropped, since we would be removing negatively affected units from the control group.

However, we find that the estimated effects are remarkably stable as shown in Table 3. Comparing to Table 1, excluding the close-controls not-yet-treated increases the cumulative effect on the number of firms after 15 periods from 0.246 to 0.254. The long-term effects are quasi-identical. This suggests that the observed treatment effects are not driven by negative effects on nearby untreated areas. Similarly, the results for the flow variables remain comparable although we observe decrease in long-term births and deaths when dropping close-controls. We run the same analysis including never-treated municipalities as in the previous section, but excluding close-controls. Once again, we find small variations compared to the results including close-controls, suggesting that there are no spillovers issue with the neighboring untreated municipalities.

In sum, those different results provide indirect evidence that SUTVA holds in our setting, and that the local effects of highways are not driven by negative effects in close-controls nor firm relocations from neighboring areas, but rather by local growth.

Table 3: Cumulative effect j years 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)		(6)	
≤8	-0.133	(0.165)	0.041	(0.059)	-0.034	(0.291)	0.265*	(0.151)	-0.306	(0.213)	0.688**	(0.302)
-7	-0.101	(0.082)	-0.067	(0.099)	-0.052	(0.296)	-0.016	(0.235)	-0.454	(0.284)	0.173	(0.293)
-6	-0.072	(0.070)	0.003	(0.070)	0.383**	(0.188)	0.313**	(0.138)	-0.060	(0.217)	0.254	(0.268)
-5	-0.072	(0.057)	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	(.)	0.000	(.)
-3	0.013	(0.019)	0.010	(0.021)	0.227	(0.167)	0.047	(0.270)	-0.125	(0.216)	0.415*	(0.248)
-2	0.035	(0.037)	0.033	(0.034)	0.131*	(0.079)	0.497***	(0.182)	-0.301	(0.231)	0.362	(0.297)
-1	0.048	(0.050)	0.043	(0.045)	0.225**	(0.109)	0.141	(0.173)	-0.350*	(0.184)	0.481*	(0.252)
+0	0.020	(0.053)	0.071	(0.052)	0.213**	(0.108)	0.115	(0.153)	0.163	(0.234)	0.274	(0.267)
+1	0.049	(0.065)	0.096	(0.066)	0.299**	(0.124)	0.292	(0.188)	-0.171	(0.208)	-0.050	(0.270)
+2	0.062	(0.075)	0.121	(0.077)	0.381***	(0.104)	0.240	(0.167)	-0.285	(0.190)	0.059	(0.267)
+3	0.082	(0.088)	0.141	(0.088)	0.467***	(0.116)	0.316**	(0.160)	-0.394*	(0.215)	0.364	(0.250)
+4	0.098	(0.098)	0.156	(0.095)	0.400***	(0.146)	0.638**	(0.255)	-0.331*	(0.196)	0.265	(0.271)
+5	0.107	(0.110)	0.176*	(0.102)	0.321***	(0.110)	0.600*	(0.334)	-0.319	(0.208)	0.336	(0.339)
+6	0.127	(0.122)	0.194*	(0.108)	0.455***	(0.139)	0.914***	(0.332)	-0.156	(0.186)	0.494*	(0.292)
+7	0.122	(0.131)	0.186*	(0.099)	0.386**	(0.188)	0.445*	(0.245)	-0.323	(0.220)	0.295	(0.260)
+8	0.123	(0.144)	0.184**	(0.093)	0.418**	(0.204)	0.510*	(0.266)	-0.275	(0.213)	0.365	(0.259)
+9	0.112	(0.157)	0.173**	(0.088)	0.392*	(0.203)	0.482**	(0.196)	-0.140	(0.223)	0.237	(0.284)
+10	0.109	(0.170)	0.168*	(0.086)	0.400*	(0.210)	0.372*	(0.208)	-0.312	(0.216)	0.156	(0.286)
+11	0.117	(0.180)	0.176**	(0.089)	0.413**	(0.210)	0.382*	(0.221)	-0.351	(0.237)	0.046	(0.306)
+12	0.120	(0.192)	0.185**	(0.094)	0.428**	(0.217)	0.488**	(0.228)	-0.218	(0.221)	0.028	(0.299)
+13	0.135	(0.200)	0.202**	(0.101)	0.452**	(0.192)	0.505**	(0.228)	-0.231	(0.217)	0.207	(0.314)
+14	0.150	(0.212)	0.219**	(0.108)	0.495**	(0.204)	0.527**	(0.242)	-0.180	(0.213)	0.334	(0.348)
≥15	0.172	(0.240)	0.254**	(0.121)	0.548***	(0.201)	0.680***	(0.255)	-0.206	(0.223)	0.099	(0.357)
Long-term effect	0.258	(0.325)	0.268*	(0.142)	0.443***	(0.162)	0.558*	(0.293)	-0.143	(0.241)	-0.040	(0.438)
# Observations	42702		42559		41772		37237		34320		32345	
# Municipalities	1041		1039		1010		921		825		794	
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	

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

7.1.3 Heterogeneous Difference-in-Differences (Wooldridge, 2021, 2023)

We now turn to the results obtained after allowing the treatment effects to differ across cohort and time. Given the data requirements, we are only able to replicate the regressions that include municipalities located between 15 km and 30 km away from highway accesses. Therefore, we need to compare the results with those obtained in Table 2, specification (1). The results for the stock of firms are of comparable magnitude, as shown in Table 4. This suggests that heterogeneity does not play a major role in our results in Section 6.1 and our setting.

When using our flow variables, we have to compare the results to Table 13 in Appendix C.1. We did not find any effect on the creation of firms in the standard staggered difference-in-differences framework with time and municipality fixed effects. The ETWFE regression finds a positive effect on the creation of firms in the years following highway construction. The increase lasts until 6 years after the road access opened. We encounter into statistical power issues again when estimating the effects on the death of firms. However, if we look at the coefficients only, we do find similar trends and positive values as well. Columns (4) and (5) are noisy as usual, but it seems that some relocations outside of the treated municipalities occur when allowing for heterogeneity across time and cohorts.

7.1.4 Estimation without single-year firms

Given that we constructed the panel data based on cross-sectional data, the digitalization of firm names, and a fuzzy matching algorithm, we may have unmatched observations that should have been regrouped with others. In reality, we can expect most of the newly created firms to exist 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 them. The results are comparable to the ones in the full sample as highlighted in Tables 1. We do find smaller coefficients and long-term effects, but the patterns remain similar. The single-year firms are not driving the results.

7.1.5 Net Birth estimation

As a further robustness check, we create a "*net birth*" variable, which is calculated using the current number of created firms from which we subtract the deaths from the previous period. This is a way to correct the potential inflation of the number of creations in cases where false negatives remain in the panel. If we missed some correct matches, we will have an additional death and birth each. We attempt to solve this issue with this corrected

Table 4: Cumulative effect j years 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	

Note: 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 *$p < 0.10$.

Table 5: Cumulative effect j years 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		45671		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	

Note: 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 only in one period before disappearing.

outcome variable. The results differ in terms of the coefficient, but the overall trend is very similar. The effects differ in periods 5 to 7 and 13 to 14. We find an effect that was not present in period 9. Potential false negatives in our matching procedure do not drive our main results. Note that, using our "*net birth*" variable, we might have corrected for successive deaths and births that are perfectly right.

Table 6: Cumulative effect j years before/after access (<15 km) - Net Birth

	Net Birth			
	(1)		(2)	
≤8	-0.547*	(0.328)	-0.023	(0.349)
-7	-0.351	(0.232)	0.021	(0.373)
-6	-0.385	(0.254)	0.353	(0.236)
-5	-0.387*	(0.214)	0.145	(0.280)
-4	0.000	(.)	0.000	(.)
-3	-0.386*	(0.232)	0.234	(0.194)
-2	-0.357***	(0.115)	0.155	(0.155)
-1	-0.248*	(0.128)	0.230	(0.169)
+0	-0.227	(0.146)	0.300**	(0.151)
+1	-0.170*	(0.102)	0.387**	(0.191)
+2	-0.149	(0.161)	0.491***	(0.175)
+3	-0.053	(0.230)	0.616***	(0.207)
+4	-0.096	(0.108)	0.552**	(0.224)
+5	-0.352***	(0.109)	0.288	(0.265)
+6	-0.432**	(0.168)	0.309	(0.302)
+7	-0.129	(0.150)	0.519	(0.343)
+8	-0.059	(0.174)	0.593*	(0.360)
+9	-0.267*	(0.136)	0.478*	(0.283)
+10	-0.176	(0.144)	0.553	(0.338)
+11	-0.147	(0.169)	0.579	(0.393)
+12	-0.289**	(0.143)	0.481	(0.329)
+13	-0.196	(0.152)	0.567	(0.359)
+14	-0.106	(0.188)	0.684	(0.421)
≥15	-0.173	(0.171)	0.690*	(0.397)
Long-term effect	0.061	(0.227)	0.536*	(0.324)
# Observations	45056		44624	
# Municipalities	1024		1022	
Year FE	Yes		Yes	
Municipality FE	Yes		Yes	
Municipal time trends	No		Yes	

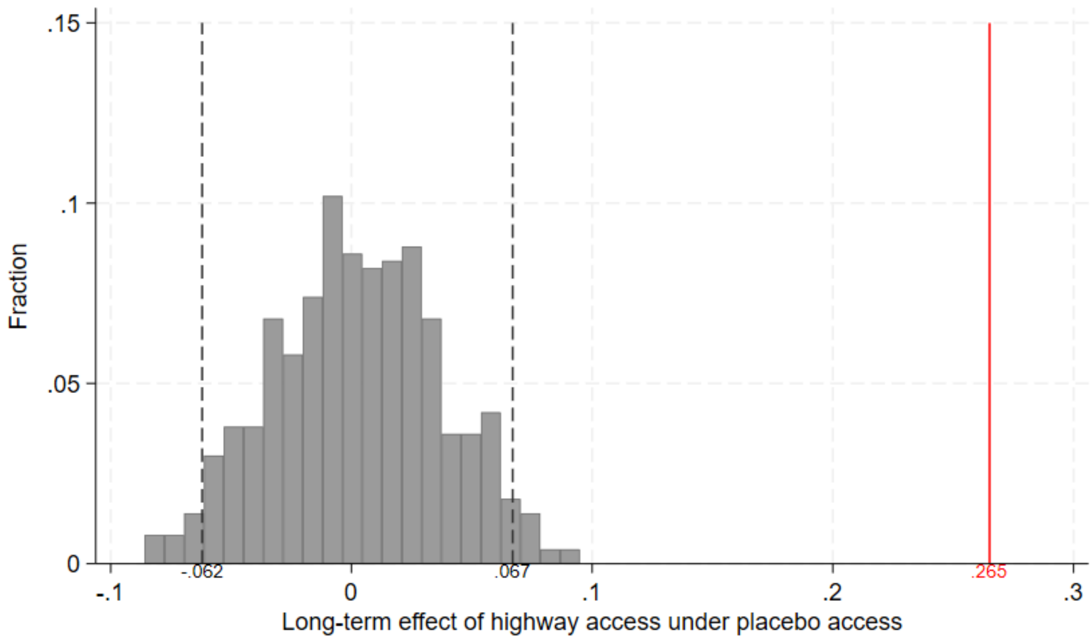
Note: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 15 km of an access. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$. We exclude observations appearing only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

7.1.6 Placebos - Randomization of treatment timing

As a final robustness check, we conducted placebo tests in which we randomized the treatment date for municipalities within 15 km of an access. Regarding the stock, we use specification (2) of our baseline results and randomize treatment timing 1000 times. The dependent variable is the number of firms at the municipal level. The red line shows the long-term coefficient found in Table 1 specification (2). Both dashed lines show the coefficients for which there is statistical significance at the 5% level. As Figure 5 shows, the placebo coefficients are around four times smaller compared to our estimated coefficient.

We proceed similarly with the flow variables for which we found an effect: the births and deaths of firms. We randomized treatment date 500 times in this case, due to the computational duration. The resulting figures are in Appendix C.5. They show that, again, the long-term effect when using the real treatment timing is way larger than all the randomized treatments. The placebo tests suggest that our results are not the result of chance.

Figure 5: The long-term effect on the number of firms - Placebo tests



Note: Resulting histogram from our placebo tests where we randomized 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, specification (2).

7.2 Extensions

7.2.1 Distance to access, to urban center, and interactions

First, we show that the effects are spatially limited and decay as the distance to the access increases. In terms of the stock of firms, Table 7 suggests that the most attractive distance is between 5 km and 10 km from a highway entry, with 10 km to 15 km also attracting firms. The distance ranging from 0 to 5 km does attract firms, but we do not find any statistically significant effect. The positive effect lasts only up to 15 km. More interestingly, column (2) shows that the impact on firm creation is positive in all distance bands, except between 0 and 5 km from the access. Column (3) highlights the large increase in the death of firms concentrated near the highways, especially in the 5 to 10 km distance band. As expected, we do not find any results when looking at the movements of existing firms.

Table 7: Heterogeneity - Distance to access

	Stock (1)		Births (2)		Deaths (3)		Relocation In (4)		Relocation Out (5)	
Long-term effect (0-5 km)	0.222	(0.212)	0.295	(0.306)	0.614	(0.396)	-0.763	(0.665)	-0.103	(0.582)
Long-term effect (5-10 km)	0.386**	(0.173)	0.737***	(0.185)	1.052***	(0.317)	-0.303	(0.317)	-0.212	(0.515)
Long-term effect (10-15 km)	0.220*	(0.131)	0.532***	(0.148)	0.210	(0.262)	0.084	(0.290)	0.119	(0.383)
Long-term effect (15-20 km)	0.049	(0.144)	0.458***	(0.169)	-0.102	(0.308)	-0.399	(0.253)	-0.486	(0.382)
# Observations	53332		51982		46913		42636		40635	
# Municipalities	1214		1189		1091		969		945	

Note: Include municipality fixed effects, time fixed effects, and municipal time trends. 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 *$p < 0.10$.

Next, we investigate the channels behind the increase in both births and deaths of firms. To do so, we interact our access variables with a dummy variable indicating if the treated municipality is less than 20 km away from an urban center. Table 8 shows that the increase in the number of firms is driven by the municipalities distant from the cities. This is confirmed by the results in columns (2) and (3). The creation of firms also occurs in municipalities located more than 20 km away from the urban centers. We do not observe any effect within a distance of 20 km, suggesting that firms tend to avoid locating themselves near urban centers when they are established. Regarding deaths, they are happening in both cases. The coefficient is larger closer to urban centers.

Those results are in line with the core-periphery theory, which predicts that economic activities will be concentrated in central locations as remote places become connected to centers. Highways provide a competitive advantage to rural municipalities with access compared to those without access. At the same time, high-speed roads reduce commuting time between large cities and reduce transportation costs in cities. Decreasing transportation costs expand the market reach of firms in cities, which are usually more productive. Firms located further away now face competition from those in the city centers. Table 8 suggests that firms close to the centers are dying way more, while no firms are getting created in those areas. For municipalities more than 20 km away from the centers, we observe both an increase in births and deaths. Deaths can result from both increased local competition and competition from city centers. The smaller coefficient suggests that the effect of competing with central firms is weaker the further municipalities are from cities.

We also interact our access variables with a variable indicating if the municipality has 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 statistical significance. This is surprising, as we could have expected firms to position themselves to benefit from both the road and rail networks.

When we look at the flows behind this stock effect, we can see that the effects on the creation of firms 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. This puzzling combination can only be explained by the difference in the initial levels of variables in each case.

Table 8: Heterogeneity - Presence of railways & Distance to urban centers

	Stock		Births		Deaths		Relocation In		Relocation Out	
	(1)		(2)		(3)		(4)		(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)
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	

Note: Include municipality fixed effects, time fixed effects, and municipal time trends. 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.

7.2.2 Size of firms and distribution by size

In this section, we investigate the heterogeneity of the effects depending on the size of the firms. We use the number of directors sitting on the board of directors and the nominal capital as proxies for firms' size. Our results seem to be driven by relatively small firms. When we observe the effects depending on the nominal capital percentiles, as measured in the national capital size distribution, we find effects in the smaller firms. We find large increases in the number of firms in the bottom 50th percentile, as well as in the 50-75th percentiles. We do not find any effects in the different groups we formed within the top 25% of the nominal capital distribution.

When we use the size of the board of directors, we also find that the increasing number of firms is driven by small-sized companies, as measured by board size. We find results suggesting positive long-term effects for the firm with one, two, and three board members. The number of firms with larger boards is not affected by the opening of highway access. However, the results are far less conclusive given the unstable dynamic effects we measure.

Table 9: Cumulative effect on the number of firms in the bottom 50th, 50th-75th, 75th-90th, 90th, 90th-99th, and 99th percentile of national capital size distribution j years before/after access (<15 km)

	(1) Bottom 50th		(2) 50-75th		(3) 75-90th		(4) Top 10%		(5) 90-99th		(6) Top 1%	
≤-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	

Note: 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 *$p < 0.10$.

Table 10: Cumulative effect on the number of firms with different board sizes j years before/after access (<15 km)

	(1) 1 board member		(2) 2 board members		(3) 3 board members		(4) 4 board members		(5) 5 board members		(6) 6+ board members	
≤-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	

Note: 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 *$p < 0.10$.

8 Conclusion

This paper presents new evidence on the impact of large-scale transportation infrastructure on firm dynamics at the local level. Exploiting the staggered development of Switzerland's national highway network and using a novel historical panel covering all limited companies from 1960 to 2003, we find that access to the highway network significantly increases the number of firms in treated municipalities. This effect persists over time and is driven by higher firm entry and exit rates, rather than by the relocation of existing firms.

Our findings reveal that highway access generates a significant and persistent increase in the number of firms in treated municipalities. This effect is primarily driven by an increase in firm creations, which more than offsets the observed increase in firm exits. In contrast, we find no conclusive evidence that highway access leads to a systematic relocation of existing firms.

Through a series of robustness checks and extensions, we can strengthen our findings and provide a more comprehensive explanation for the simultaneous observation of more births and deaths. Firstly, we show that the effects are coming mostly from small-sized firms, in terms of both nominal capital and size of the board of directors. Secondly, the distance between treated municipalities and city centers plays a role in firm dynamics. The increase in the number of firms comes from municipalities located more than 20 km away from cities. Firms in municipalities within 20 km suffer from an increase in competition, leading to a larger increase in deaths. At the same time, firms tend to avoid establishing themselves near urban centers. Finally, we find results suggesting that firms contribute to urban sprawl.

We obtain similar results when we run the analysis excluding firms that existed for only one year. Correcting for potential false matches in the panel construction (using net births instead of births) does not change the results either. The results are also comparable when using 10 km as our treatment definition. Finally, our placebo tests show that our long-term effects are way larger than when we randomize treatment timing.

These results suggest that improved market accessibility fosters net economic growth rather than merely redistributing existing activity across space. Even in a country with extensive existing rail networks and a well-developed road network, 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 carry broader implications for developed countries considering infrastructure expansions or improvements.

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Appendix

A Documents & Data preparation

A.1 Raw Data & Panel Construction

We have digitalized all existing editions of the "*Verzeichnis der Verwaltungsräte Schweizerischer Aktiengesellschaften / Répertoire des administrateurs des sociétés anonymes suisses*" published between 1934 and 2003. One example can be found in Figure 6 below. These collections are centered around individual directors. They are based on the 27 commercial registries and list every person who holds a mandate on the board of directors of a limited company (AG) in a specific year, including information on their name, address, function on the board, firm name, firm location, and nominal capital. We have scanned these collections and collaborated with Sugarcube Sàrl (specializing in the digitalization of complex documents) to establish a structured database. The raw database contains around 4.6 million individual and 4.2 million firm entries. The dataset comprises yearly cross-sections for the years 1934, 1943, 1956, 1960, 1962-1966, 1969, 1972, 1975, and 1979-2003. From this extensive data, we can aggregate the number of firms at the municipal level and characterize the local firm structure through size indicators, such as nominal capital or the number of directors.

As the structure of the board directories focuses on individuals, firm information is not unique. Several individuals can sit on a board. Thus, we can observe multiple entries for the same firm in a specific year. Therefore, we need to deduplicate and connect firm entries: i) identify unique firms in any given year (given potential OCR errors and reporting differences in the original publications), deduplicate and consolidate on an annual basis, and ii) identify and link unique firms over the yearly cross-sections. To achieve this, we employed a matching approach implemented in sequences. We start by standardizing the firm names. This allows us to find new perfect matches within municipalities. Although Sugarcube Sàrl already merged the firms with the same name in the data they provided us, we might have new exact matches due to the standardization of characters and specific terms. We restrict the matching within municipalities, as the regulations of the Swiss Commercial Registry do not allow two firms to have the same name in Switzerland. We then allow for different firm names to be matched together (using a fuzzy matching algorithm).

We rely on the Stata "*strgroup*" command to evaluate if two firm names are comparable enough to match them. We start with a threshold of 10% and re-implement the algorithm using a 20% threshold. The lower the threshold, the more comparable the matched observations. The Stata "*strgroup*" command matches strings based on their Levenshtein edit distance. Every step is manually evaluated by two student assistants. They reviewed at all the resulting matches and determined whether they were correctly matched or not. Based on this information, we can measure the number of firms and distinguish their size (in terms of nominal capital and the number of directors) at the municipal level.

Moreover, we can also establish changes in firm size and relocation decisions at the firm level. This provides the possibility of establishing measures of firm sorting and concentration or dispersion at the municipal as well as varying regional levels (flexible aggregation of municipal information). This, however, requires clean panel structures in our firm data. We rely on what we call the "*flow approach*". It consists in cutting the tasks in several smaller steps that solve part of the panel construction issues.

We first identify the cases where we are highly confident that the firm is the same across multiple periods. To do so, we create subsamples of successive years (1934 and 1943, 1943 and 1960, 1960 and 1962, etc.). Within those subsamples, we start by matching firms with the same name and limit the options to firms in the same municipality. If observations have the same company names and are in the same municipality, they receive the same panel ID. We apply the same matching procedure to all pairs of periods. We now regroup all the different subsamples together and attribute the same panel ID in a cascade. If an observation A in 1934 is matched with an observation B in 1943, which is also matched with an observation C in 1960, then we must match A and C.

As those series are very strictly built, we do not require manual examination of the matches. We are confident that the observations that are not the first or last appearance in a group are correctly matched and will no longer be helpful. Thus, we separate them from the next matching steps. Only the observations at the end or the start of each series can be further matched (not starting in 1934 or ending in 2003, our first and last periods, respectively).

We then proceed similarly, allowing the company names to differ by 10% and still be matched (within municipalities). We run the matching procedure on the subsample of first and last observations within a panel ID (as the middle observations come from perfect matches) to extend the existing series. We refer to the observations in this subsample as the "*potential matches*". Single observations, those that have not been matched yet, are also included. The subsample size allows us to run the matching procedure over all periods directly. Our two assistants evaluated the resulting subsample of matches. We then increase the threshold to 20% and we manually evaluate the resulting matches. After this second step, we now have a panel of firms with an ID for non-moving firms over time (as we restricted the matching within municipalities). Every new step reduces the subsample of potential matches thanks to the extension of our existing panel IDs (or when we link to existing IDs together).

Next, we have to allow for matching across municipalities. When we drop the within-municipality restriction, we encounter computational issues. Each observation has to be compared to all the others in the subsample of potential matches. We attempted to implement the matching using brute force on our servers, but they crashed due to the size of the task. We turned again to a "*flow approach*" where we start with stricter matching restrictions to reduce the "*potential matches*". Again, we create subsamples of successive years (1934 and 1943, 1943 and 1960, 1960 and 1962, etc.), and run the matching algorithm without geographical restriction. As usual, we start by regrouping observations with the same firm names across successive years using the "*potential matches*". Again, we then increase the matching thresholds to 10% and 20%. Each additional step reduced the size of the potential matches subsample and improved the attribution of our panel IDs.

The matching algorithm we chose measures the distance based on character comparisons. This is why, in our last round of matching, we implemented a token-based matching algorithm using the Stata command "*matchit*". Token-based algorithms are performing way better when the orders of words are switched. It was not an issue in cross-sections, but it seems that the order in which different parts of similar firm names are written can change across periods. We had to run the matching over successive periods again. Every resulting matched sample was evaluated manually.

The final firm-level panel is composed of :

- 4 million individual observations
- Around 430k distinct AGs, which are limited companies and stock corporations, publicly listed or not

Which can then be aggregated using the geographical information we have. For this project, we aggregate our firm-level data at the municipal level using the 2012 state of municipalities data. We use 2012 to employ the same universe of municipalities as Fretz et al. (2021), allowing us to utilize their access data.

Figure 6: Example : *"Répertoire des administrateurs des sociétés anonymes suisses"*

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**

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

A.2 Dealing with missing years in the panel

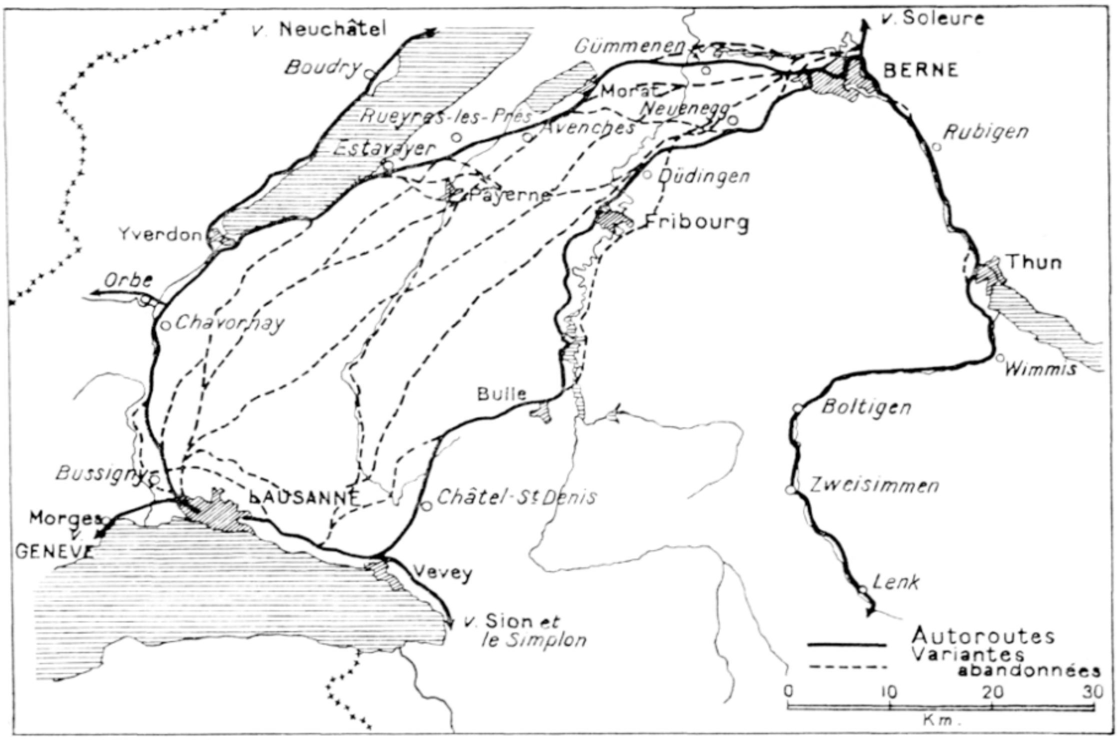
As mentioned several times, we have missing data in some years prior to the 1980s. Regarding the stock, we use a linear approximation as the number of consecutive missing years does not exceed three years. The number of firms is a stable number that, in almost all cases, steadily grows over time. Flow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

The number of created firms generally increases over time. We use this fact to determine a rule for attributing the creations of firms over missing years. As an example, we can use the number we just used. We observe 10 firms created in period t after a gap of three years (the longest over the years after 1960). We start by dividing those 10 creations by 2. We then attribute 5 to period t, and 5 to period t-1. We redo the same and divide the 5 creations in period t-1 by 2 before attributing each half to period 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 creation to later periods. Note that firms that are created but also close during a gap are never observed in our data. The numbers of creations and deaths could be higher without gaps. The same procedure is applied to fill gaps in all our "*flow variables*".

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 & Turner (2012) or Mahajan (2024), for example. We quickly found out that such plans did not exist consistently in the Swiss context. For example, we did found a map (see Figure 7) 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 7: Example: All different options considered to connect Bern and Lausanne



Reference: Piveteau (1964)

B Descriptive Statistics

B.1 Summary Statistics

Table 11 presents the summary statistics of the variables used in this research. We present the means and standard deviations (in parentheses) for the entire sample across all years. We split the metrics by type of municipality.

Table 11: 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

Source: Population data (Swiss Federal Statistical Office 2000, 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.

B.2 Stylized Facts

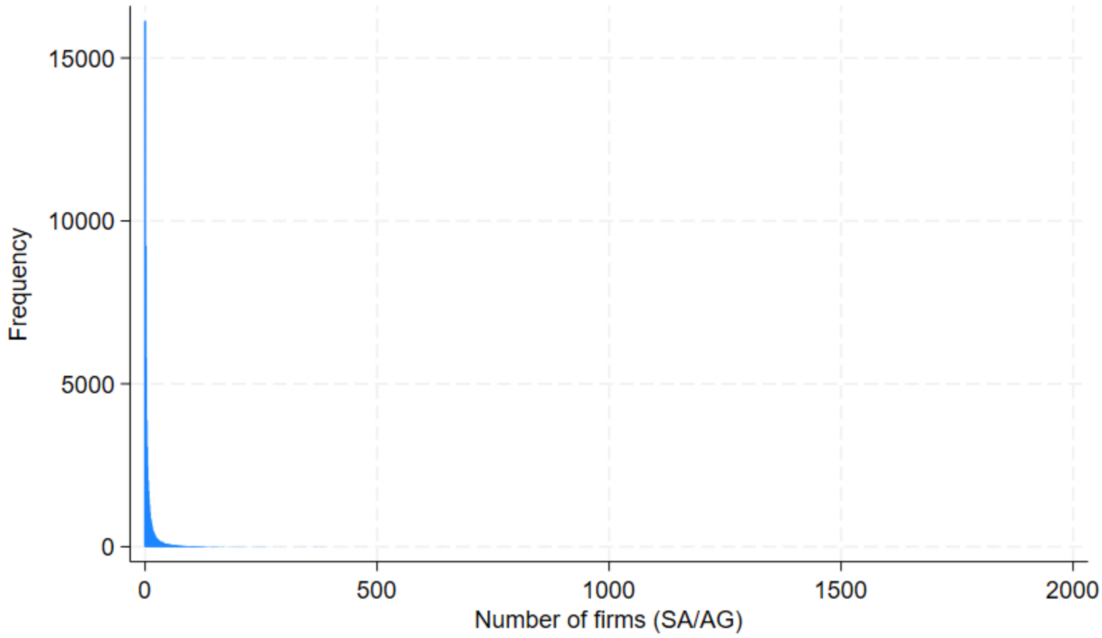
We report the evolution of our main outcome variables by type of municipality in Table 12. This table compares the average fraction of firm and worker variables in non-urban connected, non-urban non-connected, and urban municipalities at the beginning and after the highway construction.

Table 12: 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)

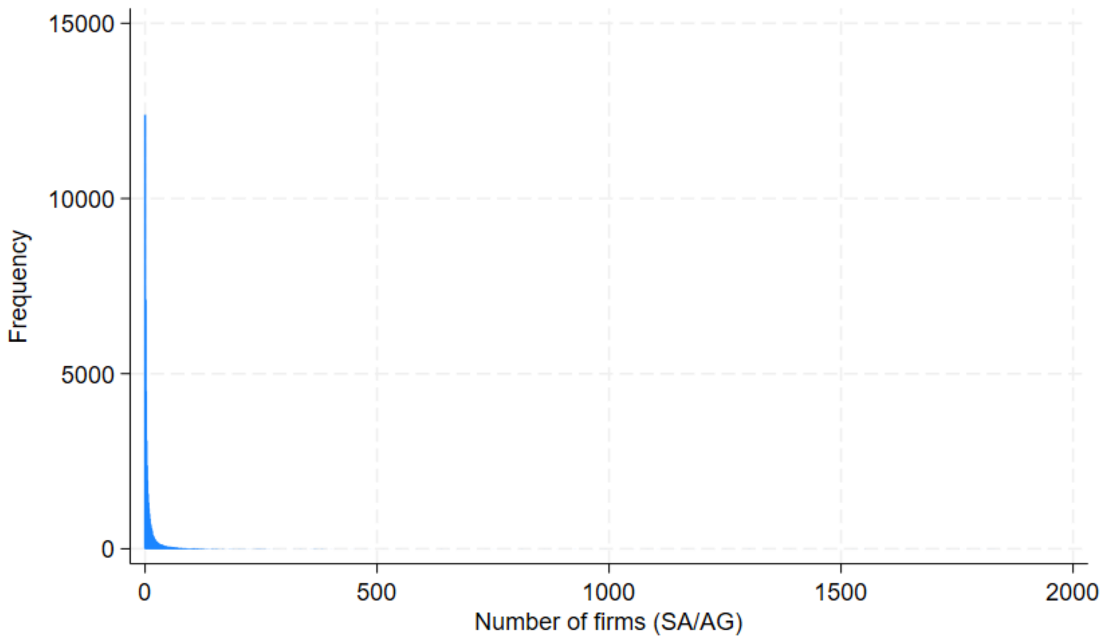
B.3 Frequency Distributions

Figure 8: Frequency distribution of the number of firms - Non-urban municipalities



Note: 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 2, this corresponds to (2) and (3).

Figure 9: Frequency distribution of the number of firms - Non-urban connected municipalities



Note: 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 2, this corresponds to (2), i.e. the non-urban connected municipalities.

C Additional results

C.1 Main analysis - Flow Variables, all specifications

Table 13 provides the complete results for our flow variables. It also includes the regressions without municipal time trends.

Table 13: Cumulative effect j years 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	

Note: 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 *$p < 0.10$.

C.2 Inclusion of non-connected municipalities

Table 14: Cumulative effect j years 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	

Note: 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 15: Cumulative effect j years 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	

Note: 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 *$p < 0.10$.

C.3 Robustness checks and extensions

C.3.1 Spillovers - Exclusion of "close-controls"

Table 16: Cumulative effect j years before/after access (<15 km), including never treated between 15 km and 20 km, excluding municipalities close to treated units (<5 km)

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.106	(0.141)	0.031	(0.058)	-0.027	(0.292)	0.247	(0.151)	-0.327	(0.211)	0.623**	(0.316)
-7	-0.094	(0.077)	-0.069	(0.100)	-0.043	(0.299)	-0.017	(0.224)	-0.484*	(0.286)	0.140	(0.303)
-6	-0.068	(0.065)	0.002	(0.070)	0.397**	(0.184)	0.281**	(0.137)	-0.071	(0.217)	0.212	(0.271)
-5	-0.069	(0.056)	0.009	(0.056)	0.048	(0.216)	0.252*	(0.145)	0.007	(0.211)	0.133	(0.272)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.012	(0.018)	0.012	(0.020)	0.244	(0.167)	0.041	(0.262)	-0.135	(0.216)	0.336	(0.253)
-2	0.032	(0.033)	0.036	(0.035)	0.143*	(0.080)	0.499***	(0.182)	-0.328	(0.233)	0.343	(0.296)
-1	0.044	(0.044)	0.048	(0.045)	0.246**	(0.110)	0.120	(0.165)	-0.365**	(0.179)	0.434*	(0.245)
+0	0.013	(0.041)	0.080	(0.053)	0.227**	(0.110)	0.106	(0.150)	0.155	(0.229)	0.234	(0.266)
+1	0.040	(0.050)	0.106	(0.067)	0.315**	(0.123)	0.290	(0.190)	-0.168	(0.198)	-0.065	(0.272)
+2	0.052	(0.059)	0.133*	(0.078)	0.395***	(0.104)	0.241	(0.169)	-0.291	(0.186)	-0.002	(0.292)
+3	0.070	(0.070)	0.155*	(0.089)	0.485***	(0.117)	0.313**	(0.158)	-0.392*	(0.208)	0.337	(0.248)
+4	0.083	(0.078)	0.170*	(0.097)	0.413***	(0.145)	0.640**	(0.262)	-0.329*	(0.190)	0.244	(0.275)
+5	0.090	(0.087)	0.192*	(0.104)	0.334***	(0.108)	0.597*	(0.344)	-0.317	(0.198)	0.315	(0.342)
+6	0.109	(0.097)	0.212*	(0.110)	0.474***	(0.141)	0.918***	(0.340)	-0.154	(0.179)	0.490*	(0.288)
+7	0.102	(0.101)	0.205**	(0.100)	0.404**	(0.189)	0.457*	(0.247)	-0.316	(0.215)	0.298	(0.254)
+8	0.100	(0.111)	0.204**	(0.094)	0.439**	(0.207)	0.514*	(0.269)	-0.268	(0.204)	0.362	(0.253)
+9	0.087	(0.121)	0.194**	(0.089)	0.407**	(0.203)	0.494**	(0.194)	-0.131	(0.217)	0.243	(0.276)
+10	0.080	(0.132)	0.189**	(0.087)	0.415**	(0.209)	0.384*	(0.204)	-0.305	(0.206)	0.162	(0.277)
+11	0.085	(0.141)	0.197**	(0.090)	0.427**	(0.209)	0.394*	(0.219)	-0.344	(0.230)	0.052	(0.300)
+12	0.085	(0.150)	0.207**	(0.095)	0.442**	(0.217)	0.500**	(0.227)	-0.214	(0.215)	0.037	(0.291)
+13	0.097	(0.155)	0.223**	(0.102)	0.465**	(0.191)	0.517**	(0.228)	-0.217	(0.212)	0.225	(0.305)
+14	0.109	(0.163)	0.240**	(0.109)	0.511**	(0.204)	0.540**	(0.241)	-0.169	(0.209)	0.352	(0.339)
≥15	0.105	(0.173)	0.276**	(0.121)	0.562***	(0.200)	0.695***	(0.254)	-0.196	(0.218)	0.123	(0.347)
Long-term effect	0.181	(0.244)	0.288**	(0.141)	0.453***	(0.159)	0.579**	(0.294)	-0.118	(0.237)	0.005	(0.433)
# Observations	45265		45092		44213		39155		35703		33897	
# Municipalities	1154		1150		1117		991		870		845	
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	

Note: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 20 km of an access. We include rural (never-connected) municipalities with an access between 15 km and 20 km in the control group, but we exclude control municipalities located within 5 km from treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***p < 0.01, **p < 0.05, and *p < 0.10.

Table 17: Cumulative effect j years before/after access (<15 km), including never treated between 15 km and 25 km, excluding municipalities close to treated units (<5 km)

	Number of Firms (1)		Number of Firms (2)		Births (3)		Deaths (4)		Relocations In (5)		Relocations Out (6)	
≤8	-0.105	(0.119)	0.025	(0.056)	-0.017	(0.288)	0.246	(0.151)	-0.328	(0.208)	0.561*	(0.322)
-7	-0.094	(0.073)	-0.070	(0.100)	-0.036	(0.298)	-0.014	(0.216)	-0.491*	(0.280)	0.101	(0.305)
-6	-0.067	(0.062)	0.002	(0.070)	0.405**	(0.180)	0.302**	(0.132)	-0.075	(0.219)	0.181	(0.275)
-5	-0.069	(0.055)	0.009	(0.056)	0.048	(0.216)	0.262*	(0.142)	-0.004	(0.216)	0.105	(0.273)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.012	(0.017)	0.012	(0.020)	0.242	(0.166)	0.069	(0.242)	-0.155	(0.214)	0.332	(0.255)
-2	0.032	(0.030)	0.038	(0.035)	0.145*	(0.079)	0.507***	(0.190)	-0.349	(0.239)	0.333	(0.305)
-1	0.045	(0.040)	0.051	(0.046)	0.254**	(0.108)	0.151	(0.168)	-0.378**	(0.180)	0.432*	(0.252)
+0	0.014	(0.033)	0.084	(0.055)	0.228**	(0.107)	0.131	(0.152)	0.131	(0.231)	0.237	(0.274)
+1	0.041	(0.040)	0.111	(0.069)	0.320***	(0.124)	0.301	(0.203)	-0.198	(0.197)	-0.044	(0.280)
+2	0.053	(0.049)	0.138*	(0.080)	0.394***	(0.104)	0.280	(0.176)	-0.312*	(0.184)	0.024	(0.301)
+3	0.071	(0.058)	0.161*	(0.092)	0.490***	(0.122)	0.342**	(0.159)	-0.421**	(0.204)	0.361	(0.257)
+4	0.085	(0.064)	0.177*	(0.099)	0.414***	(0.145)	0.669**	(0.283)	-0.361*	(0.186)	0.277	(0.284)
+5	0.092	(0.071)	0.200*	(0.106)	0.337***	(0.111)	0.630*	(0.365)	-0.349*	(0.200)	0.349	(0.354)
+6	0.111	(0.078)	0.220*	(0.113)	0.475***	(0.146)	0.954***	(0.364)	-0.187	(0.177)	0.529*	(0.297)
+7	0.103	(0.077)	0.214**	(0.103)	0.401**	(0.194)	0.487*	(0.262)	-0.350*	(0.212)	0.346	(0.261)
+8	0.101	(0.084)	0.214**	(0.097)	0.436**	(0.211)	0.555**	(0.282)	-0.310	(0.201)	0.413	(0.259)
+9	0.088	(0.093)	0.204**	(0.092)	0.402*	(0.207)	0.528***	(0.202)	-0.165	(0.213)	0.294	(0.283)
+10	0.081	(0.102)	0.200**	(0.089)	0.409*	(0.215)	0.422**	(0.209)	-0.347*	(0.202)	0.218	(0.283)
+11	0.086	(0.110)	0.208**	(0.092)	0.423**	(0.214)	0.434*	(0.229)	-0.379	(0.231)	0.117	(0.313)
+12	0.086	(0.117)	0.217**	(0.097)	0.431*	(0.220)	0.537**	(0.236)	-0.250	(0.209)	0.110	(0.297)
+13	0.097	(0.119)	0.234**	(0.103)	0.451**	(0.195)	0.555**	(0.237)	-0.253	(0.205)	0.309	(0.310)
+14	0.109	(0.125)	0.251**	(0.111)	0.500**	(0.207)	0.577**	(0.251)	-0.205	(0.202)	0.435	(0.343)
≥15	0.105	(0.125)	0.287**	(0.122)	0.550***	(0.203)	0.736***	(0.261)	-0.232	(0.214)	0.221	(0.352)
Long-term effect	0.183	(0.178)	0.297**	(0.141)	0.444***	(0.161)	0.607**	(0.299)	-0.146	(0.236)	0.108	(0.427)
# Observations	47941		47768		46889		41493		37653		35633	
# Municipalities	1219		1215		1182		1046		915		886	
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	

Note: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 25 km of an access. We include rural (never-connected) municipalities with an access between 15 km and 25 km in the control group, but we exclude control municipalities located within 5 km from treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

Table 18: Cumulative effect j years 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				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.108	(0.111)	0.010	(0.056)	-0.013	(0.296)	0.223	(0.150)	-0.317	(0.208)	0.569*	(0.322)
-7	-0.093	(0.071)	-0.073	(0.102)	-0.026	(0.307)	-0.011	(0.219)	-0.470*	(0.280)	0.115	(0.312)
-6	-0.067	(0.059)	-0.000	(0.072)	0.415**	(0.187)	0.301**	(0.131)	-0.058	(0.220)	0.181	(0.279)
-5	-0.067	(0.053)	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	(.)	0.000	(.)
-3	0.013	(0.016)	0.015	(0.020)	0.262	(0.171)	0.094	(0.241)	-0.128	(0.212)	0.340	(0.256)
-2	0.034	(0.030)	0.043	(0.036)	0.164*	(0.086)	0.528***	(0.185)	-0.316	(0.235)	0.339	(0.307)
-1	0.048	(0.039)	0.059	(0.047)	0.275**	(0.114)	0.174	(0.166)	-0.340*	(0.180)	0.437*	(0.253)
+0	0.021	(0.032)	0.097*	(0.056)	0.254**	(0.116)	0.160	(0.151)	0.169	(0.228)	0.237	(0.277)
+1	0.049	(0.039)	0.127*	(0.070)	0.349***	(0.133)	0.339*	(0.201)	-0.169	(0.198)	-0.055	(0.287)
+2	0.062	(0.046)	0.155*	(0.082)	0.418***	(0.113)	0.325*	(0.172)	-0.275	(0.182)	0.029	(0.304)
+3	0.081	(0.055)	0.180*	(0.094)	0.518***	(0.131)	0.390**	(0.158)	-0.384*	(0.203)	0.361	(0.258)
+4	0.095	(0.061)	0.197*	(0.102)	0.440***	(0.152)	0.722**	(0.285)	-0.317*	(0.186)	0.281	(0.288)
+5	0.103	(0.067)	0.222**	(0.109)	0.364***	(0.119)	0.685*	(0.367)	-0.302	(0.196)	0.350	(0.359)
+6	0.123*	(0.073)	0.244**	(0.116)	0.505***	(0.157)	1.013***	(0.368)	-0.143	(0.175)	0.528*	(0.300)
+7	0.115	(0.071)	0.239**	(0.107)	0.431**	(0.204)	0.544**	(0.263)	-0.308	(0.210)	0.344	(0.256)
+8	0.115	(0.076)	0.241**	(0.101)	0.465**	(0.221)	0.616**	(0.281)	-0.271	(0.198)	0.402	(0.258)
+9	0.102	(0.084)	0.232**	(0.096)	0.432**	(0.217)	0.594***	(0.199)	-0.117	(0.211)	0.298	(0.279)
+10	0.096	(0.094)	0.229**	(0.094)	0.437*	(0.226)	0.493**	(0.204)	-0.298	(0.199)	0.216	(0.277)
+11	0.101	(0.101)	0.238**	(0.096)	0.453**	(0.226)	0.505**	(0.222)	-0.327	(0.225)	0.120	(0.310)
+12	0.101	(0.107)	0.248**	(0.102)	0.458**	(0.232)	0.603**	(0.237)	-0.202	(0.206)	0.113	(0.292)
+13	0.112	(0.109)	0.265**	(0.108)	0.479**	(0.206)	0.621***	(0.237)	-0.206	(0.202)	0.307	(0.303)
+14	0.124	(0.115)	0.282**	(0.115)	0.526**	(0.219)	0.646**	(0.252)	-0.159	(0.199)	0.436	(0.337)
≥15	0.121	(0.112)	0.318**	(0.126)	0.572***	(0.214)	0.807***	(0.264)	-0.185	(0.210)	0.216	(0.343)
Long-term effect	0.201	(0.157)	0.328**	(0.144)	0.460***	(0.167)	0.675**	(0.306)	-0.109	(0.228)	0.096	(0.418)
# Observations	50173		50000		49121		43525		38907		36863	
# Municipalities	1271		1267		1234		1094		944		915	
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	

Note: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 30 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 from treated municipalities. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

C.3.2 Urban Sprawl - Firms & Employment

To explain our main findings, we conducted an additional analysis that included all municipalities. We face endogeneity problems in those regressions because we include both centers and suburban municipalities that were targeted by the treatment due to their economic importance.

Table 8 shows how the firm variables were affected by the road construction in treated municipalities, depending on the distance to the 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. Comparing Zürich with Bern, Thun, or Lugano does not make sense. The more interesting results are those for municipalities outside of centers. Similarly to the trends shown in Table 12, 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 away from the centers, we begin to see positive effects on the creation of firms. 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 the municipalities located more than 40 km away, as we find increases in all variables at the same time.

Table 19: Urban Sprawl

Firm outcomes					
	# of Firms (1)	**Creation of Firms (2)**	**Death of Firms (3)**	**Relocation In (4)**	**Relocation Out (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

Note: 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 *$p < 0.10$.

C.3.3 Mean Equality Testing

Although we observe parallel trends in our dynamic estimations, we compare the values of variables for the period 1960-1962 between groups, depending on the timing of the treatment. As we use eventually treated municipalities as controls, we want to test whether early treated ones are comparable to later ones.

Fretz et al. (2021) run a similar exercise using their data, but they can use pre-treatment periods for their mean equality testing. One can refer to their Table Appendix C.2 for the same test using other variables and 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, so we use variables from the periods 1960-1962, before the main openings. We then 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.

Table 20: Testing mean 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 (1)-(5) (p-value)
# Firms (in 1,000)	2.45 (16.92)	3.48 (27.24)	1.73 (4.51)	2.13 (4.55)	1.60 (5.29)	0.47
# Firms (SA/AG) growth rate	9.34 (30.05)	8.41 (28.37)	9.35 (27.98)	11.69 (38.89)	9.34 (26.09)	0.82
Firms' birth	0.69 (6.34)	0.99 (10.17)	0.48 (1.79)	0.60 (1.86)	0.42 (1.99)	0.38
Firms' death	0.06 (0.57)	0.08 (0.88)	0.04 (0.23)	0.05 (0.24)	0.03 (0.20)	0.64
Firms relocating in	0.04 (0.50)	0.05 (0.79)	0.03 (0.16)	0.02 (0.15)	0.03 (0.19)	0.75
Firms relocating out	0.02 (0.30)	0.04 (0.47)	0.01 (0.11)	0.02 (0.15)	0.02 (0.15)	0.37
Total employment 2nd and 3rd sectors (in 1,000)	0.24 (0.47)	0.27 (0.58)	0.22 (0.39)	0.22 (0.41)	0.17 (0.33)	0.17
Share of employment in 2nd sector	0.61 (0.22)	0.62 (0.23)	0.60 (0.22)	0.59 (0.21)	0.61 (0.24)	0.56
Share of employment in 3rd sector	0.39 (0.22)	0.38 (0.24)	0.40 (0.22)	0.41 (0.21)	0.40 (0.24)	0.61
# Municipalities	1082	397	342	194	76	

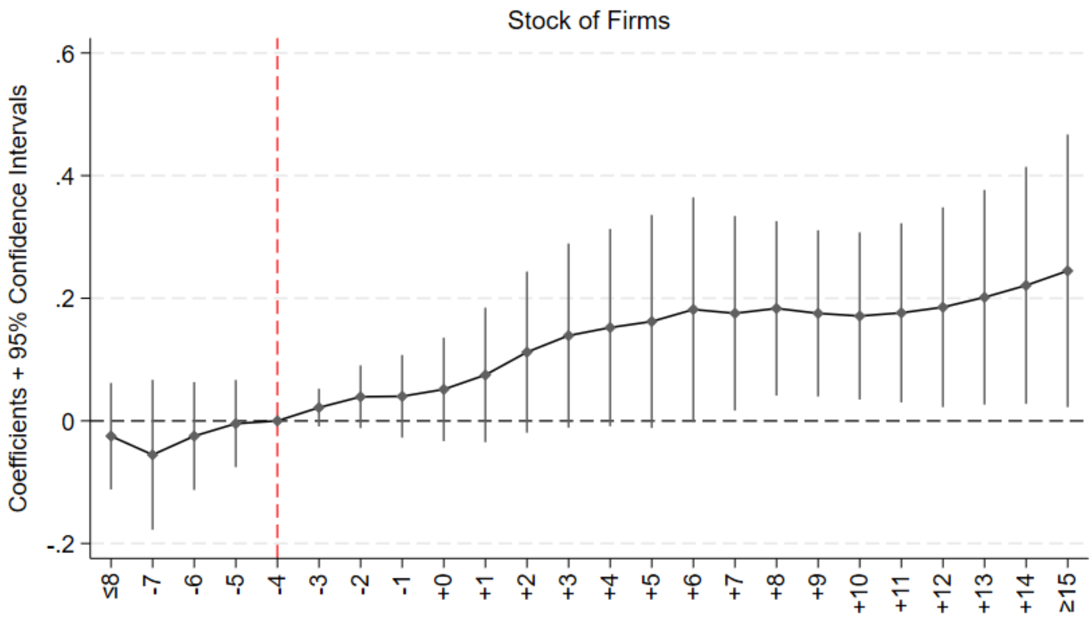
C.4 Results using 10 km treatment group

Table 21: Cumulative effect j years before/after access (<10 km)

	Number of Firms				Births		Deaths		Relocations In		Relocations Out	
	(1)		(2)		(3)		(4)		(5)		(6)	
≤8	-0.240	(0.176)	-0.025	(0.044)	0.036	(0.235)	-0.116	(0.122)	-0.359**	(0.165)	-0.067	(0.176)
-7	-0.109	(0.068)	-0.055	(0.062)	0.051	(0.200)	-0.007	(0.160)	-0.115	(0.174)	-0.386	(0.238)
-6	-0.088	(0.054)	-0.025	(0.045)	0.293*	(0.174)	-0.196	(0.140)	-0.226	(0.195)	-0.066	(0.217)
-5	-0.059	(0.040)	-0.004	(0.036)	0.145	(0.178)	-0.099	(0.109)	-0.126	(0.180)	-0.107	(0.178)
-4	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
-3	0.029*	(0.016)	0.022	(0.016)	0.324**	(0.136)	-0.096	(0.183)	-0.325*	(0.190)	0.003	(0.148)
-2	0.052	(0.033)	0.039	(0.026)	0.184*	(0.104)	0.147	(0.128)	-0.513***	(0.196)	0.107	(0.198)
-1	0.063	(0.045)	0.040	(0.034)	0.148	(0.110)	0.062	(0.115)	-0.553***	(0.168)	0.292	(0.193)
+0	0.079	(0.057)	0.051	(0.043)	0.195**	(0.091)	0.055	(0.124)	-0.043	(0.184)	0.124	(0.204)
+1	0.110	(0.074)	0.075	(0.056)	0.257***	(0.100)	0.284**	(0.141)	-0.123	(0.190)	-0.036	(0.217)
+2	0.139	(0.089)	0.112*	(0.067)	0.386***	(0.088)	0.201	(0.146)	-0.137	(0.163)	0.034	(0.220)
+3	0.171	(0.104)	0.139*	(0.077)	0.441***	(0.098)	0.245*	(0.132)	-0.174	(0.191)	0.218	(0.167)
+4	0.190	(0.116)	0.152*	(0.082)	0.308**	(0.127)	0.510**	(0.216)	-0.144	(0.153)	0.351*	(0.190)
+5	0.202	(0.130)	0.162*	(0.089)	0.278***	(0.097)	0.516*	(0.282)	-0.226	(0.170)	0.238	(0.271)
+6	0.228	(0.142)	0.182*	(0.093)	0.455***	(0.124)	0.741**	(0.303)	-0.158	(0.174)	0.491**	(0.239)
+7	0.228	(0.149)	0.175**	(0.081)	0.355*	(0.182)	0.342**	(0.144)	-0.120	(0.214)	0.313*	(0.173)
+8	0.244	(0.163)	0.183**	(0.073)	0.343*	(0.197)	0.264	(0.180)	0.033	(0.199)	0.392**	(0.180)
+9	0.241	(0.178)	0.175**	(0.069)	0.325	(0.205)	0.283**	(0.131)	-0.002	(0.195)	0.266	(0.186)
+10	0.241	(0.192)	0.171**	(0.070)	0.317	(0.212)	0.283**	(0.143)	-0.251	(0.226)	0.163	(0.196)
+11	0.255	(0.205)	0.176**	(0.075)	0.338	(0.214)	0.264*	(0.140)	-0.419**	(0.210)	0.252	(0.195)
+12	0.257	(0.221)	0.185**	(0.083)	0.310	(0.233)	0.468***	(0.154)	-0.123	(0.224)	0.267	(0.209)
+13	0.274	(0.232)	0.202**	(0.089)	0.387*	(0.202)	0.438***	(0.163)	-0.173	(0.235)	0.363*	(0.217)
+14	0.293	(0.246)	0.221**	(0.099)	0.423**	(0.211)	0.475***	(0.164)	-0.137	(0.240)	0.215	(0.228)
≥15	0.300	(0.281)	0.245**	(0.113)	0.406*	(0.221)	0.610***	(0.176)	-0.164	(0.249)	0.285	(0.215)
Long-term effect	0.403	(0.355)	0.259**	(0.121)	0.293	(0.194)	0.674***	(0.189)	-0.116	(0.250)	0.387*	(0.207)
# Observations	32868		32782		31960		28982		26796		25628	
# Municipalities	747		746		732		674		609		596	
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	

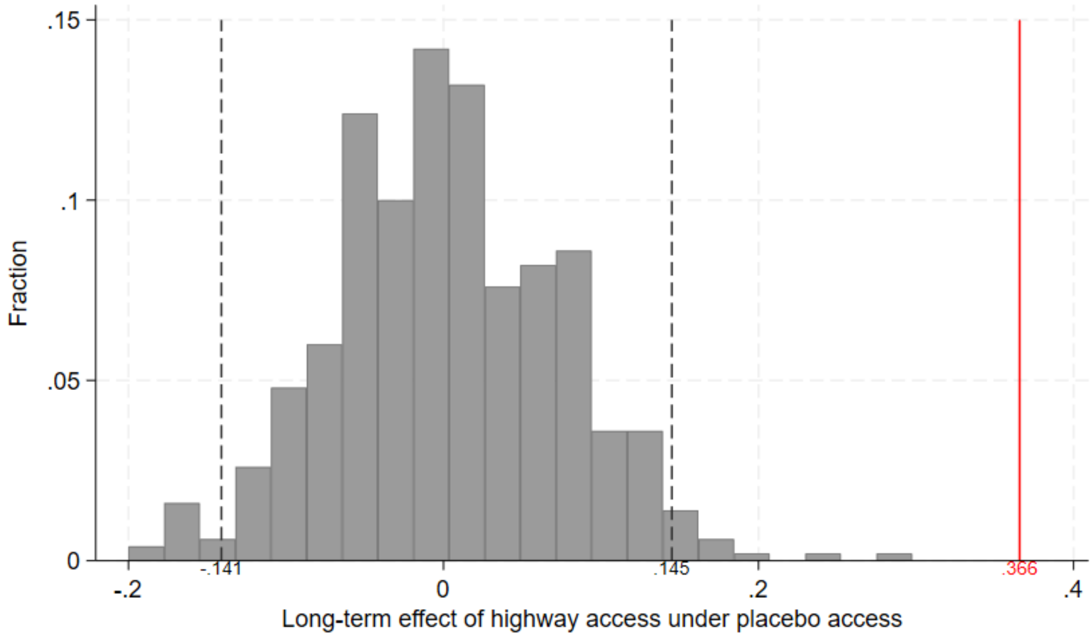
Note: All specifications include municipality and time fixed effects. The sample includes all non-urban municipalities within 10 km of an access. Standard errors (in parentheses) are clustered at the district level. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

Figure 10: Cumulative effect j years before/after access (<10 km) - Number of firms



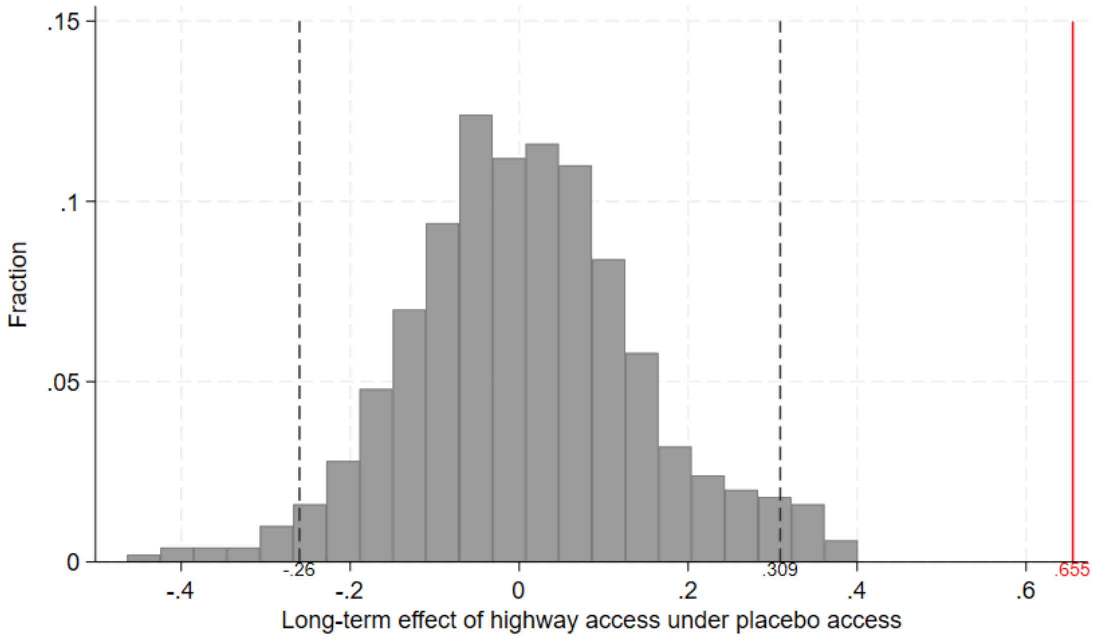
C.5 Placebos - Births & Deaths

Figure 11: The long-term effect on the creation of firms - Placebo tests



Note: Resulting histogram from our placebo tests where we randomized 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, specification (3).

Figure 12: The long-term effect on the death of firms - Placebo tests



Note: Resulting histogram from our placebo tests where we randomized 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, specification (4).