Essays on
Spatial Economics and Multinational Firms

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Für meine Eltern

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Abstract

This thesis aims to contribute to the literature in spatial economics and to analyse, how different factors shape regional economic structures. Thereby, I will in particular focus on:

i) The effect of a regionally very unevenly distributed, exogenous population shock on population growth within and between regions.
ii) The impact of universities on firm performance based on the universe of firms in Germany.
iii) The multinational productivity premium and its variation between urban and rural areas.

After a short introduction in Chapter 1, Chapter 2 analyses the persistence of a large population shock, the inflow of eight million ethnic Germans from Eastern Europe to West Germany after World War II. Based on census data it is shown that the shock was persistent within local labour markets, but diminished between local labour markets. This shows that the choice of spatial units can significantly affect the estimated persistence of population shocks and explain why previous studies on the persistence of population shocks reached conflicting conclusions.

Chapter 3 studies the impact of universities on the performance of firms in Germany. Firms, that are located in a county with a university, make 0.92 % more revenues per employee than firms, that are located in a county without a university. This finding is substantiated by an analysis of a subsample of universities, which were founded for political reasons. Studying the effect for different types of German universities indicates that the positive impact on firm performance is primarily driven by high-skilled regional labour supply, while proximity to the knowledge spillovers of research-intensive universities is associated with weaker firm performance. The counter-intuitive latter finding can be partially explained by the networks of multinational firms.

Chapter 4 analyses the role of regional characteristics for the productivity of multinational firms in Germany. Using administrative data of the Federal Statistical Office of Germany, the analysis identifies a so far unexplored rural productivity premium for multinational firms of about 5 log points in revenues per employee. This finding is substantiated by a two-stage treatment effects estimator, that analyses the causal effect of foreign takeover on the productivity of German firms. The results can be explained by stronger local competition in urban areas, which potentially increases the negative effects of outgoing knowledge spillovers on foreign technology leaders, making rural locations an attractive choice for them.

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Chapter 1

Introduction

After his famous example of a pin factory, Adam Smith went on to explain the conditions and circumstances that give rise to the division of labour and gains from specialisation:

> *"There are some sorts of industry, even of the lowest kind, which can be carried on nowhere but in a great town. A porter, for example, can find employment and subsistence in no other place. A village is by much too narrow a sphere for him [...]. In the lone houses and very small villages which are scattered about in so desert a country as the highlands of Scotland, every farmer must be butcher, baker, and brewer, for his own family."* (Smith, 1776, Book I, Chapter III, §2)

This early notion, that densely populated urban areas allow to a greater extend for division of labour as well as other economic advantages, so called agglomeration effects,[1] is studied by regional and urban economics. Following Proost and Thisse (2019, p. 577) I summarise regional and urban economics as spatial economics.[2] Due to a relatively large increase in data availability, empirical research in these fields surged over the last years. The three articles in this thesis contribute to this growing literature in spatial economics by addressing three distinct research questions. In the remainder of this introduction, I summarise the content of these articles, yet the review of the respective literature and how each of the articles contributes to it, is delegated to the respective

[1] Agglomeration economies are defined by Brakman et al. (2019, p.194) as "increasing returns at the location level". An early review of agglomeration economies is provided by Marshall (1890), who distinguishes between agglomeration from technology spillovers, labour pooling and sharing of intermediate inputs. Modern authors, such as Duranton and Puga (2004) distinguish between sharing, learning, and matching effects.

[2] Proost and Thisse (2019, p. 577) define regional economics as the subdiscipline, which "focuses on the mobility of goods and production factors within a large, economically integrated space (e.g., a nation or trade bloc) [.... ,while urban] economics studies the formation of cities, their spatial structure, and their social composition."

chapter.[3]

Chapter 2 studies the persistence of a large, unexpected, and regionally very unevenly distributed population shock, the inflow of eight million ethnic Germans from Eastern Europe to West Germany after World War II.[4] Using detailed census data from 1939-1970, the chapter shows that the shock proved persistent within local labour markets, but was largely reversed between labour markets. The results show that the choice of spatial units can significantly affect the estimated persistence of population shocks. They can thus help to explain why previous studies on the persistence of population shocks reached conflicting conclusions. The chapter is thereby interesting for the wider academic debate, whether exogenous population shocks can have a long-lasting effect by pushing an economy towards a higher equilibrium, or if these shocks should only have temporary effect.

Chapter 3 analyses the impact of universities on firm performance based on the universe of firms in Germany from 2013 to 2017.[5] From a regional perspective, firms have 0.92 % more revenues per employee in counties with a university than in a county without a university. To address potential endogeneity concerns, I focus on a subsample of universities, founded for political reasons. Analysing different types of German universities indicates that the high-skilled regional labour supply is important for the positive impact of universities, while proximity to the knowledge spillovers of research-intensive universities is associated with weaker firm performance. The latter finding is at least partially explained by the networks of multinational firms. The fact that multinational firms behave differently to national firms if they are located in proximity to research-intensive universities, is interesting for political and academic debate about profit shifting. Yet, this fact also gave rise to the broader question, whether multinational firms are affected differently by regional factors.

This broader question is addressed in Chapter 4. The chapter analyses the multina-

[3] Chapters 2 to 4 of this cumulative dissertation originate from separate articles. Therefore, notations and formulations, e.g. British versus American English, are adopted from the respective articles and might differ between chapters. Chapter 2 builds on joint work with Sebastian Braun, Anica Kramer and Michael Kvasnicka. Chapter 3 is single authored and chapter 4 is co-authored with Hartmut Egger and Elke Jahn.

[4] This chapter is based on Braun et al. (2021) and was published in the Journal of Economic Geography. My work on this chapter has benefited from comments by Hartmut Egger, Elke Jahn and seminar participants at the University of Bayreuth. Christian Kagerl provided valuable research assistance in collecting and processing various historical data for different sub-national administrative regions in Germany.

[5] This chapter is based on Meier (2024) and was published in the German Economic Review. When working on this chapter, I have benefited from comments by Hanna Adam, Sebastian Braun, Hartmut Egger, Bernd Fitzenberger, Christina Gathmann, Elke Jahn, Stefan Kornitzky, and Michael Pflüger. Kathrin Muth, Ana-Lena Jung and Christian Kagerl have provided valuable research assistance.

tional productivity premium and its variation between urban and rural areas.[6] Using administrative data for Germany, the analysis confirms previous research findings that multinational firms are more productive than their national counterparts. Additionally, the chapter identifies an urban productivity premium for national firms and a previously unexplored rural productivity premium for multinational firms. Employing a two-stage treatment effects estimator, the chapter shows a positive causal effect of foreign takeover on the productivity of German firms, which is more pronounced in rural than in urban areas. Finally, local competition is identified as a key factor in explaining the observed productivity patterns. Stronger competition potentially increases the negative effects of outgoing knowledge spillovers on foreign technology leaders, making rural locations an attractive choice for them. Overall these findings are of particular interest for the academic literature on urban agglomeration effects as well as for the literature on multinational firms and their location decisions.

Finally, Chapter 5 concludes and summarises the most important results of each chapter.

[6] This chapter is joint work with Hartmut Egger and Elke Jahn. Working on this chapter I have benefited from comments by Hanna Adam, Janina Botzki, Maximilian von Ehrlich, Michaela Kesina, Douglas Nelson, Philip Sauré, and Daniel Sturm as well as participants of the European Trade Study Group, the Swiss Workshop on Local Public Finance and Regional Economics, the Göttinger Workshop for International Economics, the Oxford-Bayreuth Doctoral Research Workshop, and the Graduate Seminar at the University of Bayreuth.

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Chapter 2

Local Labor Markets and the Persistence of Population Shocks: Evidence from West Germany, 1939-70

2.1 Introduction

We study the persistence of a very large population shock, the inflow of eight million displaced Germans (expellees) from Eastern Europe to West Germany after World War II. This population shock hit West German counties very unequally, with expellee inflow rates ranging from 1.4% of the pre-war population to as much as 83%. We show that this migration-induced regional population shock had a persistent effect on the distribution of population *within* labor markets, but was largely reversed *between* labor markets.

Our findings can help to explain the disparate results in the growing empirical literature on the persistence of population shocks. This literature exploits population shocks to gauge the relative importance of the two main explanations put forward for the spatial distribution of economic activity, locational fundamentals and increasing returns.[1] The locational fundamentals theory holds that long-lasting geographic conditions, such as access to a river, determine the spatial distribution of economic activity. Consequently, shocks to the spatial distribution of population should have only

[1] See Redding (2010) for a general overview of the existing empirical literature on new economic geography, including the empirical approaches to distinguish between locational fundamentals and increasing returns.

temporary effects on regional population patterns. The increasing returns theory, in contrast, suggests that population density itself may enhance productivity because of agglomeration economies. According to this second theory, shocks to the distribution of economic activity could well have long-run consequences if they are large enough to shift the economy from one equilibrium to another (see Henderson, 1974; Krugman, 1991, for seminal theoretical contributions).

Empirical studies that exploit exogenous population shocks to explore these explanations have produced diverging results.[2] A first set of studies shows that bombings during World War II had no persistent effect on city size in Japan (Davis and Weinstein, 2002) and West Germany (Brakman et al., 2004). Furthermore, Davis and Weinstein (2008) find that the industrial structure of Japanese cities also recovered quickly to its pre-war pattern. The findings of this first set of studies provide empirical support for the locational fundamentals theory, which predicts that temporary shocks have only temporary effects. A distinctive feature of these studies is that they typically use larger cities as their unit of observation.[3] This is of importance for the argument developed in this chapter, since larger cities are usually located in different regional labor markets.

A second set of studies, in contrast, finds that migration-induced population shocks during and after World War II were highly persistent. Sarvimäki (2011) shows that the inflow of forced migrants into rural areas of Finland had a re-inforcing effect on post-war population growth, and Schumann (2014), focusing on the West German state of Baden-Württemberg, shows that expellee inflows had a persistent effect on municipality size. Similarly, Eder and Halla (2016) find that inner-Austrian migration out of the (temporary) Soviet occupation zone still affects the spatial distribution of population in Austria today. The findings of this second set of studies hence suggest that locational fundamentals do not determine long-run population patterns.

We contribute to this empirical literature by studying the persistence of a major population shock, the inflow to West Germany of German expellees from Eastern Eu-

[2] Disentangling locational fundamentals and economies of scale is empirically challenging. This is because locational fundamentals are long-lasting and may have promoted economies of scale later on, and because exogenous changes in locational fundamentals are extremely rare. Exploiting exogenous population shocks is thus a popular identification strategy for distinguishing between increasing returns and locational fundamentals. Bleakley and Lin (2012) is a prominent exception in this regard. The authors exploit the fact that a natural advantage, namely portage sites, became obsolete over time. Their results support agglomeration effects and path dependency: Even after portage sites lost their function for transportation, cities along these places grew faster.

[3] Miguel and Roland (2011) is an exception in this regard. The authors use district-level data to show that US bombing during the Vietnam War had no long-run effect on later economic development in Vietnam.

rope after World War II.[4] Two features make the historical episode particularly well suited for our analysis. First, the inflow was not only large, increasing West Germany's population from 39 million in 1939 to 48 million in 1950, but also very unequally distributed across West German counties. Second, the initial allocation of expellees was driven by the availability of housing and the geographic distance between origin and destination regions, not by economic fundamentals. In particular, we show that conditional on control variables for the local housing supply, the distribution of expellees was unrelated to pre-war trends in population growth.

We show that the choice of the regional unit of observation and the type of variation exploited, so far largely ignored in the literature, are vital for the estimated persistence of the population shock. Specifically, we find that expellee inflows had a persistent effect on the spatial distribution of population within local labor markets. In contrast, the inflows had little effect on the distribution between labor markets, as population growth in 1950-70 reversed much, though not all, of the initial population shock.

We interpret our findings in the light of the classic monocentric land use theory, developed by Alonso (1964), Mills (1967) and Muth (1969). We think of labor markets as functional geographic areas, in which workers commute from the periphery to an urban core. Two empirical observations guide our interpretation: First, expellee inflow rates were considerably larger in the periphery of labor markets than in their core. Second, inflows induced the construction of local roads. The monocentric model predicts that better transport infrastructure decreases the share of population living close to the labor market core, i.e., fosters suburbanization, and increases the overall population of a labor market. Our empirical findings are consistent with these two predictions. We argue that road infrastructure investments increased the equilibrium size of labor markets but were not large enough for them to fully absorb expellee inflows. Consequently, migration from high- to low-inflow labor markets reversed much, though not all, of the initial population shock between labor markets. Within labor markets, road infrastructure investments induced suburbanization. Since expellees were already over-represented in the periphery, their inflow did not necessitate re-adjustment within labor markets. To put it differently, the spatial distribution of population after the expellee inflow was consistent with the (post-migration) equilibrium within but not between labor markets.

Our basic point is thus simple: empirical studies on the persistence of population

[4] Previous studies have exploited regional variation in expellee inflow rates to analyze the short-run effect on native employment (Braun and Omar Mahmoud, 2014) and structural change (Braun and Kvasnicka, 2014), the dynamic response of local labor markets (Braun and Weber, 2016), and the effect on productivity and regional economic development (Peters, 2017).

shocks should carefully explain whether they consider the (determinants of) spatial equilibrium within or between geographic areas to facilitate cross-study comparisons. Results from inter-city regressions, for instance, are not directly comparable to those from intra-city regressions, as the relevant determinants of spatial equilibrium are likely to differ.[5] This general insight can help to explain the diverging results in the existing literature on the persistence of population shocks.[6] To illustrate, consider the aforementioned study by Schumann (2014) who also focuses on the inflow of expellees to West Germany after World War II. Schumann (2014) restricts the analysis to one federal state, Baden-Württemberg. After the war, Baden-Württemberg was temporarily divided into two occupation zones, a French and an American zone. Expellees were initially not resettled into the French zone of occupation, which created a sharp discontinuity at the border to the American zone of occupation. Schumann (2014) shows that this discontinuity is still visible 25 years after the war. Importantly, however, municipalities along the occupation zone border often belonged to the same local labor market. Schumann thus effectively exploits only variation *within* local labor markets.

Unlike Schumann, our analysis considers the whole of West Germany and exploits variation in expellee inflows not only within but also between local labor markets. When we exploit only variation within local labor markets, we confirm the results Schumann obtained for municipalities in Baden-Württemberg. However, and importantly, we also show that his results do not carry over to population patterns *between* local labor markets. At this more aggregated regional level, population patterns quickly revert back towards their pre-war level. Our preferred estimate suggests that 83% of the initial shock is dissipated 25 years after the war. The finding highlights the crucial relevance of the choice of the regional unit[7] and the type of variation exploited in the analysis for the estimated persistence of a population shock.

[5]Duranton and Puga (2015) make this distinction very explicit in their discussion of the effect of transport infrastructure in the urban growth literature. In particular, they distinguish between the effect on inter-city population growth and the effect on intra-city suburbanization.

[6]Our findings complement previous arguments by Schumann (2014) who suggests that locational fundamentals might be particularly important for geographically diverse countries and for urban areas. Likewise, Sarvimäki (2011) suggests that a population shock may be large enough to change the equilibrium of rural areas "at the brink of becoming a local manufacturing center" (p. 3) but not the equilibrium of well established cities.

[7]The choice of regional unit also conciliates the findings of Schumann (2014) and the results on internal migration in Braun and Weber (2016). The latter develop a two-region search and matching model to analyze how regional labor markets adjusted to the expellee inflow, and show that migration from high- to low-inflow regions was an important channel of adjustment. The result appears to contradict Schumann who finds no evidence for major outflows from the high-inflow American occupation zone. The different units of observations can explain these seemingly disparate findings: While Schumann (2014) studies small municipalities located close to each other, Braun and Weber (2016) divide West Germany in their analysis in only two large regions.

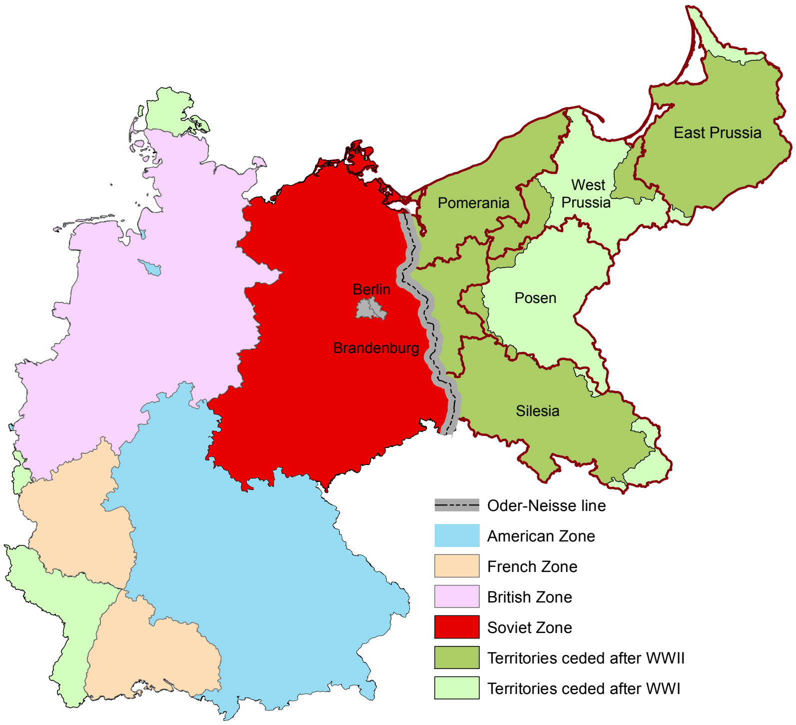
Our findings are also relevant for the literature that studies the effect of immigrant inflows on population outflows. This literature has not yet reached a definite conclusion: Some studies find that immigrant inflows lead to native outflows (Borjas, 2006; Boustan et al., 2010), whereas other studies find no such link (Card and DiNardo, 2000; Card, 2001). Using net migration as an additional outcome variable, we show that variation in expellee inflows between but not within local labor markets is negatively associated with net population flows, mirroring our results for population growth. Since expellees were more likely to migrate than natives (Bauer et al., 2013; Braun and Weber, 2016), they are likely to have contributed disproportionally to these migration flows.

The chapter proceeds as follows. Section 2.2 provides background information on the expellee inflow to West Germany after World War II. Section 2.3 describes the various data sources and the identification strategy we use in our empirical analysis. Section 2.4 presents our regression results. Section 2.5 interprets our findings. Finally, Section 2.6 summarizes our main findings and concludes.

2.2 Background

After World War II, West Germany experienced the inflow of eight million expellees (*Heimatvertriebene*), most of them from the ceded eastern provinces of the defeated German Reich. The displacement of Germans took place from 1944 to 1950 and occurred in three distinct phases (for further details see, e.g., Connor (2007), Douglas (2012), and Schulze (2011)). The first phase began in 1944, when hundreds of thousands of Germans from the eastern provinces of the German Reich fled from the approaching Red Army. Most of these refugees planned to return home after the end of the war, and therefore fled to the nearest West German regions. After Nazi Germany's unconditional surrender in May 1945, Polish and Czech authorities began to drive their remaining German populations out. These so-called wild expulsions, which constituted the second phase of the displacement, were not yet sanctioned by an international agreement. The third phase began after the Soviet Union, the United Kingdom, and the United States signed the Potsdam Agreement in August 1945. The Potsdam Agreement shifted Germany's eastern border westwards to the Oder-Neisse line. The former eastern provinces of the German Reich were placed under Polish or Russian control (see Figure 2.1). Germans remaining east to the new border were brought to post-war Germany in compulsory and organized transfers. The German territory west to the Oder-Neisse line was divided into four occupation zones: a British, a French, an American, and a Soviet zone.

Figure 2.1: *The Division of Germany and German Territorial Losses after World War I and II*



Source: Own illustration. *Basemap*: MPIDR (2011).

Overall, the mass exodus of Germans from East and Central Europe involved at least 12 million people. Most expellees re-settled in West Germany. By September 1950, expellees accounted for 16.5% of the West German population.[8] However, the population share of expellees differed greatly across West German counties, ranging from 1.8% in Pirmasens to 41.4% in Goslar. Our empirical analysis will exploit this pronounced regional variation, which we will now discuss in greater detail along with its underlying reasons.

Regional Distribution: Figure 2.2a illustrates the immigration-induced increase in population across counties, as measured by the number of expellees in 1950 over the population in 1939 (henceforth, expellee inflow rate). This figure provides three main insights. First, there were large differences in the expellee inflow rate *between* occupation zones. In particular, the rate was much higher in the American zone (30.2 %) and British zone (31.4%) than in the French zone (7.5%). This is because the

[8] Most expellees arrived until 1946. In the October 1946 census, the first one conducted after World War II, the number of expellees registered already accounts for 76% of the respective expellee total recorded in the September 1950 census.

French initially refused to accept any expellees in their occupation zone. The French felt not bound by the Potsdam Agreement, as they had not been invited to the Potsdam conference. As a result of the French refusal, expellees were initially transferred only to the American and British occupation zones in the third phase of the displacement. This created a sharp discontinuity in expellee inflow rates at the border between the American and French zones of occupation, as illustrated in greater detail in Figure 2.2b. It is this sharp discontinuity that Schumann (2014) exploits to estimate the persistence of the expellee inflow on the spatial distribution of population in parts of Baden-Württemberg.[9]

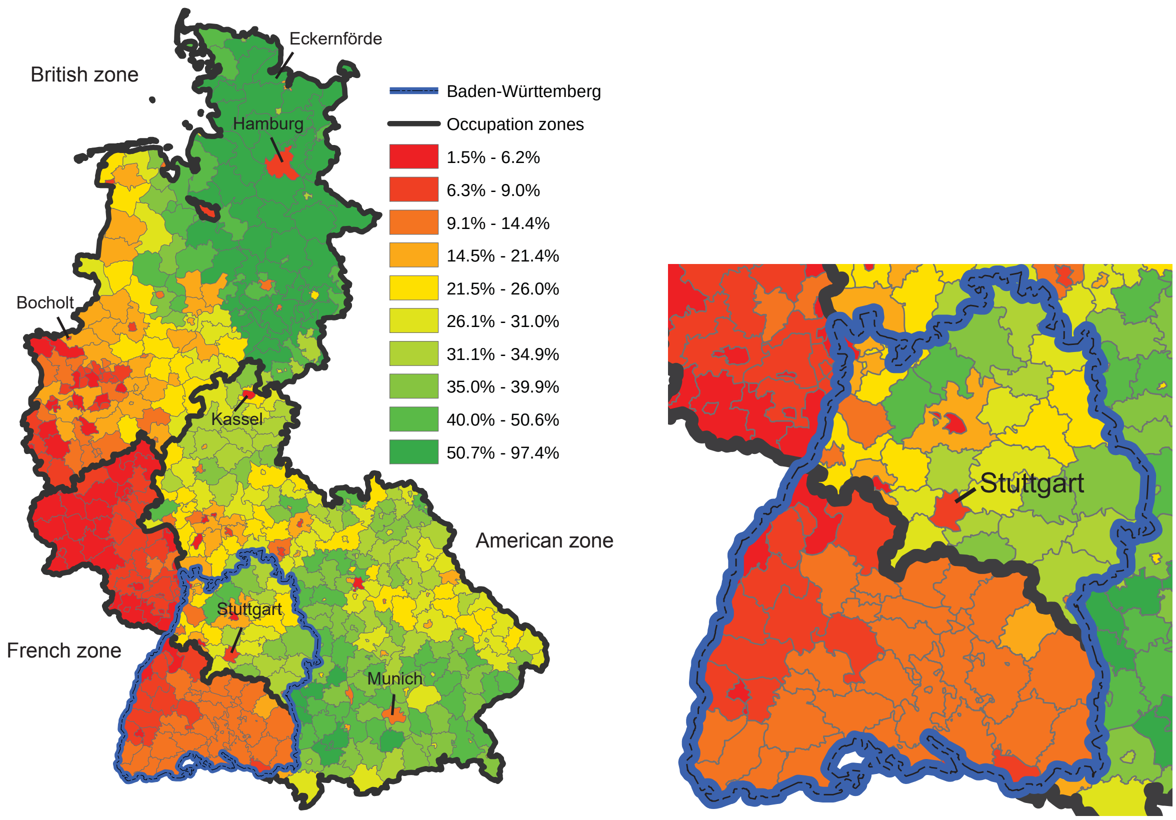
Second, the population share of expellees also differed greatly *within* occupation zones. This is particularly evident for the British zone where the expellee inflow rate ranged from 4.0% in the western county of Bocholt to 83.5% in the north-eastern county of Eckernförde. This west-east divide was a result of the largely undirected flight of Germans during the final stages of the war (the first phase of the displacement). As the Soviet troops pushed westwards, Germans residing in the eastern provinces of the German Reich were forced to seek shelter further west. The refugees thus crowded in the most accessible regions in the west and north-west of West Germany. Refugees from East Prussia, for instance, mostly ended up in the northern state of Schleswig-Holstein, as East Prussia and Schleswig-Holstein were connected via the Baltic Sea. The wild expulsions (second phase of the displacement) only added to the regional imbalance between counties in the west and east, as Polish and Czech authorities often just drove Germans across the border into occupied Germany. Many Germans from the Sudetenland, for instance, were forced into neighboring Bavaria.

Third, the population share of expellees also differed systematically between cities and surrounding rural areas. Figure 2.2b highlights the example of the city of Stuttgart. While the expellee inflow rate was only 8.5% in Stuttgart, it ranged from 27.3% to 31.7% in the five immediately neighboring rural counties. Similar patterns can be observed for other cities such as Hamburg in the north, Kassel in the center, and Munich in the south of Germany. Expellees were generally more likely to be placed in rural areas, where the housing stock had remained largely intact during the war (Connor, 2007).

Recapitulating the above, the historical setting we explore provides rich spatial variation in expellee inflows rates. Expellee inflow rates differed both *between* counties

[9] In related recent work, Wyrwich (2020) studies the long-run effects of the French occupation zone on population growth. He documents that regions in the French occupation zone saw lower growth in 1939-2010 compared to regions in the American or British zone, a finding that the author attributes to the French refusal to accept expellees in their zone of occupation.

Figure 2.2: *Expellee Inflow Rates*



(a) *West Germany* **(b)** *Baden-Württemberg*

Notes*: The figures depicts the number of expellees per county on 13 September 1950 over the population per county on 1 September 1939 in West Germany (panel 2.2a) and the state of Baden-Württemberg (panel 2.2b). The black line depicts the border of the three occupation zones. The blue line, which partly overlaps with with the black line, depicts the border of the West German state of Baden-Württemberg.* Source*: Statistisches Bundesamt (1952).* Basemap*: MPIDR (2011).*

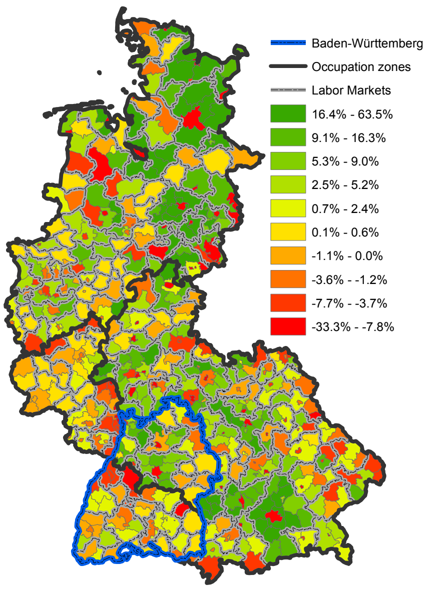
far away from each other–for instance, between counties located in the west and the north of Germany–and between neighboring counties–for instance, between neighboring counties at either side of the French occupation zone border. The average inflow rate across all counties was 0.270, with a standard deviation of 0.176.

Variation Between and Within Local Labor Markets: The labor markets of neighboring counties are often well connected through commuting flows, and several counties typically form one local labor market. Based on commuting flows, IfW (1974) defines 164 labor market regions, each consisting of an average of 3.4 counties.[10] Expellee inflow rates in our setting differ greatly both *within* and *between* these local labor markets. To show this, we decompose the overall variation in expellee inflow rates. Let I_{ij} be the expellee inflow rate for county i located in labor market j. We decompose I_{ij} into a between component, $\bar{I}_j$, and a within component, $I_{ij} - \bar{I}_j$. The between component is simply the expellee inflow rate measured at the level of local labor market j, while the within component is the difference between the inflow rate of a particular county i in labor market j and the inflow rate of labor market j.

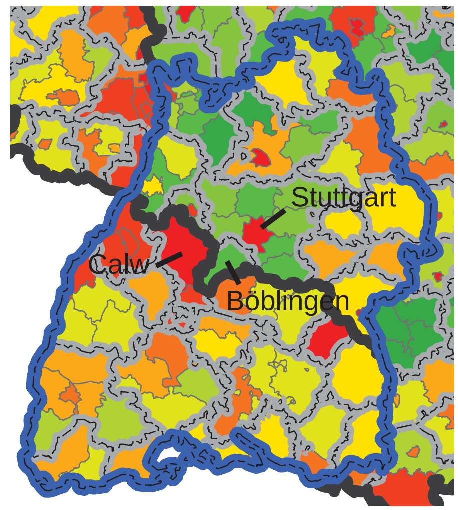
Figure 2.3a illustrates for West German counties the within component, i.e., the variation in expellee inflow rates across counties located in the same labor market region. The within component ranges from -0.333 to 0.635 with a standard deviation of 0.112. Zooming in to the state of Baden-Württemberg, Figure 2.3b illustrates that the within-labor-market variation comes from three sources. First, the borders of local labor markets (the dashed black line on grey ground in the figure) frequently spanned counties from both sides of the French occupation zone border, and these counties typically experienced very different inflow rates. The counties of Calw and Böblingen, for instance, were both part of the same labor market but their inflow rates differed greatly. Whereas the inflow rate of Calw stood at 8.7% in 1950, the inflow rate of Böblingen was 30.5% (see Figure 2.3b). The inflow rate in Calw, therefore, was significantly below the inflow rate of the local labor market in which it was situated. Second, local labor markets frequently consisted of both a larger city, typically with low expellee inflow rates, and surrounding hinterlands, with larger inflow rates. The city of Stuttgart is a case in point (see again Figure 2.3b). Third, variation in expellee inflow rates within local labor markets also reflected the east-west or north-south gradient in inflow rates, although this variation was typically more modest between neighboring counties.

[10]To the best of our knowledge, the definition in IfW (1974) is the earliest definition of local labor markets in West Germany. A few counties belong to more than just one local labor market. In this case, we assign the county to the labor market with which it shares the larger area.

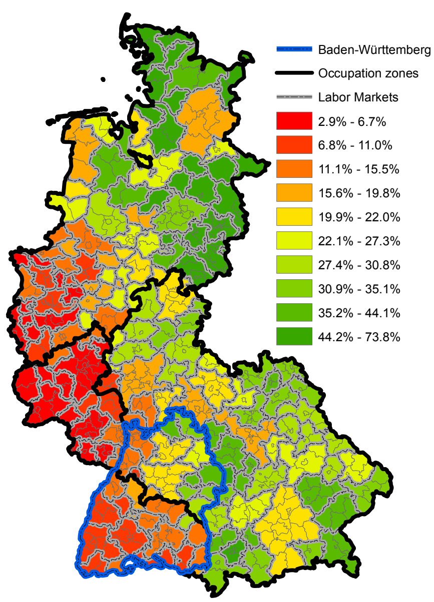
Figure 2.3: *Variation in Expellee Inflow Rates Within and Between Labor Markets*



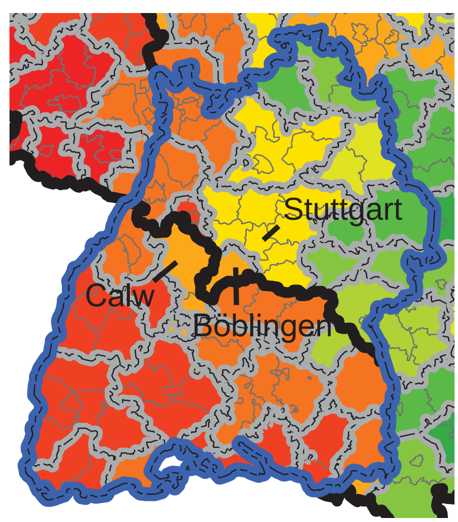
(a) *West Germany, within*



(b) *Baden-Württemberg, within*



(c) *West Germany, between*



(d) *Baden-Württemberg, between*

Notes*: The figures depict the number of expellees per county on 13 September 1950 over the population on 1 September 1939 per county in West Germany (panels 2.3a and 2.3c) and in the state of Baden-Württemberg (panels 2.3b and 2.3d). The upper two panels calculate figures at the level of counties, the lower two panels at the level of local labor markets. The solid black line depicts the borders of the three occupation zones, the dashed black line on grey ground depicts the borders of local labor markets, and the blue line, which partly overlaps with the black line, depicts the border of the West German state of Baden-Württemberg.*
Source*: Statistisches Bundesamt (1952).* Basemap*: MPIDR (2011).*

In addition to this variation within local labor markets, there was also sizeable variation in expellee inflow rates between local labor markets. Figure 2.3c illustrates this between component of the total variation in expellee inflow rates for West Germany. The between component varies between 0.029 and 0.738, with a mean of 0.257 and a standard deviation of 0.162. The figure shows that much of the variation in the between component came from the stark difference between local labor markets in the north and east of the country and those in the west and south-west. As noted before, this east-west divide was mostly the result of the largely undirected flight to the most accessible West German regions at the end of World War II; and it was reinforced by the French refusal to allow any expellees into their occupation zone in the south-west of Germany. Importantly, however, Figure 2.3d, which zooms in to the state of Baden-Württemberg, shows that the sharp discontinuity of expellee inflow rates at the French occupation zone largely disappears when inflow rates are calculated at the level of local labor markets. This is mainly because some labor markets spanned counties from both sides of the occupation zone border. Moreover, the low inflow rate into Stuttgart counter-balanced the high inflow rates of counties in its hinterland, including those at the occupation zone border.

2.3 Empirical Strategy

We exploit regional variation in expellee-induced population increases across West German counties. We use West German counties in their 1970 borders.[11] As major changes to county borders occurred in the 1970s, we also confine the period of analysis up to that year.[12] Our main data sources are the population and occupation censuses of 1939, 1946, 1950, 1961 and 1970 which we have digitalized for our analysis. Appendix 2.G provides a detailed overview of the data sources for all variables.

[11] There are 548 counties in 1970. However, a few of them experienced changes in their administrative borders between 1939 and 1970. While population data for 1939, 1950 and 1970 are available for counties in their 1970 borders, some of our control variables refer to counties in their 1939 or 1950 borders. We account for border changes between 1939 and 1970 by merging counties so that county borders are generally comparable over time (see Appendix 2.F for the details). This leaves us with 511 counties. Counties located in the states of Rhineland-Palatinate and Schleswig-Holstein saw major border changes in 1969/70. For counties located in these two states, we use the administrative borders immediately before the major border changes.

[12] Changes to administrative county borders, mainly in the 1970s, reduced the total number of counties from 548 in 1970 to just 321 in 1987, the year of the first census after the 1970 census we use in our analysis.

Within and Between Regressions: We begin by estimating the following OLS regression:

$$G_{ij}^{70,50} = \alpha_1 + \beta_1 I_{ij}^{50,39} + X_{ij}\gamma_1 + u_{ij}, \tag{2.1}$$

where $G_{ij}^{70,50}$ is the population change in 1950-70 over the population in 1939 of county i in labor market j (henceforth: population growth in 1950-70), $I_{ij}^{50,39}$ is the expellee inflow rate of county i between 1939-50, X_{ij} is a vector of covariates, and u_{ij} is an error term.[13] The regression tests whether expellee-induced population growth in 1939-50 reduced or reinforced population growth in 1950-70. The former case is typically interpreted in the literature as evidence for the importance of locational fundamentals, the latter as evidence for the importance of agglomeration economies. Specification (2.1) mimics the conventional approach in the literature (see, for instance, Sarvimäki, 2011; Davis and Weinstein, 2002) to test whether shock-induced population growth in one period affects population growth in subsequent (post-shock) periods.

Our key hypothesis is that the persistence of expellee-induced population growth will differ depending on the type of variation we exploit in the empirical analysis. We thus run two additional specifications in which we only exploit variation within or between local labor markets:

$$\text{Within:} \quad (G_{ij}^{70,50} - \bar{G}_j^{70,50}) = \beta_2(I_{ij}^{50,39} - \bar{I}_j^{50,39}) + (X_{ij} - \bar{X}_j)\gamma_2 + (u_{ij} - \bar{u}_j), \tag{2.2}$$

$$\text{Between:} \quad \bar{G}_j^{70,50} = \alpha_3 + \beta_3 \bar{I}_j^{50,39} + \bar{X}_j\gamma_3 + \bar{u}_j, \tag{2.3}$$

where $\bar{Z}_j$ denotes the value of variable Z for local labor market j. Specification (2.2) considers deviations from labor-market-wide levels, and thus exploits only variation between (nearby) counties *within* the same local labor market. Specification (2.3) aggregates the county-level data to the level of local labor markets, and only uses the variation *between* (more distant) local labor markets in West Germany. The between specification differs from Specification (2.1) in the choice of the regional unit considered: The former studies local labor markets, the latter focuses on counties. Our key hypothesis thus states that $\beta_2 \neq \beta_3$.[14]

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[13] We normalize both population change in 1950-70 and expellee inflows by population in 1939 to simplify the interpretation of β_1. In particular, $\beta_1 = -1$ indicates that the expellee-induced population shock is completely reversed by 1970. We show in Section 2.4.1 that our results are robust to normalizing the dependent variable by population in 1950.

[14] This hypothesis implies that regression equation (2.1) is misspecified. In particular, we postulate a regression model in which labor-market wide expellee inflows have a different effect on post-war population growth than deviations from this average, i.e., $G_{ij}^{70,50} = \alpha_1 + \beta_2(I_{ij}^{50,39} - \bar{I}_j^{50,39}) + \beta_3 \bar{I}_j^{50,39} + X_{ij}\gamma_1 + u_{ij}$.

Identification: Identifying the *causal* effect of population growth on subsequent population growth is challenging because confounding factors may drive population growth in both periods (Davis and Weinstein, 2002; Sarvimäki, 2011). Our empirical exercise isolates variation in wartime population growth that is due to the inflow of expellees. The key identifying assumption for a causal interpretation of β_1, β_2, and β_3 is that there is no unobserved factor that drives both the expellee inflow rate and population growth in 1950-70. In particular, estimates will be upward (downward) biased if expellees systematically selected, based on unobservable characteristics, into West German regions with a higher (lower) underlying potential for population growth.

For several reasons, self-selection of expellees was arguably a minor problem until 1950, when we measure expellee inflows. First, expellees did not choose their initial destination in West Germany based on local economic conditions (which, in turn, are likely to correlate with potential population growth). Expellees initially fled to the most accessible regions in West Germany and were later forcibly transferred to a destination (see Section 2.2). Second, the military governments of the occupation powers, overburdened by the mass inflow of millions of expellees, did not redistribute expellees according to local economic conditions (Braun and Omar Mahmoud, 2014; Braun and Kvasnicka, 2014). Finally, once expellees were resettled in a destination, they could not just move on by their own choice. The occupying powers enacted severe moving restrictions (Müller and Simon, 1959), so that the initial distribution of expellees proved very persistent in the first years after the war.

Our specific historical context thus limits concerns of endogenous self-selection. However, there are still two main threats to identification. First, while military governments did not allocate expellees according to local economic conditions, the distribution of expellees was not altogether random. Since the main objective of military authorities at the time was to find accommodation for all expellees, expellees were under-represented in urban areas that were devastated by the war and offered only limited housing capacity. If war destruction and urbanization rates had an effect on post-war population growth, coefficients on expellee inflow rates will be biased in unconditional OLS regressions. Second, moving restrictions were gradually phased out by 1949. Some expellees, as a consequence, might have moved endogenously by 1950.

We deal with these threats to identification in two main ways. First, we control for war destruction and urbanization, and for other local characteristics that might have affected population growth. We then show that conditional on these covariates, expellee inflow rates are unrelated to regional population growth before the war. This corroborates our argument that once we condition on urbanization and measures of war destruction, expellee inflows were unrelated to potential population growth. Appendix 2.A also shows that differences in pre-war economic characteristics between counties with high and low expellee inflow rates tend to disappear once we control for war destruction. Second, we use the expellee inflow rate between 1939 and 1946 as an instrument for the expellee inflow rate between 1939 and 1950. Since strict restrictions on relocations were still in place in 1946, this IV regression exploits only variation in expellee inflow rates that is attributable to the initial inflow of expellees, and not to subsequent, and potentially endogenous, relocations within West Germany.

Controls: We control for regional characteristics that might have affected expellee settlement patterns and influenced potential population growth. First and foremost, we include various measures of war destruction. War destruction correlates–through the availability of housing–with local expellee inflow rates and might have affected also post-war population growth.[15] We use three different measures of war destruction. As our baseline measure, we consider the share of dwellings built until 1945 that were damaged in the war, using information from the 1950 housing census. Unfortunately, the housing census did not count dwellings that were completely destroyed in the war. The share of damaged dwellings is thus calculated only relative to residential housing that could still accommodate residents in 1950. Our second measure is rubble at the end of the war per capita in 1939, as also used in previous work by Brakman et al. (2004), Burchardi and Hassan (2013) and Braun and Kvasnicka (2014). Unfortunately, data on rubble are only available for the 199 largest West German cities. We aggregated the city-level data to the county level, assuming that smaller municipalities did not suffer any war destruction. The rubble indicator will thus underestimate the extent of war destruction in counties with smaller municipalities. The third measure classifies the loss in housing space in four categories, ranging from 'no losses' (1) to 'very substantial losses' (4). This indicator variable is based on various administrative sources at the national and federal state level.

Second, concerning measures of urbanization, we control for a county's population density in 1939. Urban areas offered less potential for housing expellees, and thus received lower expellee inflows. At the same time, population growth may have systematically differed between rural and urban areas. We also use, as alternative measures of urbanization, the population share living in cities with at least 10,000 inhabitants and dummies for the size of the largest city in the local labor market (for cities populated by 100,000-250,000 and more than 250,000 inhabitants).

[15] Heavily destroyed cities, in fact, grew faster after the war (Brakman et al., 2004).

A third set of covariates includes variables that proxy pre-war economic conditions. First, we include information on pre-war turnover per worker, sampled from turnover tax statistics. This variable accounts for pre-war differences in economic conditions and development. Second, we include the share of the total workforce in a county that is employed in agriculture in 1939.

Finally, we also include a dummy for counties that are less than 75 kilometers away from the post-war inner-German border. Redding and Sturm (2008) show that cities at the inner-German border generally experienced lower population growth than other West German cities, and attribute this difference to a disproportionate loss in market access for cities at the new border. At the same time, counties at the inner-German border received higher-than-average expellee inflows, due to their proximity to the eastern territories of the German Reich (see Section 2.2).

Expellee Inflows and Pre-war Population Growth: Before we present our main results, we show that pre-war population growth is uncorrelated with expellee inflow rates once we condition on our set of covariates. Table 2.1 presents the results from regressing population growth in 1871-1910, 1910-1939, and 1925-1939 on expellee inflow rates and on our standard set of covariates (Appendix 2.D presents the corresponding conditional scatter plots). The coefficient on the expellee inflow rate is not statistically significant in three out of the four regressions, the exception being population growth in 1871-1910 (see column (1)). This positive correlation, however, is driven by just a few outliers that experienced excessive population growth during this phase of rapid industrialization (esp. in the Ruhr area where few expellees arrived). Dropping the 11 counties with annual population growth of above 10%, as done in column (2) of Table 2.1, causes the estimated coefficient on the expellee inflow to drop sharply from 0.016 to 0.003 and turn statistically insignificant.[16] Overall, therefore, these findings corroborate our identifying assumption that conditional on our covariates, expellee inflow rates do not correlate with a region's underlying population growth.

[16] The fast-growing counties were typically small in 1871. Weighting the regression in Column (1) by 1871 population halves the coefficient estimate from 0.016 to 0.008.

Table 2.1: *Expellee Inflows and Pre-war Population Growth*

	1871-1910	1871-1910	1910-1939	1925-1939
	(1)	(2)	(3)	(4)
Inflow Expellees 1950	0.016***	0.003	0.001	0.001
	(0.006)	(0.003)	(0.002)	(0.003)
Observations	511	500	511	466

Notes: The dependent variable in columns (1) and (2) is population growth in 1871-1910, in column (3), population growth in 1910-1939, and in column (4), population growth in 1925-1939. Column (2) excludes the 11 counties with annual population growth of above 10% in 1871-1910. All regressions include our standard set of control variables, i.e., population density in 1939, the employment share in agriculture in 1939, turnover per capita in 1935, the share of damaged dwellings, and a dummy for counties within 75 km of the inner-German border. Data on population in 1925 is missing for counties located in the state of Rhineland-Palatinate. Robust standard errors clustered at the level of local labor markets are reported in brackets. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

2.4 Results

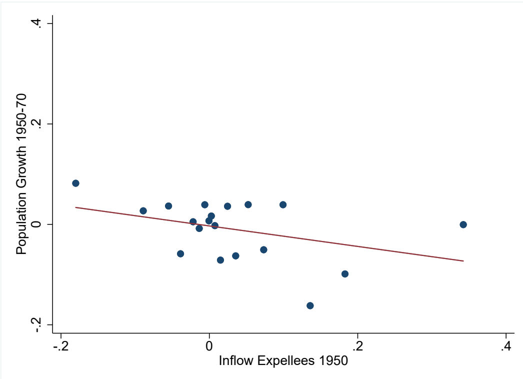
2.4.1 Baseline Results

Binned Scatter Plots: We begin by documenting graphically the importance of the two sources of variation exploited in our analysis. Figure 2.4 depicts unconditional binned scatter plots of population growth in 1950-70 and expellee inflow rates, grouping expellee inflow rates into 20 equal-sized bins. Figure 2.4a uses only variation within local labor markets, whereas Figure 2.4b uses only variation between local labor markets. Each scatter plot also shows the respective linear OLS regression line.

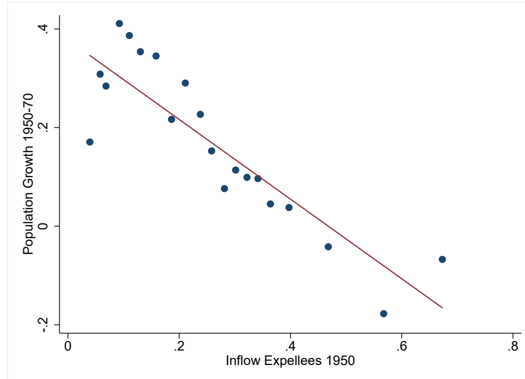
Figure 2.4a shows a weakly negative relationship between the expellee inflow rate and post-war population growth. The binned scatter points are quite dispersed around the regression line, which suggests that its slope is only imprecisely estimated. The estimated OLS slope coefficient is -0.204 with a standard error of 0.834. The unconditional regression thus suggests that expellee-induced population growth had a persistent effect on population patterns within local labor markets, as subsequent population growth did not reverse the initial shock.

This does not imply, however, that there has been no adjustment *between* labor market regions. In fact, Figure 2.4b shows that local labor markets that exhibited faster (slower) population growth in 1939-1950 grew, on average, less (more) strongly in 1950-1970. The estimated slope coefficient is -0.808 with a standard error of 0.090. This strong and statistically significant negative association is suggestive of significant

Figure 2.4: *Binned Scatter Plots (Unconditional)*



(a) *Within Local Labor Markets*



(b) *Between Local Labor Markets*

Notes*: The figures depict binned scatter plots of population growth in 1950-70 and expellee inflow rates, grouping expellee inflow rates into 20 equal-sized bins. Panel 2.4a relates deviations from labor-market-wide averages to each other, whereas Panel 2.4b considers labor-market-wide averages themselves.*

population adjustments that reversed most of the initial population shock (a coefficient of -1 would indicate complete reversion).

Taken together, Figures 2.4a and 2.4b illustrate our main point. The persistence of population shocks might be very different, depending on whether one considers variation within or between local labor markets. In our setting, the within variation points towards a high persistence of population shocks, which, in the relevant literature, is typically interpreted as evidence against the importance of locational fundamentals. The between variation, in contrast, suggests that across local labor markets, population shocks are largely reversed, which is in line with the locational fundamentals hypothesis.

Regression Results: For reasons discussed in Section 2.2, expellee-induced population growth in 1939-50 is unlikely to be completely orthogonal to underlying population growth potential in 1950-70. We therefore next test whether the unconditional correlations are still evident in a multivariate regression framework.

Table 2.2 reports our main regression results. The table reports conditional OLS (columns (1)-(3)) and IV estimates (columns (4)-(6)). For each set of estimates, we first present results that are based on the overall variation in expellee inflows (columns (1) and (4)), and then results that are based only on the variation of expellee inflows within local labor markets (columns (2) and (5)) and between local labor markets (columns (3) and (6)).

In the first specification, we regress population growth between 1950 and 1970 on our key explanatory variable, the expellee inflow rate, and our set of covariates. As

Table 2.2: *Main Results*

	OLS			IV		
	Overall	Within	Between	Overall	Within	Between
	(1)	(2)	(3)	(4)	(5)	(6)
Inflow Expellees 1950	-0.311**	0.131	-0.671***	-0.498***	-0.060	-0.830***
	(0.140)	(0.124)	(0.202)	(0.130)	(0.123)	(0.155)
Pop.density 1939	-0.021***	-0.012***	-0.012***	-0.021***	-0.011***	-0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Share agriculture 1939	-0.682***	-0.660***	-0.339***	-0.659***	-0.633***	-0.303***
	(0.098)	(0.096)	(0.105)	(0.095)	(0.091)	(0.107)
Turnover p.c. 1935	-0.003	-0.099***	0.053**	0.003	-0.104***	0.055**
	(0.021)	(0.019)	(0.024)	(0.020)	(0.020)	(0.024)
Share of damaged dwellings	0.208**	0.415***	-0.103	0.139	0.365***	-0.155**
	(0.088)	(0.103)	(0.087)	(0.086)	(0.100)	(0.077)
0/1 Inner-German border	-0.129***	-0.038	-0.095***	-0.108***	-0.036	-0.074**
	(0.037)	(0.033)	(0.032)	(0.037)	(0.033)	(0.034)
R-squared	0.314	0.260	0.441	0.307	0.255	0.434
Observations	511	511	157	511	511	157
F-Statistic, excl. instrument				995.4	716.4	563.5
First-stage coefficient				0.924***	0.941***	0.946***
				(0.029)	(0.035)	(0.040)

Notes: The dependent variable is the change in population between 1950-70 over the population in 1939. Regression models (1) and (4) use the overall variation in the data, whereas models (2) and (5) uses only the variation within local labor markets, and models (3) and (6) the variation between local labor markets (see Section 2.3 for details). The IV regressions in columns (4) to (6) use the expellee inflow rate in 1946 as an instrument for the expellee inflow rate in 1950. Robust standard errors are in brackets. Standard errors are clustered at the level of local labor markets in models (1), (2), (4), and (5). *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

shown in column (1) of Table 2.2, the estimated coefficient on the expellee inflow rate is -0.311 with a standard error of 0.140. A one percentage point increase in a county's expellee inflow rate thus reduced subsequent population growth in 1950-70 by 0.311 percentage points. The result–based on the overall variation for West Germany at county level–suggests that there was some reversion to the pre-shock population distribution. Overall, therefore, counties subjected to a larger positive (negative) population shock in 1939-1950 tended to show lower (higher) average population growth in subsequent decades. However, the magnitude of reversion was limited, at least until 1970 and for West Germany as a whole.

In specifications (2) and (3), we decompose the total variation of the population shock into two components, a within local labor market component and a between local labor market component.

Specification (2) considers the deviation of variables from the labor-market-wide mean. Exploiting only variation between counties within the same local labor market provides evidence on the persistence of population shocks that differentially hit counties located in the same labor market. As shown in column (2), the estimated coefficient on our population shock measure turns statistically insignificant in our within regression (and is now, with 0.131, even positive). Thus, within local labor markets, the population shock appears to have been persistent, showing no sign of reversion.

In specification (3), we aggregate our county-level data to the local labor market level and then re-run our full-fledged model at this higher level of regional aggregation. This way, we exploit only variation between local labor markets. The point estimate of −0.671 indicates that between local labor markets, the initial population shock was, to a large degree, reversed in 1950-70. For any percentage point increase in the expellee inflow rate in 1950, subsequent population growth was reduced by 0.671 percentage points. Comparing the results of specifications (1) and (3) also highlights the importance of the unit of observation: Moving from counties to local labor markets more than doubles the absolute magnitude of the coefficient on the expellee inflow rate.

We next estimate IV regressions to alleviate concerns that some expellees might have endogenously moved by 1950 after moving restrictions were phased out in 1949. The IV regressions isolate the variation in inflow rates that is due only to the initial placement of expellees. Their results are shown in columns (4)-(6) of Table 2.2.

The first-stage results suggest that we do not have a weak instrument problem. The IV results generally confirm our OLS results although the IV estimates are more negative than the OLS estimates. First, when using the overall-variation (column (1) vs. (4)), the estimated coefficient is now −0.498, considerably smaller than the OLS estimate of −0.311. Second, the within estimator now turns negative to −0.060 (column (5)). However, the estimated coefficient is not statistically significant at any conventional level. The expellee-induced population shock did not induce lower population growth in 1950-70, implying a persistent effect on the spatial distribution of population *within* local labor markets. Finally, the negative point estimate of the between specification also decreases slightly from −0.671 in specification (3) to −0.830 in specification (6). The estimate implies that a 1 percentage point increase in population growth between 1939 and 1950 reduces population growth between 1950 and 1970 by 0.830 percentage points. The population shock hence had very little effect on the spatial distribution of population *between* local labor markets 25 years after the war.

We also estimated the within regressions of Table 2.2 separately for the British, French and American zones of occupation (see Table 2.B1 in the Appendix). We find little evidence of effect heterogeneity by zone of occupation. Treatment effects are all statistically insignificant, except in the IV regression for the American zone of occupation, in which we find an imprecisely estimated negative effect. Furthermore, we checked whether the use of a common denominator (1939 population) for the ratios used as dependent and independent variables may have introduced spurious correlation that would invalidate our estimates of the expellee effect. Re-estimating the main regressions from Table 2 for a dependent variable that normalizes the change in population between 1950-70 by the population in 1950 instead of 1939 produces results qualitatively identical to our main results reported in Table 2.2 (see Table 2.C1 in the Appendix).

2.4.2 Robustness Checks

We conduct several tests to assess the robustness of our IV results. Table 2.3 provides the results of these robustness checks, reproducing our main results–from columns (5) and (6) of Table 2.2–in Panel A.

First, we use alternative measures of wartime destruction and urbanization, our two key control variables. In our baseline analysis, we use the share of damaged dwellings as a measure of war destruction, and pre-war population density as a measure of urbanization. As a robustness check, we instead use rubble in 1945 per inhabitant in 1939 and a categorical variable that ranges from 1 "no destruction" to 4 "heavy destruction" as alternative measures for war destruction. We also use the share of population in bigger cities and dummies for the size of the largest city in the local labor market as alternative measures for urbanization (see Section 2.3 for details on the alternative controls). In a final step, we use all destruction and urbanization measures jointly. Panel B. of Table 2.3 shows that our results remain robust to the use of these alternative measures of war destruction and urbanization.

Second, we add different measures of pre-war population growth to our set of controls (population growth in 1871-1910, 1910-1939, 1925-1939, and population growth in all of these periods). Pre-treatment trends in population dynamics, if correlated with expellee inflows in 1950, may confound our estimates of the effect of expellees on post-war population dynamics. As shown in Panel C. of Table 2.3, however, our findings also prove robust to the addition of such controls. In fact, estimated treatment coefficients in the between specification, rather than being attenuated, increase in absolute magnitude, getting closer to minus one.

Third, we add controls for pre-war economic structure (see Panel D. of Table 2.3), i.e., controls for the 1939 sectoral employment structure (industry, services, trade, domestic services) and the 1939 occupational employment structure (blue-collar, white-

Table 2.3: *Robustness Checks - IV Results on Expellee Inflow Effect*

	Within (1)	Between (2)
A. Baseline regression	-0.060	-0.830***
	(0.123)	(0.155)
B. *Alternative control variables for destruction and urbanization*		
... using rubble per capita	-0.138	-0.781***
	(0.128)	(0.143)
... using loss in housing space (categorial)	-0.123	-0.791***
	(0.129)	(0.147)
... using population share in cities with at least of 10,000 inhabitants in 1939	-0.165	-0.789***
	(0.128)	(0.164)
... using dummies for size of largest city in the local labor market	-0.095	-0.847***
	(0.121)	(0.155)
... using all destruction and urbanization measures jointly	-0.105	-0.777***
	(0.128)	(0.168)
C. *Pre-war population trends*		
... adding population growth 1871-1910	-0.071	-0.834***
	(0.126)	(0.152)
... adding population growth 1910-1939	-0.002	-0.831***
	(0.133)	(0.154)
... adding population growth 1925-1939	0.006	-0.953***
	(0.155)	(0.159)
... adding population growth 1871-1910, 1910-1939, and 1925-1939	-0.023	-0.962***
	(0.154)	(0.154)
D. *Pre-war economic structure*		
... adding controls for sectoral employment structure 1939	0.073	-0.792***
	(0.150)	(0.188)
... adding controls for occupational employment structure 1939	-0.149	-0.759***
	(0.131)	(0.135)
... adding controls for sectoral and occupational employment structure	-0.060	-0.715***
	(0.156)	(0.134)
E. *Miscellaneous checks*		
... weighted with 1939 population	-0.067	-0.798***
	(0.114)	(0.148)
... without additional border adjustments	0.047	-0.920***
	(0.129)	(0.109)
... jointly estimated β_2 and β_3	0.050	-0.777***
	(0.139)	(0.127)

Notes: The table reports IV estimates of the effect of the expellee inflow rate in 1950 on population growth in 1950-1970. Each cell reports estimates from a separate regression, except for the coefficients in the last row of Panel E. The dependent variable is the change in population between 1950-70 over the population in 1939. Regression model (1) uses the variation within local labor markets, and model (2) uses the variation between local labor markets (see Section 2.3 for details). Each regression in Panel A., C., D. and E. includes our standard set of control variables, i.e., population density in 1939, the employment share in agriculture in 1939, turnover per capita in 1935, the share of damaged dwellings, and a dummy for counties within 75 km of the inner-German border. Regressions in Panel B. include our standard set of control variables but replace the standard covariates for wartime destruction and urbanization by alternative covariates. Regressions in Panel C. add different measures of pre-war population growth to the set of control variables. Regressions in Panel D. add controls for the 1939 sectoral employment structure (industry, services, trade, domestic services) and occupational employment structure (blue-collar, white-collar, civil servant, family co-worker, self-employment) to the set of control variables. The first regression in Panel E. estimates weighted regressions, using the 1939 population as weights. The second regression in Panel E. is our baseline regression applied to an adjusted sample of 548 counties, in which we only merge those counties that formed one county at any time between 1939 and 1970 (see Appendix 2.F). The third regression in Panel E. estimates the within and between coefficients jointly, see footnote 14. Robust standard errors are in brackets. Standard errors are clustered at the level of local labor markets in column (2). *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

collar, civil servant, family coworker, self-employment). Differences in pre-war economic structure, if systematically related to the scale of the expellee inflow in 1950, may again confound our relationship of interest. Controlling for the sectoral and occupational employment structure in 1939, however, does not change our findings.

Finally, we carry out a number of additional miscellaneous checks. First, we estimate weighted regressions, using population in 1939 as weights (see top row in Panel E. of Table 2.3). Second, we re-estimate our baseline regression for an adjusted sample of 548 counties, in which we only merge those counties that formed one county at any time between 1939 and 1970 (see second row in Panel E. and Appendix 2.F for further details). Finally, in the bottom row of Panel E. we estimate the within and between coefficients jointly (see the specification in footnote 14). Our results prove robust in all of these miscellaneous checks.

2.4.3 Net Migration Rate 1950-70

So far, we have considered the effect of the expellee inflow on post-war population growth. Our findings show that the migration-induced population shock had a persistent effect on the distribution of population within local labor markets, whereas the shock was largely reversed between labor markets. In this subsection, we document that these patterns reflect significant net migration flows occurring between but not within local labor markets. This observation will be important for the interpretation of our results in Section 2.5.

Specifically, we regress the net migration rate in 1950-70, defined as net migration in 1950-70 over population in 1939, on the expellee inflow rate in 1950 and our standard set of controls (see Table 2.4). As before, we run both OLS and IV regressions, exploiting either the overall, within, or between variation in expellee inflow rates. The estimated coefficients of the expellee inflow rate in 1950 have the same sign and are close in magnitude to the corresponding coefficients in our baseline regressions reported in Table 2.2. This suggests that post-displacement migration flows do indeed explain a very large share of the overall effect that expellee inflows had on post-war population growth in 1950-70, both overall and between local labor markets.[17] The estimated coefficient of the expellee inflow rate in 1950 in the IV within regression (column (5)), while marginally significant at the 10% level, is but a fifth in magnitude of that of the

[17]The net migration rate is one component of total population growth. The latter is made up of the sum of net migration and net natural changes of population. Since we normalize both population growth and net migration by population in 1939, coefficients in Tables 2.2 and 2.4 are directly comparable.

Table 2.4: *Expellee Inflows and Net Migration Rates 1950-70*

	OLS			IV		
	Overall	Within	Between	Overall	Within	Between
	(1)	(2)	(3)	(4)	(5)	(6)
Inflow Expellees 1950	-0.340***	0.012	-0.663***	-0.513***	-0.166*	-0.788***
	(0.128)	(0.101)	(0.159)	(0.119)	(0.095)	(0.125)
R-squared	0.424	0.425	0.574	0.418	0.420	0.568
Observations	511	511	157	511	511	157
F-Statistic, excl. instrument				995.4	716.4	563.5
First-stage coefficient				0.924***	0.941***	0.946***
				(0.029)	(0.035)	(0.040)

Notes: The dependent variable is net migration between 1950 and 1970 over the population in 1939. Each regression includes our standard set of control variables, i.e., population density in 1939, the employment share in agriculture in 1939, turnover per capita in 1935, the share of damaged dwellings, and a dummy for counties within 75 km of the inner-German border. Regression models (1) and (4) use the overall variation in the data, whereas models (2) and (5) use only the variation within local labor markets, and models (3) and (6) the variation between local labor markets (see Section 2.3 for details). The IV regressions in columns (4) to (6) use the expellee inflow rate in 1946 as an instrument for the expellee inflow rate in 1950. Robust standard errors are in brackets. Standard errors are clustered at the level of local labor markets in models (1), (2), (4), and (5). *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

corresponding coefficient estimate in the IV between regression (column (6)). Post-displacement migration flows hence depend much more heavily on variation in expellee inflow rates between than within local labor markets.

These results have implications also for the literature that studies the link between immigrant inflows and population outflows. In particular, we showed that expellee inflows and net population flows are much more strongly correlated between than within local labor markets. Previous work suggests that expellees were particularly mobile and thus responsible for a disproportionate share of population movements (Braun and Kvasnicka, 2014; Braun and Weber, 2016). Consistent with these earlier findings, Figure 2.E1 in the Appendix shows that the distribution of expellee population shares at county level was much less dispersed in 1961 than in 1950 (the standard deviation decreased from 0.093 in 1950 to 0.063 in 1961). Expellees were more equally distributed in 1961 than in 1950, as they moved in disproportionate numbers from regions with high expellee inflows to regions with low expellee inflows. One potential explanation for this empirical fact is that newly arrived expellees were less bound to specific regions than natives–and hence reacted stronger to regional differences in economic opportunities, in line with the hypothesis that ’immigrants grease the wheels of the labor market’ (Borjas, 2001).

2.4.4 Alternative Units of Observation

We conclude by highlighting once more–but in an alternative and more direct way of exposition that also considers an additional and larger regional unit than the local labor market–the importance of the unit of observation for the estimated effect of the expellee inflow on subsequent population growth. We have already shown that moving from counties to local labor markets as the unit of observation considerably increases the absolute magnitude of the coefficient on the expellee inflow rate in both OLS and IV regressions. Panels A. and B. of Table 2.5 reproduce these earlier results from Table 2.2 (columns (1), (3), (4), and (6)). Panel C. adds a third level of regional aggregation, and estimates our standard regression at the level of *Raumordnungsregionen*, of which there are 36 in post-war West Germany. *Raumordnungsregionen* are also based on a functional definition, but cover a larger set of counties than local labor market regions.

Table 2.5: *Alternative Units of Observation*

	OLS (1)	IV (2)
A. Counties (N=511)	-0.311**	-0.498***
	(0.140)	(0.130)
B. Local Labor Markets (N=157)	-0.671***	-0.830***
	(0.202)	(0.155)
C. Raumordnungsregionen (N=36)	-0.832***	-0.960***
	(0.188)	(0.187)

Notes: The table reports OLS and IV estimates of the effect of the expellee inflow rate in 1950 on population growth in 1950-1970. Each cell reports estimates from a separate regression. The dependent variable is the change in population between 1950-70 over the population in 1939. Control variables are population density in 1939, the employment share in agriculture in 1939, turnover per capita in 1935, the share of damaged dwellings, and a dummy for counties within 75 km of the inner-German border. Panel A. considers the 511 counties in West Germany, Panel B. the 157 local labor markets, and Panel C. the 36 *Raumordnungsregionen* in West Germany. Robust standard errors are in brackets. Standard errors in Panel A. are clustered at the level of local labor markets. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 2.5 shows that at each level of aggregation, the expellee inflow rate exerts a negative impact on population growth in 1950-70. Most importantly, however, the absolute magnitude of this effect increases considerably with the level of aggregation. It is lowest for counties (Panel A.), i.e., the smallest unit considered, and highest for *Raumordnungsregionen* (Panel C.), the largest aggregation level. Local labor markets

(Panel B.) fall in between these two, both in terms of aggregation level and in the absolute size of the estimated effect. The higher the level of aggregation, therefore, the less persistent proves the initial population shock. In fact, the IV coefficient estimate of -0.960 for *Raumordnungsregionen* suggests that at this largest aggregation level considered, the initial population shock was almost completely reversed by 1970.

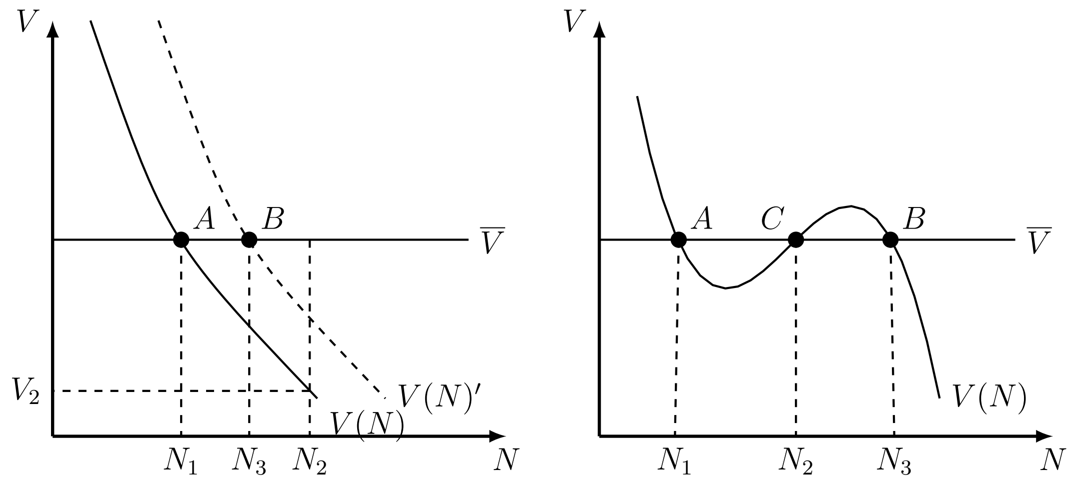
2.5 Interpretation

We now explore potential explanations for the high persistence of the population shock within but not across local labor markets. We interpret our findings through the lenses of the monocentric city model in the spirit of Alonso (1964), Mills (1967) and Muth (1969).[18] The model's distinction between the distribution of population *between* and *within* urban areas makes it a natural starting point for our purpose. We can think of labor markets as functional urban areas, which consist of a city and the surrounding periphery (Dijkstra et al., 2019). The periphery is integrated into the city's labor market through commuting. We first consider the distribution of population between and then within labor markets.

Between Labor Markets: Consider a single labor market within a system of many labor markets. Individuals in the labor market receive indirect utility $V(N)$ where N is the population of the labor market. In the standard monocentric model, $V(N)$ is strictly decreasing in N, as higher population drives up house prices without affecting the exogenously given wage. Costless migration between labor markets ensures that utility is the same in all labor markets and equal to the exogenous reservation utility $\overline{V}$. This spatial equilibrium condition is illustrated in Panel (a) of Figure 2.5. The equilibrium A with $(N_1, \overline{V})$ occurs at the intersection of the downward sloping indirect utility curve $V(N)$ with line $\overline{V}$. This first view predicts that temporary population shocks have no permanent effects. Suppose, for instance, that population increases from N_1 to N_2 (due to exogenous immigration). Indirect utility falls to V_2, inducing individuals to emigrate to other labor markets. Equilibrium is then restored in A.[19]

[18]We present only a stylized description of the underlying model. Interested readers might consult Brueckner (1987) or Fujita (1989) for a detailed description of the monocentric city model and Duranton and Puga (2014) for a recent review of key theories of urban growth. We focus on the open city case of the monocentric model where population is endogenous. The closed case treats population as exogenous and allows utility to adjust.

[19]For the sake of simplicity, we discuss the effect of immigration from the perspective of a single labor market. We thus abstract from the effects of system wide shocks that affect all labor markets in an economy. Our focus on a single labor market is clearly an oversimplification in our context.



(a) *Shifting Equilibrium* **(b)** *Multiple Equilibria*

Figure 2.5: *Local Labor Market Population following Immigration*

Even in this view, however, population shocks may permanently affect population patterns if they alter second nature geography, a point recently highlighted by Maystadt and Duranton (2018).[20] Suppose, for instance, that policy makers respond to immigration by investing in commuting infrastructure, thereby shifting the indirect utility function to $V(N)'$. The new unique equilibrium is now in B with $(N_3, \overline{V})$. As drawn, we would still observe emigration (of magnitude $N_2 - N_3$) but population does not revert back to its initial level.[21] This interpretation is consistent with our findings, as we observe strong but incomplete reversal of the initial population shock between labor markets.

The traditional view sketched so far highlights the costs of bigger labor markets. An alternative view stresses the productive benefits of larger labor markets in the form of agglomeration economies. For instance, interactions between workers may be more productive in thicker labor markets, so that wages increase in population. Population increases then have two opposing effects on individuals' utility, a negative one through higher congestion costs and a positive one through higher wages. If the latter effect dominates the former, indirect utility will increase with labor market size.

However, it is in line with the typical empirical specification in the literature, which studies the effect of shocks on the size or growth of individual spatial units (Brakman et al., 2004). Our specification in (2.1) is no exception in this regard.

[20] The paper shows that the temporary presence of refugees had permanent positive effects on hosting economies in Tanzania. The authors present evidence that this 'Big-Push' effect of refugees was due to subsequent investments in transport infrastructure rather than a switch to a new equilibrium in a setting with multiple equilibria.

[21] The monocentric model with endogeneous population predicts that lower commuting costs, higher wages, and lower agricultural rents increase city-wide population (Brueckner, 1987).

Panel (b) of Figure 2.5 illustrates such a case. As drawn, agglomeration economies dominate congestion costs for intermediate population levels (as in, e.g., Bleakley and Lin, 2012).[22] $V(N)$ now intersects $\overline{V}$ three times. A and B are stable equilibria, which are restored following small perturbations. The third equilibrium in C is unstable, as the labor market would move to either A or B following small perturbations away from C. Under agglomeration economies, population shocks can have permanent effects by shifting the labor market from one equilibrium to another. Suppose, for instance, that labor market equilibrium is in A with $(N_1, \overline{V})$. If immigration boosts population to beyond N_2, the labor market will permanently shift to B with $(N_3, \overline{V})$. In addition, we will observe out-migration if immigration increases population to beyond N_3. Importantly, these results hold without changes in second nature geography. Our result of incomplete population reversal at the level of local labor markets could thus also be interpreted as a shift between multiple equilibria.

Do our results point to a shift in the unique equilibrium induced by infrastructure investments or to the existence of multiple equilibria? While a conclusive answer is beyond the scope of the chapter, the development of the local road network in 1939-70 sheds some light on this question. Data are available for municipalities with at least 10,000 inhabitants (from various volumes of the *Statistical Yearbooks of German Municipalities*), though unfortunately not at county level. We distinguish between municipalities with 1950 expellee inflow rates above and below the median. Panel (a) of Figure 2.6 plots the difference in roads per capita between the two groups, along with the corresponding confidence intervals.[23] The figure illustrates that high- and low-inflow municipalities did not differ in the road network density (in meter per head) before the war. The effect of the expellee inflow is clearly visible in 1949, when road density per head was much lower in high-inflow municipalities. By 1970, however, the difference has disappeared.

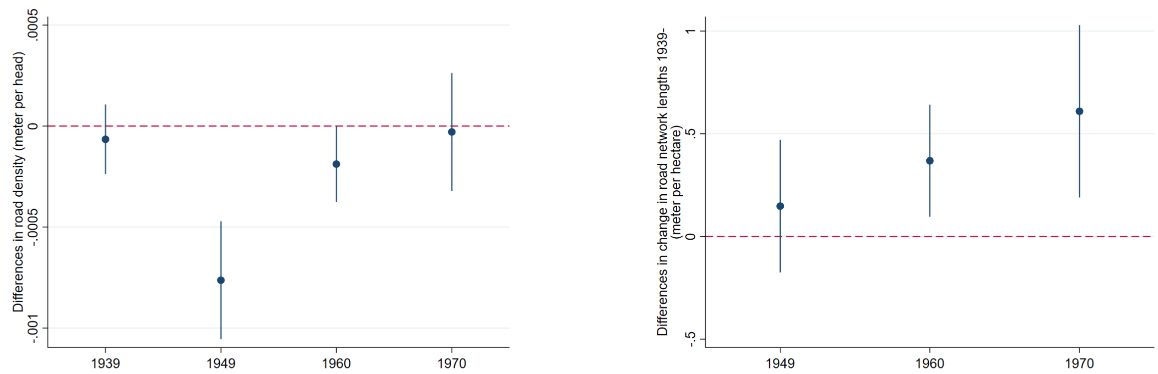
Panel (b) of Figure 2.6 shows that part of the adjustment process in 1949-70 was driven by more road construction in high-inflow municipalities (rather than population outflows). Between 1939 and 1970, the length of the road network increased by 6.1 meters per hectare more in high-inflow than in low-inflow municipalities (or by 46.9% relative to the control mean of 13.0). This gap in road construction only emerges after

[22] Bleakley and Lin (2012) discuss the plausibility of this shape of the indirect utility function in their footnote 27.

[23] We restrict our sample to the 152 municipalities for which data are available for all time periods. The underlying regression controls for log population in 1939 and land area. The results are also robust to adding indicators for war destruction, which are, however, not available for all municipalities in our sample. The results are also robust to dropping municipalities that absorbed other municipalities or settlements over time.

Figure 2.6: *Differences in Road Networks between Municipalities with High and Low Expellee Inflows*

(a) *Road Network Density* **(b)** *Net Road Construction since 1939*



Notes*: The figures depicts differences between municipalities with expellee inflow rates above and below the median in road density in meter per head (Panel (a)) and in the change in roads per hectare since 1939 (Panel (b)). Differences are estimated in regressions of the dependent variable on a dummy indicating whether a municipality's expellee inflow rate in 1950 is above or below the median inflow rate. Control variables are log population in 1939 and land area. Each point estimate is marked by a dot and stems from a separate regression. The vertical bands indicate the 95 percent confidence interval of each estimate.*

the expellee inflow. The results in Figure 2.6 are thus consistent with the idea that infrastructure investment acted as an equilibrium shifter.

Overall, our discussion suggests that the weak persistence of the population shock between labor markets is best understood as the result of migration-induced investments into road infrastructure. These investments shifted the equilibrium size of labor markets, but were insufficient to prevent out-migration from high- to low-inflow labor markets.

Within Labor Markets: Can we square this explanation for weak persistence between labor markets with our finding of strong persistence within labor markets? We argue that the persistent effect within labor markets reflects sub-urbanization, induced by road infrastructure investment. Since expellees arrived pre-dominantly in the labor market periphery (as we document below), initial inflows were not correlated with later population growth within labor markets. This was because the population shares in the labor market core and periphery in 1950 were already (largely) consistent with the new equilibrium, while the labor-market wide population level was not. We first illustrate the argument theoretically, and then provide suggestive empirical evidence.

Our theoretical discussion closely follows Duranton and Puga (2015), to which we

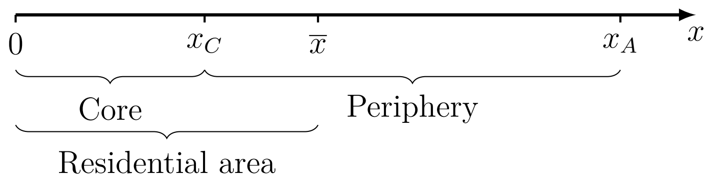


Figure 2.7: *Labor Market Core, Periphery and Residential Area*

refer the interested reader for details. Consider a linear monocentric labor market. As before, migration between labor markets equalizes utility to a common and exogenous level. All residents commute to a job at a single point $x = 0$, the central business district (CBD). Commuting costs, given by τx, increase linearly with distance to the CBD. All residents earn the same wage from employment in the CBD. This leaves $w - \tau x$ for expenditure on housing and a numeraire good. While the price of the numeraire good is the same everywhere, the rental price of housing varies with distance to the CBD. Housing is produced by a perfectly competitive construction industry, using land and capital under constant returns to scale. All individuals are identical and freely mobile. Therefore, they must derive a common utility level at the residential equilibrium.

The model predicts that in equilibrium, the price of units of housing increases as we move closer to the CBD. Centrally located residents economize on housing and inhabit smaller dwellings. The model thus highlights the fundamental trade-off between accessibility and space in residential choice. Higher housing prices close to the CBD are reflected in higher land prices, which in turn cause developers to build taller buildings. Consequently, population density increases as we move closer to the CBD due to a combination of taller buildings and smaller individual dwellings. Land is built upon if the rent in residential use, $R(x)$, is at least as high as the rent $\underline{R}$ in the next best alternative use, say agriculture. The edge of the residential area is thus located at an endogenously determined distance $x = \overline{x}$ from the CBD, such that $R(\overline{x}) = \underline{R}$.

Within the model, a reduction in local commuting costs τ, e.g. from an expansion of the road network, will increase total population, consistent with our previous discussion of labor market wide population. The population increase, which comes in response to the utility gain from lower commuting costs, drives up house prices everywhere. More expensive housing then offsets the utility gain and restores utility equalization between labor markets. The additional population is accommodated through two channels, rising densities and an expansion of the residential area. The model predicts that the second channel is the more important one, so that local infrastructure improvements increase the population share of the periphery.

To see this, define the labor market core as the segment between $x = 0$ and an

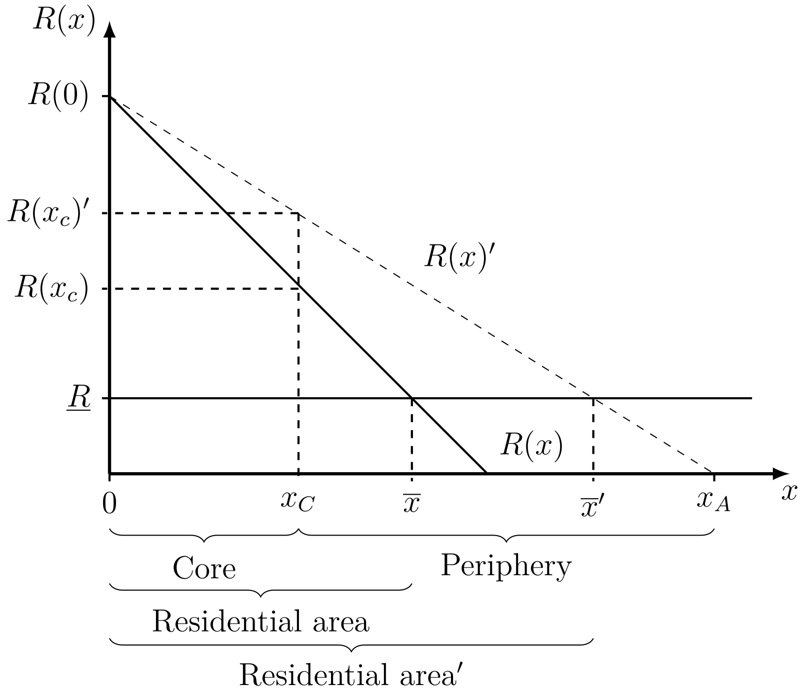


Figure 2.8: *The Effect of Lower Commuting Cost on Land Rents and the Edge of the Residential Area*

exogenous point x_C, and the periphery as the segment between x_C and the exogenous administrative border of the labor market x_A (where $x_C < \overline{x} < x_A$, see Figure 2.7). The extent of the residential area has to be sufficient to house the labor market population, i.e.,

$$N = \int_0^{\overline{x}} n(x)dx, \tag{2.4}$$

where $n(x)$ is population density. Following Duranton and Puga (2015), density can be expressed as $n(x) = -\frac{1}{\tau}dR(x)dx$. Let $N_P = \int_{x_C}^{\overline{x}} n(x)dx$ denote the (endogenous) population in the periphery. Using the expression for $n(x)$, one can solve for N and N_P:

$$N = \frac{R(0) - \underline{R}}{\tau}, \quad N_P = \frac{R(x_c) - \underline{R}}{\tau}, \tag{2.5}$$

where we have used that $R(\overline{x}) = \underline{R}$. The share of the total population located in the periphery is thus

$$\frac{N_P}{N} = \frac{R(x_c) - \underline{R}}{R(0) - \underline{R}}. \tag{2.6}$$

Figure 2.8, adapted from Duranton and Puga (2014), illustrates the effect of lower commuting costs on population shares in the core and periphery. It plots land rents $R(x)$ as a function of distance to the CBD before (solid line) and after (dashed line) the decline in τ. The intersection of $R(x)$ with $\underline{R}$ determines the edge of the residential area. The fall in τ causes land rents to increase everywhere except at $x = 0$ where residents do not benefit directly from lower commuting costs (and immigration keeps utility unchanged). The shift in land rents pushes out the edge of the residential area from $\overline{x}$ to $\overline{x}'$ but leaves the land rent at the edge unchanged at $\underline{R}$. Equation (2.6) then implies that the share of population in the periphery increases after a fall in τ. Better commuting infrastructure increases the share of land built on in the periphery, thereby boosting sub-urbanization.

In summary, immigrant inflows, by inducing infrastructure improvements, can cause a permanent increase in the population share of the periphery. If immigrants arrive mainly in the periphery, as is the case in our setting, the initial inflows might not correlate with subsequent population growth within labor markets. This is because the migration-induced change in the population shares in core and periphery might already be consistent with the post-migration equilibrium shares. This argument does not preclude emigration from high- to low-inflow labor markets. It just requires these emigration flows to not originate disproportionally from the periphery.

We conclude our discussion by providing three pieces of suggestive evidence that are consistent with our interpretation. First, within a labor market, expellees arrived disproportionally in counties that belonged to the labor market periphery rather than the labor market core.[24] The average difference between the inflow rate in the periphery and core is 9.2 percentage points in our data (relative to a labor-market-wide inflow rate of 25.3 percent).

Second, the differential expellee inflow rates had a persistent positive effect on a labor market's suburbanization rate, as measured by the population share in the periphery. Table 2.6 reports the results from regressing, at the level of labor markets, the change in the suburbanization rate in 1939-1950 (column (1)) and 1939-70 (column (3)) on the difference in the expellee inflow rate between periphery and core, and our usual control variables. The IV regressions in columns (2) and (4) instrument the differential expellee inflow rate in 1950 with that in 1946. The coefficient estimate of 0.235 in the IV regression in column (2) suggests that a one standard deviation increase in the differential expellee inflow rate (s.d. of 0.135) increased the change in

[24] We classify counties in our data as belonging to the labor market core if they encompass the labor market center (*Arbeitsmarktmittelpunkt*), as listed in IfW (1974). All other counties are classified as periphery. The classification is likely to underestimate the true difference between core and periphery, as counties in the core often encompass both the labor market center and parts of the periphery. We drop the 53 (out of 157) labor markets for which all counties belong to the labor market core or no core could be identified.

Table 2.6: *Difference in Expellee Inflow Rates and Sub-urbanization*

	1939-50		1939-70	
	OLS (1)	IV (2)	OLS (3)	IV (4)
Difference in expellee inflow rate 1950	0.241***	0.235***	0.171***	0.153***
(Periphery - Core)	(0.022)	(0.025)	(0.029)	(0.031)
Pop.density 1939	-0.001**	-0.002***	-0.002**	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Share Agriculture 1939	-0.040	-0.042	-0.100***	-0.105***
	(0.031)	(0.030)	(0.034)	(0.033)
Turnover p.c. 1935	0.004	0.004	0.010*	0.010*
	(0.005)	(0.005)	(0.006)	(0.006)
Share of damaged dwellings	0.079***	0.078***	0.099***	0.097***
	(0.024)	(0.023)	(0.028)	(0.028)
0/1 Inner-German border	-0.004	-0.004	0.000	0.002
	(0.006)	(0.006)	(0.008)	(0.008)
R-squared	0.684	0.684	0.635	0.634
Observations	104	104	104	104
F-Statistic, excl. instrument		323.3		323.3
First-stage coefficient		0.939***		0.939***
SE		(0.052)		(0.052)

Notes: The dependent variable is the change in the suburbanization rate in 1939-50 (columns (1) and (2)) and 1939-70 (columns (3) and (4)). Suburbanization is defined as the population of a labor market residing in peripheral counties. We exclude the 53 labor markets, for which all counties belong to the core or no core can be identified. The IV regressions in columns (2) and (4) use the difference in the expellee inflow rate in 1946 as an instrument for the difference in the expellee inflow rate in 1950. Robust standard errors are in brackets. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

the suburbanization rate in 1939-50 by 0.68 standard deviations (s.d. of 0.047). While the coefficient estimate decreases somewhat for suburbanization in 1939-70, it remains positive, statistically significant and economically meaningful (at roughly two-thirds of the size for 1939-50).

Third, we continue to find population shocks to be persistent within labor markets also when distinguishing only between core and periphery. Specifically, we re-run our main regression using the variation in expellee inflows within labor markets, but now aggregate counties in the core and periphery. We thus have at most two observations per labor market, one for the core and one for the periphery. Table 2.7 shows the resulting OLS and IV regression results in columns (3) and (4), while reproducing our original regression results in columns (1) and (2) (from Table 2.2, columns (2) and (5)). Results are very similar to our baseline estimates. In particular, expellee-induced

Table 2.7: *Within Labor Markets Regression Results for Core-Periphery Classification*

	County		Core-Periphery	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Inflow Expellees 1950	0.131	-0.060	0.034	-0.159
	(0.124)	(0.123)	(0.207)	(0.211)
Pop.density 1939	-0.012***	-0.011***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)
Share agriculture 1939	-0.660***	-0.633***	-0.607***	-0.582***
	(0.096)	(0.091)	(0.149)	(0.137)
Turnover p.c. 1935	-0.099***	-0.104***	-0.062***	-0.065***
	(0.019)	(0.020)	(0.019)	(0.020)
Share of damaged dwellings	0.415***	0.365***	0.098	0.065
	(0.103)	(0.100)	(0.117)	(0.128)
0/1 Inner-German border	-0.038	-0.036	0.017	0.024
	(0.033)	(0.033)	(0.043)	(0.040)
R-squared	0.260	0.255	0.325	0.318
Observations	511	511	261	261
F-Statistic, excl. instrument		716.4		312.6
First-stage coefficient		0.941***		0.896***
		(0.035)		(0.051)

Notes: The dependent variable is the change in population between 1950-70 over the population in 1939. All regression models use only the variation within local labor markets (see Section 2.3 for details). Models (1) and (2) are estimated on the 511 counties. Models (3) and (4) are estimated on aggregated data, which aggregates all counties in the core of a labor market and all counties in the periphery. The IV regressions in columns (2) and (4) use the expellee inflow rate in 1946 as an instrument for the expellee inflow rate in 1950. Robust standard errors clustered at the level of local labor markets are in brackets. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

population shocks, which differentially affected core and periphery, had no statistically significant effect on within labor market population growth in 1950-70.

2.6 Conclusion

This chapter has explored the importance of local labor markets for the persistence of a major population shock, the inflow of eight million expellees to different parts of West Germany after World War II. Our results show that the estimated regional persistence of this shock depends crucially on the type of regional unit considered and the type of variation in expellee inflows exploited. The population shock proved persistent within local labor markets, but was largely reversed between labor markets. We argue that the persistent effect within labor markets is best understood as a relative decline of the labor market core, caused by migration-induced investments into transport infrastructure. These investments also shifted the equilibrium size of labor markets but were not sufficient to prevent emigration from labor markets with high initial expellee inflows.

Our findings suggest that the choice of the regional unit should be carefully motivated when drawing conclusions from the persistence of population shocks about the determinants of the spatial distribution of economic activity. This is because these determinants are likely to differ between and within labor markets. This simple insight can also help to better understand the disparate findings in the literature on the persistence of population shocks. Early seminal work in the literature typically focused on cities as spatial units to discriminate between explanations for the distribution of economic activity (Davis and Weinstein, 2002; Brakman et al., 2004). Later work, for instance by Schumann (2014), often focused on municipalities, of which many are located in the same labor market. Our findings suggest that the results from these two bodies of literature are difficult to compare because the determinants of spatial equilibrium tend to differ between and within labor markets.

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Appendix

2.A Balancing Test on 1939 Covariates

We carried out a balancing test on 1939 variables for counties with expellee inflow rates above and below the median (see Table 2.A1). The table shows, as expected, pronounced unconditional differences between high- and low-inflow regions, which we described already in Section 2.2 when discussing the historical background to our setting. Among other differences, high-inflow regions are less urban, have more employment in agriculture, and less employment in industry. However, the table also shows that these differences decline markedly, and most of the time disappear altogether, when we condition on war destruction. Apart from geographical factors, differences between high- and low-inflow regions are hence driven primarily by war destruction and the associated availability of housing. Conditional on such destruction, remaining differences are generally minor at best.

Table 2.A1: *Balancing Test on 1939 Covariates–High and Low Inflow Counties*

	High inflow	Low inflow	Unconditional difference	Conditional difference
	(1)	(2)	(3)	(4)
Pop. density 1939	1.747	6.952	-5.205***	0.264
	(3.183)	(8.924)	[0.765]	[0.493]
Pop. share in cities ≥ 10,000 inhabitants 1939	0.147	0.450	-0.303***	-0.029
	(0.301)	(0.434)	[0.039]	[0.037]
Turnover p.c. 1935	1.183	1.598	-0.415***	0.066
	(0.576)	(1.035)	[0.080]	[0.076]
Share of damaged dwellings	0.068	0.305	-0.236***	-
	(0.069)	(0.247)	[0.020]	
0/1 Inner-German border	0.424	0.109	0.314***	0.303***
	(0.495)	(0.313)	[0.054]	[0.059]
Sectoral employment structure 1939 (shares):				
Agriculture	0.479	0.282	0.197***	0.056**
	(0.202)	(0.225)	[0.023]	[0.024]
Industry	0.303	0.423	-0.120***	-0.056***
	(0.122)	(0.149)	[0.019]	[0.018]
Private services	0.079	0.102	-0.024***	0.000
	(0.055)	(0.073)	[0.005]	[0.007]
Trade and transport	0.109	0.153	-0.043***	0.000
	(0.067)	(0.076)	[0.006]	[0.007]
Domestic services	0.030	0.040	-0.010***	-0.001
	(0.016)	(0.018)	[0.002]	[0.002]
Occupational employment structure 1939 (shares):				
Blue collar worker	0.395	0.473	-0.078***	-0.016
	(0.108)	(0.126)	[0.015]	[0.015]
White collar worker	0.072	0.120	-0.048***	-0.005
	(0.048)	(0.070)	[0.005]	[0.006]
Helping family member	0.303	0.200	0.103***	0.015
	(0.124)	(0.143)	[0.014]	[0.015]
Civil servant	0.041	0.054	-0.013***	0.001
	(0.031)	(0.037)	[0.003]	[0.003]
Self employed	0.189	0.153	0.036***	0.005
	(0.044)	(0.050)	[0.006]	[0.006]

Notes: The table compares the characteristics of regions with expellee shares above the median (high inflow regions) and regions below the median (low inflow regions). Columns (1) and (2) report the mean of each characteristic. Columns (3) and (4) report unconditional and conditional differences between high and low inflow regions, respectively. The conditional difference in column (4) is the coefficient on a dummy for high inflow regions in regressions that control for the share of damaged dwellings. Standard deviations are in parentheses, robust standard errors clustered at the level of local labor markets are in squared brackets. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

2.B Within Regression Results by Zone of Occupation

Table 2.B1: *Within Regression Results by Zone of Occupation*

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Inflow Expellees 1950	-0.036	-0.107	0.260	-0.090	0.043	-0.496*
	(0.175)	(0.286)	(0.276)	(0.164)	(0.352)	(0.261)
R-squared	0.153	0.341	0.350	0.152	0.340	0.312
Observations	165	81	265	165	81	265
Occupation zone	British	French	American	British	French	American
F-Statistic				658.3	108.1	346.7
First-stage coefficient				0.968	0.852	0.909
				0.0377	0.0819	0.0488

Notes: The table shows results of re-estimating the OLS and IV within regressions in Table 2.2 separately for the British, French and American zones of occupation. The dependent variable is the change in population between 1950-70 over the population in 1939. Each regression includes our standard set of control variables, i.e., population density in 1939, the employment share in agriculture in 1939, turnover per capita in 1935, the share of damaged dwellings, and a dummy for counties within 75 km of the inner-German border. Robust standard errors clustered at the level of local labor markets are in brackets. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

2.C Regression Results for Alternative Dependent Variable

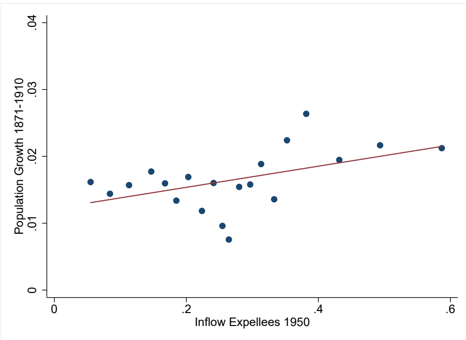
Table 2.C1: *Main Regression Results for Dependent Variable: (Pop. 1970 - Pop.1950)/Pop.1950*

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Inflow Expellees 1950	-0.334***	0.025	-0.560***	-0.447***	-0.072	-0.669***
	(0.088)	(0.081)	(0.120)	(0.084)	(0.086)	(0.095)
Pop.density 1939/100	-0.016***	-0.010***	-0.010***	-0.016***	-0.010***	-0.010***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Share Agriculture 1939	-0.464***	-0.430***	-0.247***	-0.450***	-0.416***	-0.223***
	(0.068)	(0.065)	(0.078)	(0.065)	(0.062)	(0.079)
Turnover p.c. 1935	0.018	-0.057***	0.055***	0.021	-0.060***	0.056***
	(0.014)	(0.013)	(0.019)	(0.014)	(0.014)	(0.019)
Loss in housing space (cont.)	0.247***	0.504***	-0.011	0.205***	0.479***	-0.047
	(0.067)	(0.081)	(0.066)	(0.067)	(0.078)	(0.062)
0/1 Inner-German border	-0.089***	-0.029	-0.079***	-0.076***	-0.028	-0.065***
	(0.024)	(0.023)	(0.022)	(0.024)	(0.022)	(0.022)
R-squared	0.409	0.335	0.573	0.405	0.332	0.567
Observations	511	511	157	511	511	157
F-Statistic, excl. instruments				995.4	716.4	563.5
First-stage coefficient				0.924***	0.941***	0.946***
				(0.029)	(0.035)	(0.040)

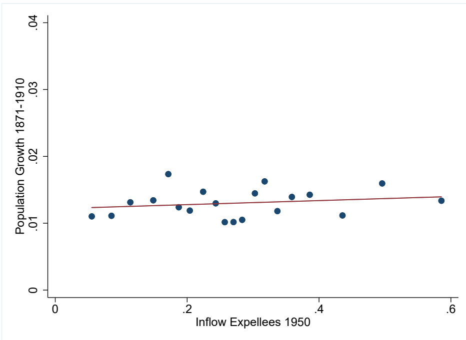
Notes: The table shows results of re-estimating the regressions in Table 2.2 for a slightly changed dependent variable, the change in population between 1950-70 over the population in 1950. Otherwise, specifications are identical to those in Table 2.2. Regression models (1) and (4) use the overall variation in the data, whereas models (2) and (5) uses only the variation within local labor markets, and models (3) and (6) the variation between local labor markets (see Section 3 for details). Robust standard errors are in brackets. Standard errors are clustered at the level of local labor markets in models (1), (2), (4), and (5). *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

2.D Binned Scatter Plots – Expellee Inflows and Pre-war Population Growth

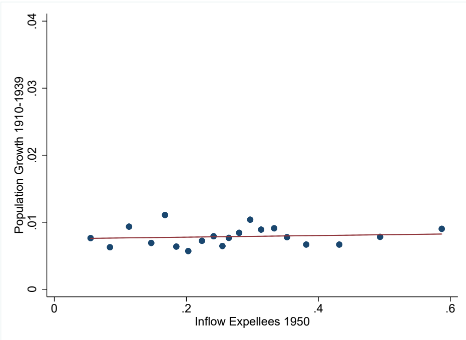
Figure 2.D1: *Binned Scatter Plots (Conditional)*



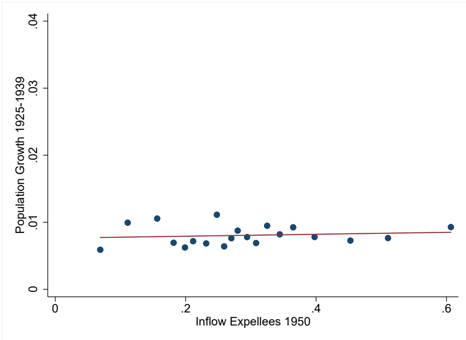
(a) *Population growth 1871-1910 (all counties)*



(b) *Population growth 1871-1910 (98% sub-sample)*



(c) *Population growth 1910-1939*



(d) *Population growth 1925-1939*

Notes*: The figures in Panel (a), (b), (c) and (d) depict binned scatter plots of residualized population growth in 1871-1910 (Panel (a) and (b)), 1910-1939 (Panel (c)), and 1925-1939 (Panel (d)) and residualized expellee inflow rates in 1950, grouping expellee inflow rates into 20 equal-sized bins. The 98% sub-sample considered in Panel (b) excludes the top 11 (2% of) counties with the fastest population growth in the period 1871-1910. Covariates include our standard set of control variables, i.e., population density in 1939, the employment share in agriculture in 1939, turnover per capita in 1935, the share of damaged dwellings, and a dummy for counties within 75 km of the inner-German border. See Table 2.1 in the main text for the corresponding regression results.*

2.E Kernel Density Estimates of Expellee Share in 1950 and 1961

Figure 2.E1: *Kernel Density Estimates*

Notes: The figure shows Kernel density estimates of the expellee population share at county level on 17 September 1950 (solid line) and 6 June 1961 (dashed line).

2.F Merging of Counties

The administrative borders of some West German counties changed between 1939 and 1970. In order to make county borders comparable over time, we follow the procedure outlined in Braun and Dwenger (2019) (Appendix A) for changes between 1939 and 1950. We replicate their description in the following and extend the list of counties merged to also account for border changes between 1950 and 1970.

We first merge counties which, at any time between 1939 and 1970, formed one county. The counties of Hildesheim and Marienburg, for instance, were separate entities in 1939, but were merged to join the new county of Hildesheim-Marienburg in 1946. Consequently, the 1946 and 1950 censuses only contain data on Hildesheim-Marienburg. We thus merge Hildesheim and Marienburg already in the 1939 census. We proceed analogously for the counties of Bremerhaven and Wesermünde; city and rural districts of Bremen; Rhein-Wupper Kreis and Leverkusen; Kreis der Eder, Kreis des Eisenberges and Kreis der Twiste; city and rural districts of Konstanz; Coburg and Rodach bei Coburg; city and rural districts of Dinkelsbühl; city and rural districts of Donauwörth; city and rural districts of Göttingen; Gifhorn and Wolfsburg; Kempen-Krefeld and Viersen; city and rural districts of Herford; city and rural districts of Lüdenscheid; city and rural districts of Siegen.

In addition, there were some smaller border changes, in which municipalities were moved from one county to another. To deal with these border changes, we first compare the 1939 population of each county in its 1950 borders to the 1939 population of the same county in its 1939 borders. Since the majority of administrative borders remained unchanged between 1939 and 1950, the 1939 population figure is usually the same regardless of whether we use 1939 or 1950 borders. Moreover, we do not take any action if the difference between the two population figures is less than 5%. If the difference is larger than 5%, we merge the counties that exchanged municipalities. This applies to the counties of Osterholz, Verden and Bremen; Bergstraße, city and rural districts of Worms; Goslar, Wolfenbüttel and Salzgitter; Mainz, Groß-Gerau and Wiesbaden; Böblingen, Eßlingen and Stuttgart; city and rural districts of Osnabrück; city and rural districts of München; city and rural districts of Kulmbach; Lörrach and Neustadt; Norden and Emden; Braunschweig and Peine; city and rural districts of Erlangen; Sinsheim and Heilbronn; city and rural districts of Schwabach; Grevenbroich and Kempen-Krefeld; Bonn and Rhein-Siegkreis; Bielefeld, Paderborn and Wiedenbrück; Detmold and Höxter; Hamm and Unna; Meschede and Olpe; Beckum and Soest; city and rural districts of Ingolstadt.

Finally, we drop counties that have lost or gained more than 5% of its 1939 population to regions outside West Germany, in particular to counties in the Soviet Occupation Zone. These counties include Blankenburg (Rest); Helmstedt; Birkenfeld; Zweibrücken; Saarburg; Trier; Mellrichstadt; Osterode; rural and city districts of Lüneburg.

2.G Data sources

Table 2.G1: *Data Sources*

Variable	Description and data source
Dependent variables	
Population growth 1950-70	Population change 1950-70 over population in 1939, based on Statistisches Bundesamt (1974). Data on 1970 population for Schleswig-Holstein come from Statistsches Landesamt Schleswig-Holstein (1971) and for Rhineland Palatinate from Statistisches Landesamt Rheinland-Pfalz (1967) as well as Statistisches Landesamt Rheinland-Pfalz (1968).
Migration rate 1950-70	Net migration 1950-70 over population in 1939, based on Statistisches Bundesamt (1974).
Main explanatory and instrumental variable	
Expellee inflow rate 1950	Expellees in 1950 over the population in 1939, based on Statistisches Bundesamt (1952).
Expellee inflow rate 1946	Expellees in 1946 over the population in 1939, based on Statistisches Amt des Vereinigten Wirtschaftsgebietes (1950).
Control variables	
Share of damaged dwellings	Share of dwellings built before 1945 that were damaged in the war, based on Statistisches Bundesamt (1956).
Rubble per capita	Untreated rubble at the end of the war over the population in 1939, based on Deutscher Städtetag (1949).
Loss in housing space	Classifies the loss in housing space in four categories, ranging from 'no losses' (1) to 'very substantial losses' (4). This indicator is taken from Institut für Raumforschung (1955).
Pop. density 1939	Population in 1939 (in 100) per square kilometer, based on Statistisches Bundesamt (1974).
Population share in cities with at least 10,000 inhabitants in 1939	The 1939 share of population living in cities with at least 10,000 inhabitants, based on Statistisches Reichsamt (1940).
Dummies for size of largest city in the local labor market in 1939	Dummies for counties that are located in a local labor market with a city of between 100,000 and 250,000 inhabitants and more than 250,000 inhabitants in 1939, based on Statistisches Reichsamt (1940).
Share agriculture 1939	The share of the workforce in agriculture in 1939, based on Statistisches Reichsamt (1943). Additional controls for the sectoral and occupational employment structure are also based on Statistisches Reichsamt (1943).
Turnover p.c. 1935	Turnover in 1935, taken from Statistisches Reichsamt (1939), over the workforce in 1939, taken from Statistisches Reichsamt (1943).
0/1 Inner-German border	Dummy for whether a county is located within 75 kilometers of the inner-German border.
Population growth 1871-1910 (1910-39, 1925-39)	Population change 1871-1910 (1910-39, 1925-39) over the population in 1871 (1910, 1925), based on various publications of the statistical agencies of the federal states.

Chapter 3

Universities and firm performance: Evidence from Germany

3.1 Introduction

Around the globe, politicians try to imitate successful industry clusters such as Silicon Valley, the Golden Triangle between Oxford, Cambridge, and London, or Israel's Silicon Wadi. Often governors and county commissioners emphasise the importance of universities as incubators for successful firms and thus economic development within their regions. There are two main arguments why proximity to a university is beneficial for firm performance. The first argument is that universities provide human capital via educating high-skilled employees. Many graduates stay in their university regions and work for local companies. The second argument is that universities provide spatially limited knowledge spillovers because they are more likely to cooperate with local industries. Often both arguments are intertwined.

However, various studies focus either on one or the other argument. The literature about universities' supply of human capital finds positive effects of this mechanism, which however vary in size. For example, Abel and Deitz (2012) show that there are positive, but only small direct effects from graduates of US universities on local human capital. In contrast to their findings Amendola et al. (2020) emphasise the importance of the local labour supply for the economic impact of Italian universities. With respect to universities' knowledge spillovers Kantor and Whalley (2014, 2019), Rondé and Hussler (2005), or Abramovsky and Simpson (2011) find significant positive effects.

Based on these studies about the specific effects of universities there is a growing strand of research that tries to capture the overall effect of universities on the local economy. Valero and Van Reenen (2019) analyse the overall impact of universities around the globe, Cermeño (2019) of US universities, and Andersson et al. (2009) of Swedish universities. All three studies analyse multiple aspects of universities but emphasise the positive impact of high-skilled local labour supply.

This chapter contributes to the debate about universities' general impact by showing empirical evidence for the impact of universities on firms in Germany. My research question is whether firms in vicinity of universities generate more revenues per employee.[1] In line with the literature, my identifying assumption is that universities have a stronger effect on firms in their home region compared to firms located further away (Glaeser et al., 1992, 1127). The analysis is based on the universe of firms and universities in Germany, which is provided by the Federal Statistical Office of Germany for the years 2013 until 2017. The firm data is stored in the Unternehmensregisterstatistik, which is the basis for the better-known AfiD-Panel data. To the best of my knowledge, this is the first empirical analysis about the impact of universities employing a comprehensive sample of firms in a large country.

As part of my analysis, I address the main channels of universities' impact via knowledge spillovers and high-skilled local labour supply. The institutional setting of German universities is suited rather well to do so since these universities can be grouped into those focused on teaching and thus high-skilled local labour supply and those additionally focused on research and thus knowledge spillovers. My findings support previous results about the importance of local labour supply from other European universities (e.g. Amendola et al. 2020).

Furthermore, I analyse whether universities affect the performance of multinational and national enterprises equally. Multinational enterprises differ from purely national ones in various aspects. They are more productive, have a more ramified firm network, and just like universities, they are more often located in larger cities. Analysing the effects of universities on multinational and national enterprises might shed light on the mechanism how universities impact firm performance and the reasons for the better performance of multinational enterprises.

Summarising, I find that firms located in vicinity of a university generate 0.92 % more revenues per employee compared to firms located in counties without a university. To deal with concerns about reverse causality I follow the literature and focus on a specific group of historical universities, which were founded for political reasons (see

[1] Henceforth, I will use the terms firm performance and firms' revenues per employee interchangeably.

Andrews 2020; Kantor and Whalley 2019 or Liu 2015). In the case of Germany, I focus on those universities which were founded during the 1960s and 1970s mainly to ensure equal access to tertiary education (see Andersson et al. 2009; Valero and Van Reenen 2019, iv). I identify the provision of high-skilled local labour supply as a key driver of universities' impact on firm performance.

The remainder of the chapter is structured as follows: Section 3.2 reviews the existing literature about universities' economic impact. Section 3.3 presents the data and the empirical framework of the analysis. Section 3.4 discusses the results and section 3.5 concludes.

3.2 Literature review

As indicated in the Introduction there is a large academic literature about the impact of universities on firm performance, local GDP, or tax revenues. In this review, I will focus on those empirical studies about the impact of universities with an econometric approach.[2] Most of these studies focus on one of two mechanisms: the provision of knowledge spillovers on the one hand and the provision of high-skilled local labour supply on the other hand.

The strand of literature investigating the economic impact of universities via knowledge spillovers can be subcategorised into four different approaches. The first approach utilises changes in institutional rules about property rights for the results of research, e.g. patents. Hausman (2022) analyses the Bayh-Dole Act in the US, which incentivised universities to patent their researchers' results. Using a spatial equilibrium, where firms gain productivity through proximity to the knowledge spillovers from universities, she is able to show that the number of employees grew faster in university regions after the passing of the Bayh-Dole Act. Hvide and Jones (2018) document for Norway how the end of professors' full rights to their innovations led in university regions to a decline in entrepreneurship and patenting rates. For Germany, Cunningham et al. (2019) show that a reform of professor's rights to their innovations had positive initial effects on patenting and entrepreneurship.

The second group of studies estimates the impact of universities via knowledge spillovers in broader political or historic contexts. In a study about Germany, Dittmar and Meisenzahl (2022) show that especially during the time of industrialisation eco-

[2] Many other studies are either survey-based (e.g. Harris et al. 2013) or focus on the idea of Keynesian multipliers and a catalytic impact (for Germany e.g. Pavel 2008; Glückler et al. 2015 or Janzen et al. 2022). For a broader overview of studies with other methodological approaches see Drucker and Goldstein (2007).

nomic activity surged in university towns. Kantor and Whalley (2019) use the political decision to establish agricultural experiment stations throughout the US at the end of the 19th century. They find positive impacts on the total factor productivity of farms located in the vicinity of these experiment stations for the years before 1920. After the 1920s lower transport and communication costs eased spatial frictions. In another setting, Kantor and Whalley (2014) construct an instrumental variable approach based on the fact that US universities spend a given fraction of their endowment values on research. Using market shocks they show that rising university expenditures lead to an increase in local wage rates.

The third group of studies about the impact of universities via knowledge spillovers uses the so-called knowledge production function, which has been developed by Griliches (1979) and Jaffe (1989). The knowledge production function assumes that research output, which is often measured by patents, can be modelled as a function of spending on applied research and development (R&D) and basic research within the same region. Various studies, e.g. Fischer and Varga (2003) for Austria, Andersson and Ejermo (2004) for Sweden or Rondé and Hussler (2005) for France, show that applied R&D spending is more efficient in terms of patent provision when located close to an institution of basic research like a university. Another study by Maietta et al. (2017) shows mixed results for the European food and drinks industry. Especially proximity to institutions of academic excellence is insignificant for firms in this industry. For Germany, Audretsch and Lehmann (2005) show in an adapted version of the knowledge production function that firms in high-tech industries are more productive when they are located close to technical universities.

The fourth and largest group of studies about the impact of universities via knowledge spillovers focuses on direct university-industry cooperation. The respective literature is wide-ranging, but Vivas and Barge-Gil (2015), Perkmann et al. (2013), and Rybnicek and Königsgruber (2019) provide helpful reviews. A key finding of the literature is the importance of geographic proximity for university-industry cooperation to take place. Abramovsky and Simpson (2011) show for the UK that a distance of more than 25 km between a firm and the respective university limits the probability for cooperation.[3] However, the distance is less important for firms with a higher absorptive capacity, i.e. a high-skilled workforce (Bodas-Freitas et al., 2014), and firms located in an innovative industrial cluster (D'Este et al., 2013).

Next to the literature about universities' impact on regional firms via knowledge

[3] This distance is comparable to the radius of an average German county, the main spatial unit in this analysis.

spillovers the second strand of literature deals with universities' impact via the provision of human capital and local labour supply. Davis and Dingel (2019) argue in a theoretical model that cities with more learning opportunities attract high-skilled workers and become more productive. This can be shown for the example of Italian university graduates. Amendola et al. (2020) use a national tax to finance universities as an instrument for human capital production. They find that university graduates have a significant positive impact on a region's GDP per capita. For Finland, Toivanen and Väänänen (2016) show in an instrumental variable setting how university regions benefit from the education of engineers in terms of patents. Andrews (2020) finds similar results for the US. Comparing the locations of US colleges via propensity score matching with "runner up" locations, he concludes that college regions show significantly more patenting activity. Andrews emphasises that this effect is not only driven by college graduates, but also by high-skilled migrants. This is in line with the findings of Abel and Deitz (2012). In addition, these direct and indirect effects can have further consequences. For example, Moretti (2004) uses land-grant colleges as an instrument for regional human capital to show its positive impact on college and high-school graduates' wages alike. One specific channel how universities can affect firm performance is demonstrated by Feng and Valero (2020), who find a positive impact of proximity between firms and universities on management practices.

Building upon the two strands of literature, which focus either on knowledge spillovers or on local labour supply to analyse the effect of universities, there is a rather new group of studies, that analyses the overall effect of universities. My own research is most closely related to this comprehensive approach. Agasisti et al. (2019) investigate the impact of Italian universities using an instrumental variable approach based on universities' funding structure. They were able to demonstrate the importance of universities' efficiency with regard to successful graduates and research output and its positive impact on regional GDP per capita. Andersson et al. (2009) analyse the Swedish decentralisation policy for tertiary education, which started in the late 1970s. Using previous academies and teaching facilities as instrumental variables they find significant positive effects of new universities on productivity in their home regions and patenting rates. For the US, Cermeño (2019) uses propensity score matching and a difference-in-differences approach to estimate the impact of new universities established during the 20th century. Her results show that university counties saw stronger population growth and an increase in GDP. Yet, the effects were much smaller if investments in local infrastructure were lacking. On a broader scale, Valero and Van Reenen (2019) look at universities in various countries and analyse knowledge spillovers, local labour supply, and a rise in pro-democratic attitudes as the driving forces of universities' economic impact.

Based on these findings of universities' economic impact I will present my own empirical analysis of universities in Germany and their respective impact on nearby firms.

3.3 Data & Empirical methods

This section presents the data sources and structure, outlines the empirical strategy, and discusses the main variables.

3.3.1 Data sources and structure

The data set contains detailed information about the full universe of firms in Germany as well as the counties they are located in. The period of observation ranges from 2013 to 2017. The data is structured in three parts: i) the firm-level data from the Unternehmensregisterstatistik (URS, German for "company register statistics"), ii) data on universities and other research institutions and iii) regional control variables.

The URS, which is provided by the Federal Statistical Office (Destatis), is a very rich data set and contains the full sample of firms in Germany (Destatis, 2018). It is stored as cross-sectional data for single years and is the basis for the well-known longitudinal AfiD data. The data set consists primarily of administrative data and firms have a reporting obligation. Additionally, the Destatis has supplemented the administrative information on multinational business groups by purchasing data from private companies such as Bisnode. The URS contains information about firms' location, revenues, workforce, industry classification, and other relevant characteristics. The firms' main activity is described with a five-digit industry classification based on the German WZ-2008 nomenclature, which is in return based on the European NACE Rev 2 nomenclature. For the empirical analysis, I focus on private firms in manufacturing and services, which means that I exclude all firms whose main activity is either in agriculture & forest industries, wholesale & retail trade, restaurants or housing as well as all publicly owned firms.

The remaining data set contains information on firms, their plants, and their business groups if they belong to one. A firm can possess multiple plants and therefore multiple locations. In this case, the link between firm locations and the presence of a university or other county control variables is less clear-cut. To provide a comprehensible interpretation, I will therefore restrict the data set to single-plant firms.[4] A firm can be connected to other firms via ownership structure, these firms are then referred to as a business group. The URS contains information on whether the global ultimate owner of such a business group or other group members are located abroad.

The second part of my data set contains information about institutions of tertiary education and research, namely universities and Fraunhofer research institutes. First, the university data is provided by the Genesis database of the Destatis. If a university possesses faculties in more than one municipality, it is listed individually for each location. I exclude universities that no longer report any students as well as eight universities of distance learning, which have been quite rare during the observation period. In total this analysis includes 572 universities located in 226 counties. Information about universities' founding dates is taken from the German Rectors' Conference (Hochschulrektorenkonferenz, 2022) and the historic literature (see Verger 1992, 62–65; Frijhoff 1996, 90–94; Rüegg 2004, 2011). Second, the Fraunhofer data is provided by the institute's website (Fraunhofer-Gesellschaft, 2024). The Fraunhofer Gesellschaft is Europe's largest non-profit research institution (Intarakumnerd and Goto 2018, 311). The data set contains information on three types of Fraunhofer institutes, which are differentiated by size, and their respective locations. Within Germany the Fraunhofer Gesellschaft maintains research institutes in 74 counties.

The third part of my data set consists of regional control variables. The variables are either constructed on the basis of the URS data or directly obtained from the regional database (German "Regionaldatenbank") of the Destatis. In the case of the 27 German commercial airports with a regular flight schedule, the data is taken from the federal statistical yearbooks (Destatis, 2014–2018) and attributed manually to the specific counties.

The main spatial units are 294 counties and 107 county-free cities, which I group as 401 counties. Between 2013 and 2017, some changes in county borders, as well as mergers of counties, took place. If these changes were influenced by the presence of a university, it would constitute a form of the modifiable area unit problem and distort the measured effects of universities (Briant et al., 2010). Therefore, I hold the spatial units constant in their 2017 borders by constructing hypothetical historic counties for the years 2013 to 2016. To do so one needs a county conversion key, which is provided by the Federal Office for Building and Regional Planning upon request. Other spatial

[4] In Appendix 3.A, I provide an overview of four approaches to model the link between multi-plant firms and universities; based on these alternatives, I show that my results hold for multi-plant firms, too.

units in this analysis are labour market regions, which consist of several counties, and the 16 federal states of Germany. These spatial units did not change their borders during the period of observation.

The final data set consists of around 4.5 million firm-year observations. It covers more than 900,000 firms from 2013 to 2017 and contains detailed firm and regional characteristics.

3.3.2 Empirical framework

To answer the question whether firm performance is altered by proximity to a university, I estimate the following regression:

$$Perf_{i,z,j,t} = \alpha_0 + \alpha_1 Uni_{j,t} + \alpha_2 X_{i,z,t} + \alpha_3 K_{j,t} + \nu_z \cdot \tau_t + \varphi_s \cdot \tau_t + \varepsilon_{i,z,j,t} \tag{3.1}$$

The dependent variable is the logarithm of *revenues per employee* ($Perf_{i,z,j,t}$) of firm i, which is operating in industry-section z and located in county j in year t. I construct the variable based on the number of *employees* and firms' *revenues*, which are provided in intervals of EUR 1,000 by the URS.

The coefficient of interest α_1 measures the relationship between the dependent variable and a dummy, whether the respective single-plant firm is located in a county with at least one university ($Uni_{j,t}$). To avoid a biased relationship between universities and the dependent variable I control for firm ($X_{i,z,t}$) and county characteristics ($K_{j,t}$) as well as industry-year ($\nu_z \cdot \tau_t$) and state-year ($\varphi_s \cdot \tau_t$) fixed effects. $\varepsilon_{i,z,j,t}$ are robust standard errors, clustered at the firm level.

At the firm level, I use eight control variables ($X_{i,z,t}$). Firm age and firm performance are likely correlated, e.g. through a more experienced workforce. Therefore, I include the following firm-age dummies: *younger than five years*, *age between five and ten years*, and *firms older than ten years*. Firms that do not report information on age serve as a reference group. There is also a well-documented non-linear relationship between firm size and performance (see for example Riordan and Williamson 1985), which is why I control for *employees* per firm and its square. Also in later regressions, I include a dummy variable whether a firm is a *multinational enterprise* (MNE). Every firm has to be either a *multinational* or a *national enterprise* (NE). MNEs are entities of business groups that possess at least one legal entity abroad. NEs either do not belong to business groups or to business groups without any entities abroad.

At the regional level, I use four control variables ($K_{j,t}$). Regional economists emphasise the importance of agglomeration effects, i.e. increasing returns to scale at the locational level, for firm performance. One of the causes for agglomeration is spillovers, which are often categorised into two groups. Spillovers from within the same industry are referred to as Marshall-Arrow-Romer spillovers (MAR-spillovers) and spillovers from other industries are referred to as Jacob externalities (see Glaeser et al. 1992, 1127, 1128). I use the total number of employees per labour market region, who operate in the same industry-group, to control for *MAR-spillovers*. As a proxy for further aspects of agglomeration, like Jacob externalities, I control for the *population density* of a county.

Further regional control variables are the *unemployment rate* and the local *trade tax rate*. The *unemployment rate* is a proxy for local business cycles and therefore directly related to firm performance. The *trade tax rate* captures the political climate or business environment of a county. Municipalities can choose the *trade tax rate* by setting trade tax multipliers. The population-weighted average of these trade tax multipliers is aggregated at the county level and provides the regional *trade tax rate*.

In addition to the firm and regional control variables, I include industry-year fixed effects ($\nu_z \cdot \tau_t$) and state-year fixed effects ($\varphi_s \cdot \tau_t$). The industry-year fixed effects are coded on the two-digit level and identify 88 industry-sections to control for varying industry compositions between counties over time. The state-year fixed effects control for example different policies in the 16 federal states (s) over years (t).

Table 3.1 shows that the average single-plant firm in Germany generates about EUR 2 million in *revenues* with around 12 *employees*, which leads to around EUR 160.000 in *revenues per employee*. If one divides single-plant firms along their performance, those around the 1^{st} percentile generate EUR 7,400 *revenues per employee*, and those around the 99^{th} percentile around EUR 1 million *revenues per employee*. Comparing the 50^{th} percentile or median with the 1^{st} and 99^{th} percentile indicates that the amount of *revenues per employee* has a right-skewed distribution, which is why I use its logarithmic form as a dependent variable.

Comparing firms located in a county with a university to those firms located in a county without a university shows that firms in proximity to a university generate on average more *revenues* and have a larger workforce. Also the share of MNEs is higher in counties with a university. In addition, the regional control variables vary between university counties and those counties without a university. University counties show on average a larger *population density* and more same-industry workers, but also the *unemployment* and *trade tax rates* are higher in counties with a university.

Table 3.1: *Descriptive statistics*

	Full sample					Counties w/ a university	Counties w/o a university
	Mean	SD	P1	P50	P99	Mean	Mean
Revenues	2,083.260	56,596.104	25.000	340.000	26,507.000	2,177.261	1,857.000
Employees	12.225	53.176	1.000	3.000	160.000	12.579	11.374
Revenues/employee	163.728	1,026.186	7.400	102.000	1,052.190	165.052	160.540
University	0.706	0.455	0.000	1.000	1.000	1.000	0.000
Firm Age unknown	0.724	0.446	0.000	1.000	1.000	0.722	0.731
Firm Age 0 to 5	0.012	0.109	0.000	0.000	1.000	0.012	0.011
Firm Age 5 to 10	0.047	0.213	0.000	0.000	1.000	0.049	0.044
Firm Age older 10	0.215	0.411	0.000	0.000	1.000	0.217	0.213
MNE	0.023	0.150	0.000	0.000	1.000	0.026	0.017
NE	0.976	0.150	0.000	1.000	1.000	0.974	0.983
MAR-Spillovers	3.908	6.939	0.010	1.295	33.414	4.703	1.996
Population density	0.934	1.167	0.048	0.339	4.668	1.217	0.254
Unemployment rate	6.310	2.742	2.100	6.000	13.200	6.851	5.008
Trade tax rate	395.622	73.091	0.000	393.415	502.672	411.668	356.999
Observations			4,530,150			3,200,470	1,329,637

Descriptive statistics based on the URS data set of Destatis. The data set consists of firm-year observations for 2013 to 2017 and is limited to private single-plant firms in manufacturing or services. *Revenues/employee*, *Revenues*, *MAR-Spillovers* and *Population density* are indicated in units of 1,000 euros or people. The table presents the values for the 1^{st} and 99^{th} percentile (P1 & P99), since, minima and maxima cannot be displayed due to data protection regulations.

3.4 Results

The following subsection 3.4.1 presents the positive relationship between proximity to a university and firms' *revenues per employee* as well as robustness checks. In subsection 3.4.2, I address potential concerns about endogeneity to interpret this relationship causally and discuss the self-selection of high-performing firms. Subsection 3.4.3 differentiates the channels how a university can impact firm performance and subsection 3.4.4 explains the role of multinational firm networks in universities' impact on firms.

3.4.1 Universities and firm performance

In Table 3.2 I estimate the relationship between proximity to a university and firm performance. Column 1 shows that without any control variables firms located in university counties generate on average 6.26 log points less in *revenues per employee*.[5] One possible explanation for this counter-intuitive result could be that the university coefficient barely varies over time. Therefore, the university coefficient is likely to

[5]This translates to a decrease of 6.07 % in *revenues per employee*, since $(e^{-0.0626} - 1) \cdot 100 = -6.07$

capture regional differences between counties with and without a university. Hence, in column 2, I include regional control variables. As a result, the relationship between the proximity to a university and firm performance, while remaining negative, decreases and becomes insignificant. If one additionally controls for firms' industry classifications in column 3, the university coefficient becomes positive and highly significant. Firm performance varies significantly among industries and controlling for this fact explains a large part of firms' *revenues per employee*. Firms located in university counties generate on average 0.97 log points more *revenues per employee* than firms of the same industry, which are located in an otherwise similar county without a university.

Table 3.2: *Firm performance in the vicinity of a university*

	Dep Var: Log(Revenues per Employee)					
	(1)	(2	(3)	(4)	(5)	(6)
University	-0.0626^{***}	-0.0025	0.0097^{***}	0.0087^{***}	0.0063^{***}	0.0092^{***}
	(0.0009)	(0.0018)	(0.0016)	(0.0016)	(0.0017)	(0.0017)
Observations	4,530,150	4,530,150	4,530,150	4,530,150	4,530,150	4,530,150
Adj R^2	.0010	.0128	.1760	.1869	.1910	.1915
Regional Controls		Yes	Yes	Yes	Yes	Yes
Industry FE			Yes	Yes	Yes	
Firm Controls				Yes	Yes	Yes
State FE					Yes	
Year FE					Yes	
Industry-Year FE						Yes
State-Year FE						Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private single-plant firms in manufacturing or services. Constants and control variables are estimated, but not shown. Regional control variables are *MAR-Spillovers*, *Density*, *Unemployment rate* and *Trade tax rate*. Firm control variables are *Employees*, *Employees squared*, *Firm Age*<*5*, *Firm Age 5–10* and *Firm Age*>*10*. Column (1) shows the relationship between *universities* and firms' *revenues per employee*. Column (2) includes regional control variables, (3) industry fixed effects, (4) firm control variables, (5) state and year fixed effects, and (6) interactions of fixed effects. Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

Adding further controls to the estimation leads to small changes in the size of the university coefficient, but neither changes its direction nor its significance. In column 4, I control for the fact that firms located in university counties are on average older and have a larger workforce than firms located in counties without a university. Firm age is positively correlated with performance since older firms often have a more experienced workforce. Firm size and its square take up countervailing effects on performance, such as internal economies of scale or higher governance costs for larger institutions. Without controlling for these variables, part of their effects would be captured by the university coefficient. The same applies to the introduction of state and year fixed effects in column 5. The federal states are not only responsible for tertiary-education policy, but also for other policies, which might affect firm performance. Controlling for state fixed effects ensures that the university coefficient does not capture other state-wide policies. The year fixed effects control for overall trends and make the regression comparable over time. Column 6 goes one step further and introduces industry-year and state-year fixed effects to allow for industry composition as well as state policies to vary over time.

Since column 6 controls for the most common variables in the firm performance literature, I use it as my baseline regression and a reference point for further regressions. A firm located in a university county generates on average 0.92 log points more *revenues per employee* than a firm, which is located in an otherwise similar county without a university in the same federal state during the same year. This relationship between universities and firm performance is not only statistically highly significant, but also economically important. A back-of-the-envelope calculation shows that universities in Germany are related to EUR 12.8 billion in revenues per year.[6]

To elaborate on the results of my baseline estimation I perform four robustness checks: controlling for specific effects of East-German universities, regional accessibility, social capital, and amenities. Column 1 of Table 3.3 serves as a reference point since it shows the baseline estimation and is taken from Table 3.2 column 6. Due to its distinct past, East-Germany differs in various ways from the Western part of the country and politicians explicitly tried to overcome the economic differences via tertiary education policy. Yet, as shown in column 2 of Table 3.3, there is no significant difference in the relationship of universities and firm performance between East- and West-Germany.

In the second robustness check, I control for regional accessibility. If the government invests more in the infrastructure of university regions, the resulting easier accessibility and higher firm performance might be captured by the university coefficient. Therefore, column 3 includes an *airport* dummy to the regression, which somewhat lowers the strength of the university coefficient. Nevertheless, with 0.82 log points, the university coefficient is still close to the baseline specification and highly significant.

The third robustness check controls for local social capital. The presence of a university might be correlated with a county's social capital, e.g. since a university attracts open-minded people to a region. These people are likely well connected within

[6]The total revenue of all firms located in German counties with a university is EUR 1.39 trillion. The large size of this amount can be explained by the fact that it includes final as well as intermediate goods. The share, which is related to the presence of universities can be calculated as follows: EUR 1.39 trillion $\cdot(e^{0.0092} - 1)$ = EUR 12.8 billion.

their community, open for knowledge spillovers, and therefore as employees beneficial for firm performance. To control for this effect, column 4 includes the variable *voter turnout*, which is the local turnout in the last general election. *Voter turnout* is a good proxy for social capital since it is often correlated with various other indicators for social capital (see Putnam 2000, 224). Firms in counties with a higher social capital generate significantly more *revenues per employee*, but this effect does not alter the university coefficient significantly.

Table 3.3: *Robustness checks*

	Dep Var: Log(Revenues per Employee)				
	(1)	(2)	(3)	(4)	(5)
University	0.0092***	0.0089***	0.0082***	0.0089***	0.0085***
	(0.0017)	(0.0019)	(0.0019)	(0.0019)	(0.002)
University · East		0.0021	0.0025	−0.0002	0.0002
		(0.0041)	(0.0041)	(0.0041)	(0.0041)
Airport			0.0094***	0.0085***	0.0089***
			(0.0024)	(0.0024)	(0.0024)
Voter Turnout				0.0039***	0.0038***
				(0.0003)	(0.0003)
Overnights pc					−0.0002
					(0.0001)
Observations	4,530,150	4,530,150	4,530,150	4,530,150	4,530,150
Adj R^2	.1915	.1915	.1915	.1916	.1916
Control Vars	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private firms in manufacturing or services. Constants and control variables are estimated, but not shown. The control variables are *Employees*, *Employees squared*, *Firm Age<5*, *Firm Age 5−10*, *Firm Age>10*, *MAR-Spillovers*, *Density*, *Unemployment rate* and *Trade tax rate*. Column (1) shows the baseline estimation. Column (2) includes an interaction term between universities and firms, located in East-Germany. In general the difference in firm performance between East and West-Germany is already controlled for by state fixed effects. Column (3) includes a control variable for *Airports* to the baseline regression. Column (4) includes the variable *Voter Turnout* and column (5) the variable *Overnights per capita*. Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

In the fourth robustness check, I control for amenities, i.e. features that increase the attractiveness of a region. Universities are likely correlated with amenities, e.g. by hosting cultural or sport events. In return, amenities might attract high-skilled employees, which are beneficial for firm performance (see Diamond 2016). I follow the literature and use the number of touristic *overnights per capita* as a proxy for amenities (see Carlino and Saiz 2019). Column 5 shows that the relationship between amenities and firm performance is insignificant. This is in line with the findings of Audretsch et al. (2019, 156), who argue that the effect of amenities is already taken up by *population density*.

Further analysis of the baseline results can be found in Appendix 3.A and 3.B. To model the relationship between universities and the performance of multi-plant firms I use four different approaches: i) a dummy variable, whether the firm's headquarter is located in a university region, ii) an index for the share of a firm's plants, which are located in a university region, iii) a dummy variable, whether the half of the firm's plants are located in a university region, iv) an index for the share of a firm's employees, which work in plants located in a university region. Each way of modelling the relationship between universities and multi-plant firms shows positive and highly significant results, which are similar to the baseline estimation (see Table 3.A1). As shown in Table 3.B1, the results are not driven by changes in the composition of the firm sample or the university variable.

So far the chapter has shown, that universities are positively correlated with firm performance, once one controls for regional differences and the industry composition of counties. This correlation does not depend on specific features of West-German universities or other factors, which might be correlated with the presence of a university, such as accessibility, social capital, or amenities.

3.4.2 Endogeneity and self-selection

In the first part of this subsection, I address endogeneity concerns by presenting arguments for an impact of universities on firm performance and by discussing empirical evidence against reverse causality and an omitted variable bias. In the second part of this subsection, I discuss to what extent the impact of universities on firm performance is driven by the self-selection of high-performing firms into university regions.

As outlined in the literature review there are well-established arguments on how universities can have a causal impact on firm performance. The knowledge-production function literature argues that universities' basic research generates knowledge spillovers on nearby firms, whose applied research becomes more effective (see Griliches 1979; Jaffe 1989). Another strand of literature argues that universities promote human capital and increase the high-skilled local labour supply (e.g. Valero and Van Reenen 2019, 53). Since many graduates have a preference to stay in their respective university region, local firms benefit from a well-educated labour supply.

Yet, despite these arguments one needs to address concerns about reverse causality and omitted variable bias, to interpret the relationship between universities and firm performance causally. In case of reverse causality, one could argue that universities might have been simply founded in close proximity to firms with already large *revenues per employee*. In case of an omitted variable bias, the presence of a university could be correlated with uncontrolled variables impacting firm performance.

The first endogeneity concern about reverse causality is more pressing for countries where the system of tertiary education is shaped by private universities. Historically, Germany had almost no private universities with a few clerical exceptions, and most of today's private universities were founded since the 2000s (Buschle and Haider 2016, 77). Additionally, under German law even private universities need to be recognised by the government of the respective federal state (see §70 HRG).

Hence, to address the concerns about reverse causality, I follow the literature and focus on universities that arguably have been founded for political reasons (see Andrews 2020; Cantoni and Yuchtman 2014; Kantor and Whalley 2019). For Germany, this is rather straightforward as a university foundation has to be approved by the respective federal state's government. Therefore, the foundation of a university is usually debated in the federal state's parliament and the underlying reasons are very well documented. To utilise this fact, I drop observations from those counties, where a university has been founded before 1960 or after 1980, as well as those counties located in the former German Democratic Republic. Thereby I focus on universities, which were founded in the West-German federal states during the 1960s and 70s. During this period the political main objective for university foundations was to achieve equal access to tertiary education throughout the federal states (see Valero and Van Reenen 2019, iv).[7] In Appendix 3.C, I provide a detailed overview about West-German university foundations during the 1960s and 70s and the respective reasons for their location decisions.

The results of Table 3.4 substantiate the arguments about reverse causality, omitted variable bias, and self-selection. Column 1 displays the baseline estimation and column 2 excludes observations from East-Germany and those counties, where a university has been founded before 1960 or after 1980. For the remaining universities, which were founded during the 60s and 70s, reverse causality can be ruled out. Firms, that are located in vicinity of one of these universities generate 0.93 log points more *revenues per employee* than firms in regions without a university. Since this result is almost identical to the full sample, reverse causality is unlikely to drive the results.

Regarding the concerns about omitted variable bias, I already control for various

[7] In the few cases where politicians took economic reasons into consideration universities were founded in the economically struggling periphery and not the respective centres.

Table 3.4: *Examining concerns about endogeneity and self-selection*

	Dep Var: Log(Revenues per Employee)				
	(1)	(2)	(3)	(4)	(5)
University	0.0092***	0.0093***	0.0092***	0.0104***	−0.0030
	(0.0017)	(0.0025)	(0.0016)	(0.0016)	(0.0153)
Smaller City				−0.0092**	
				(0.0035)	
Larger City				0.0235***	
				(0.0054)	
Metropolis				0.0472***	
				(0.0081)	
Observations	4,530,150	1,801,227	4,530,150	4,530,150	54,653
Adj R^2	.1915	.1964	.2545	.2545	.1573
Control Vars	Yes	Yes	Yes	Yes	Yes
2Digit-Industry-Year FE	Yes	Yes	No	No	Yes
5Digit-Industry-Year FE	No	No	Yes	Yes	No
State-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private single-plant firms in manufacturing or services. Constants and control variables are estimated, but not shown. The control variables are *Employees*, *Employees squared*, *Firm Age<5*, *Firm Age 5−10*, *Firm Age>10*, *MAR-Spillovers*, *Density*, *Unemployment rate* and *Trade tax rate*. Column (1) shows the baseline estimation. Column (2) excludes firms located in East Germany as well as those in counties with a university founded after 1980 or before 1960. Column (3) includes 5-digit industry fixed effects and (4) three city-size dummies to the baseline model (Smaller City: 100K−500K inhabitants, Larger City: 500K−1M, Metropolis: >1M). Column (5) excludes all firms 5 years and older from the baseline model. Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

factors, e.g. the local *population density*, *trade tax rates*, or *MAR-spillovers* and I performed robustness checks, e.g. for regional accessibility. Given these control variables and robustness checks, there is no prominent candidate for an omitted variable. Yet, one group of potentially omitted variables could be related to the industry composition. I already control for industry-year fixed effects, but within the 88 industry-divisions university regions might host different companies compared to regions without a university. Therefore, column 3 of Table 3.4 uses the five-digit WZ industry classification to generate fixed effects for 839 industry-subclasses. Introducing these rather fine-grained fixed effects does not alter the university coefficient, which shows omitted variable biases related to the industry composition to be rather unlikely.

Another group of potentially omitted variables could be correlated with city size. I already control for *population density*, yet one might argue that instead of a linear correlation omitted variables could occur along certain thresholds of population. For example, universities as well as other public goods, like public authorities, could be located more often in cities above a certain population threshold. If these public goods have a positive impact on firm performance, this would constitute an omitted variable bias.

To elaborate on this possibility, Table 3.4 column 4 includes three dummies whether a firm is located in a smaller city (100.000 to 500.000 inhabitants), a larger city (500.000 to 1 million inhabitants), or a metropolis (more than a million inhabitants).[8] The definitions follow the Federal Office for Building and Regional Planning and firms located in rural counties serve as a reference group. Firms located in small cities generate less and firms in large cities and metropolises more *revenues per employee* than firms located in rural counties. These effects come in addition to the positive, linear effect of *density*. The lower firm performance in small cities compared to large cities could be the result of fewer public goods, like infrastructure. The lower firm performance in small cities compared to rural counties is outweighed by the positive effect of *density*. This finding points towards a small, but significant non-linear effect of *density* along certain population thresholds. Universities are more often located in smaller cities than in rural counties. Controlling for the lower firm performance in these smaller cities leads to an increase in the size of the university coefficient from 0.92 to 1.04 log points. Therefore, if there is an omitted variable correlated to population thresholds, it would lead to a conservative estimation of universities' impact and not an overestimation.

In addition to endogeneity concerns, I address the question about self-selection of firms into university regions. Universities can impact firm performance via knowledge spillovers, high-skilled labour supply, etc., which are comparable to an intensive margin. Universities can also impact firm performance by attracting high-performing firms into the region, which is comparable to an extensive margin. From the perspective of local politicians or job seekers, it might be less relevant whether a university increases the performance of existing firms or attracts better-performing firms to the region. From the perspective of local firm owners, this question is more pressing.

The very fine-grained industry-year fixed effects shown in columns 3 and 4 already address large parts of potential self-selection. Still even within the 839 industrial-subclass higher performing firms could self-select into university regions and account for the measured effect. Due to a structural break in the URS, it is hard to include a longer time horizon and estimate this effect. Yet, I am able to examine the size of

[8] For a more nuanced view, Table 3.B1 shows the same regression with a larger number of more fine-grained population dummies.

the extensive margin during the period of observation. Therefore, I limit the sample to start-ups, i.e. firms younger than five years, and additionally exclude all firms, that are located in a county where the university has been founded at least ten years ago. Column 5 of Table 3.4 shows that start-ups in university regions do not perform significantly differently compared to start-ups in regions without a university. This finding is in line with the literature (Woodward et al., 2006) and indicates that during the period of observation there has not been a self-selection of better-performing firms into university regions. Hence, there is no evidence that new entrants explain the positive effect of universities on firm performance in my sample. However, given the available time period one cannot rule out that within the same industrial subclass there has been self-selection of high-performing firms into university regions in the past.

So far the chapter has argued, that universities have a positive impact on firm performance in their home region. Using a subset of universities, which were founded for political reasons, as well as various control variables and robustness checks, I am confident to rule out reverse causality and an omitted variable bias. Although I cannot rule out that the effect of universities is at least partly driven by past self-selection of high-performing firms, the performance of newly founded firms is suggestive for the conclusion that the intensive margin of universities exerting a positive impact on incumbent firms is the more important channel of influence in the German data.

3.4.3 Universities' impact on firms via knowledge spillovers and labour supply

After discussing the positive impact of universities on the firm performance in their home region, this subsection investigates, which channels are responsible for the results. As outlined in section 3.2, most of the literature emphasises two channels through which universities can impact firm performance: the first via local knowledge spillovers, and the second via high-skilled labour supply.[9]

Since universities are associated with both channels I introduce non-university research institutes, which are solely focused on creating knowledge spillovers and not on local labour supply. To do so, I include a dummy variable, whether a firm is located in a county with a *Fraunhofer* research institute. The Fraunhofer Gesellschaft is Ger-

[9] Some authors have discussed other channels through which universities impact economic outcomes, namely the provision of social capital (see Valero and Van Reenen 2019, 54) or amenities (see Glückler et al. 2015, 329). Yet, as shown in the robustness checks of Table 3.3, controlling for these variables does not alter the estimated university coefficient in a significant way. Therefore, I focus on universities' impact on firm performance via knowledge spillovers and high-skilled labour supply.

many's largest research organisation with institutes in 74 counties.

Table 3.5: *Universities' impact via knowledge spillovers and labour supply*

	Dep Var: Log(Revenues per Employee)				
	(1)	(2)	(3)	(4)	(5)
University	0.0092***	0.0100***			
	(0.0017)	(0.0018)			
Fraunhofer		−0.0047**	−0.0005	−0.0016	0.0011
		(0.0019)	(0.0020)	(0.0019)	(0.0019)
AppliedUni			0.0110***	0.0104***	0.0112***
			(0.0023)	(0.0022)	(0.0022)
ClassicalUni			−0.0167***	−0.0090***	−0.0122***
			(0.0017)	(0.0016)	(0.0016)
Smaller City					−0.0054
					(0.0035)
Larger City					0.0315***
					(0.0056)
Metropolis					0.0544***
					(0.0083)
Observations	4,530,150	4,530,150	4,530,150	4,530,150	4,530,150
Adj R^2	.1915	.1915	.1916	.2545	.2545
Control Vars	Yes	Yes	Yes	Yes	Yes
2Digit-Industry-Year FE	Yes	Yes	Yes	No	No
5Digit-Industry-Year FE	No	No	No	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private single-plant firms in manufacturing or services. Constants and control variables are estimated, but not shown. The control variables are *Employees*, *Employees squared*, *Firm Age<5*, *Firm Age 5−10*, *Firm Age>10*, *MAR-Spillovers*, *Density*, *Unemployment rate* and *Trade tax rate*. Column (1) shows the baseline estimation. Column (2) includes *Fraunhofer* research institutes. Column (3) estimates the impact of *Applied* and *Classical universities* separately. Column (4) includes 5-digit industry fixed effects and (5) three city-size dummies to the baseline model (Smaller City: 100K−500K inhabitants, Larger City: 500K−1M, Metropolis: >1M). Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

In Table 3.5, I address the channels via knowledge spillovers and local labour supply. In the first column, I start with the baseline estimation. In the next column, I add a *Fraunhofer* dummy. Including a dummy for firms, located in counties with these research institutes, leads to a surprising result. Proximity to a *Fraunhofer* institute and its knowledge spillovers shows a statistically significant, negative relationship with firm performance. At the same time the positive coefficient of the university dummy increases.

To elaborate on this counter-intuitive result in more detail, I differentiate between two types of universities. The institutional setting of German universities is suited rather well to distinguish between the impact via high-skilled labour supply and the impact via knowledge spillovers. Historically, the German university system was shaped by *classical universities*[10] with a strong focus on basic research (Charle 2004, 49; Guagnini 2004, 621). During the educational expansion of the 20th century, various new forms of universities emerged, such as universities of applied sciences, pedagogical and administrative universities, universities of arts etc. (see Neave 2011, 41; Lackner 2019, 135–139). I aggregate all of these forms of universities to *applied universities* since they focus primarily on teaching. Due to the teaching focus, *applied universities* can be clearly associated with high-skilled local labour supply. For *classical universities* the distinction between the two channels is ex-ante less clear cut, since they focus on the provision of knowledge spillovers via basic research, but are also responsible for high-skilled labour supply.

In column 3 of Table 3.5, I distinguish between *applied* and *classical universities.* Proximity to the high-skilled local labour supply of *applied universities* leads to an increase in a firm's *revenues per employee* by 1.10 log points. Yet, proximity to the knowledge spillovers of *classical universities* is negatively related to firm performance. This is in line with the negative coefficient for *Fraunhofer* institutes in column 2. In column 3, the *Fraunhofer* coefficient turns insignificant, which is likely due to the fact that most *Fraunhofer* institutes are located close to a *classical university.* If a firm is located in a county with a *Fraunhofer* institute as well as a *classical university* these effects would occur at the same time. The same holds for *applied universities.* Here the coefficients for *applied* and *classical universities* counteract each other.

The first result of column 3 about the positive impact of *applied universities* is straightforward to interpret. Teaching graduates leads to a supply of high-skilled employees or future company founders. These graduates may have already collected some experience at local firms during internships or developed a higher regional preference during their university years, which is why the labour supply channel benefits local firms. Regressing the number of academic job entrants at the county level on the number of graduates shows a stronger correlation for graduates from *applied universities* than for those from *classical universities.*[11] This indicates that graduates from *applied universities* are likely to have a higher preference for their university region than graduates from *classical universities.* In general, the high preference of graduates

[10] I aggregate general and technical universities to *classical universities* since they both focus on basic research and show similar results.

[11] The results are provided by in Table 3.B2.

for their university regions can also explain the importance of the local labour supply channel for the impact of German universities. This importance is similar to findings from other European countries (e.g. Amendola et al. 2020), but in contrast to studies from the US (see Abel and Deitz 2012), where graduates traditionally have a higher mobility level (see Faggian et al. 2017; Haußen and Übelmesser 2015).

The second result of column 3, that firms in proximity to *classical universities* perform worse than firms located in counties without a university or *Fraunhofer* institute, is less obvious to explain. The literature noted the finding before that firms perform worse in proximity to research-intensive universities (e.g. Maietta et al. 2017, 770). However, to the best of my knowledge the underlying mechanism has not been addressed, yet.

There are several possible explanations for the negative relationship between knowledge spillovers and firm performance. It is unlikely that *classical universities* have direct negative spillovers on firm performance since the dependent variable is *revenues per employee*. In contrast to profits, revenues are not necessarily diminished by any form of costs. It is also unlikely that universities have an indirect negative impact on firms by competing for local *employees* since firms in university regions employ on average a larger workforce than firms in counties without a university (see Table 3.1). One possible explanation for the negative coefficient could be that different types of firms self-select into counties with a *classical university*. In column 4 of Table 3.5, I control for five-digit industry fixed-effects. This explains parts of the effect but does not alter the negative direction or significance of the coefficient for *classical universities*. Another explanation for the negative effect could be that *classical universities* are located in different types of cities compared to *applied universities* (see Lackner 2019, 154). In column 5 of Table 3.5, I include city size dummies to the regression, which even increases the negative coefficient for *classical universities*, since they are more often located in larger cities.

A more promising explanation for the negative coefficient of *classical universities* could be that firms in close proximity to *classical universities* and their knowledge spillovers are more focused on private research activities than generating *revenues*. In the long-run, such a behaviour could drive firms out of the market since they perform worse than their competitors. However, a specialisation in R&D might be a reasonable strategy for a firm within a (multinational) business group. The results of one firm's R&D activities can lead to gains throughout the entire business group (Bilir and Morales, 2020, 1567). If in return researching firms are not fully compensated for their activities, this can explain the lower performance of firms in close proximity to *classical universities*. To elaborate on this explanation, I will analyse multinational business groups in the next subsection.

So far the chapter has shown, that *applied universities* with a strong focus on high-skilled local labour supply have a positive impact on regional firm performance. The regional preference of graduates is a likely driver for the strength of the labour supply channel. Proximity to *classical universities* and their knowledge spillovers is negatively correlated with firm performance. To explain this finding, the ownership structure of a firm might play a role. Therefore, the next subsection analyses the effect of universities on the performance of multinational enterprises.

3.4.4 Universities and multinational enterprises

In this subsection, I analyse the effects of universities on the performance of *national enterprises* (NEs) and *multinational enterprises* (MNEs). Thereby, I elaborate on the explanation for the negative coefficient of *classical universities*, that it is specific firms of larger business groups, which perform worse in proximity to *classical universities*. For the analysis I group all firms into either NEs or MNEs. This distinction is based on information about the firms' respective business groups, given by the URS data set. MNEs are entities of business groups that possess at least one legal entity abroad. NEs are all firms without any entities abroad.

There is a large strand of literature, which shows that MNEs differ from NEs. MNEs are more productive than NEs, for example due to a more efficient allocation of resources (Yeaple, 2003). There is ample empirical evidence from various countries that MNEs are more productive than NEs (e.g. Arnold and Javorcik 2009; Bandick 2011; Liu et al. 2017). One way how multinational business groups ensure an economically efficient allocation of resources is to transfer assets, like intellectual property, to firms which are located in low-tax jurisdictions (see Davies et al. 2018). Fuest et al. (2022, 3) show that MNEs with a tax presence in Germany shift around 16 % of their profits to low-tax jurisdictions. With regard to MNEs and regional spillovers, there is larger literature about the positive effect of MNEs on surrounding firms (e.g. Fosfuri et al. 2001; Görg and Strobl 2005; Markusen and Trofimenko 2009). Yet, in reverse there has been only a limited amount of studies about the impact of regional characteristics on MNEs (e.g. Altomonte et al. 2013; Mariotti et al. 2010).

Table 3.6 shows the impact of universities on the performance of MNEs and NEs. Column 1 shows the baseline estimation and column 2 introduces a dummy variable for MNEs. MNEs generate on average 26.12 log points more *revenues per employee*

Table 3.6: *Universities and multinational enterprises*

	Dep Var: Log(Revenues per Employee)				
	(1)	(2)	(3)	(4)	(5)
University	0.0092***	0.0088***	0.0099***	0.0098***	
	(0.0017)	(0.0017)	(0.0017)	(0.0018	
MNE		0.2612***	0.3106***	0.3201***	0.3236***
		(0.0069)	(0.0131)	(0.0132)	(0.0126)
MNE·University			−0.0626***	−0.0159	
			(0.0146)	(0.0154)	
Fraunhofer				−0.0025	0.0006
				(0.0019)	(0.0020)
MNE·Fraunhofer				−0.1134***	−0.0707***
				(0.0138)	(0.0162)
AppliedUni					0.0104***
					(0.0017)
MNE·AppliedUni					0.0048
					(0.0151)
ClassicalUni					−0.0146***
					(0.0023)
MNE·ClassicalUni					−0.0846***
					(0.0163)
Observations	4,530,150	4,530,150	4,530,150	4,530,150	4,530,150
Adj R^2	.1915	.1933	.1933	.1934	.1934
Control Vars	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private single-plant firms in manufacturing or services. Constants and control variables are estimated, but not shown. The control variables are *Employees*, *Employees squared*, *Firm Age<5*, *Firm Age 5−10*, *Firm Age>10*, *MAR-Spillovers*, *Density*, *Unemployment rate* and *Tax rate*. Column (1) shows the baseline estimation. Column (2) includes an *MNE* dummy. Each firm is either a *multinational* (MNE) or *national enterprise* (NE). Column (3) estimates the interaction between the *university* dummy and the *MNE* dummy. Column (4) includes a dummy for *Fraunhofer* institutes as well as an interaction term and column (5) breaks the *university* variables into variables and interaction terms for *classical* and *applied universities*. Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

than NEs, which is in line with the literature. Adding an MNE dummy does not alter the strength of the university coefficient significantly, despite the fact that MNEs are located disproportionately in counties with a university.

In column 3 of Table 3.6, I include an additional interaction effect for MNEs, which are located in a university region. The additional effect of being an MNE and being located in a university region is negative and highly significant. The interaction term even outweighs the university coefficient. An MNE, which is located in a university region, generates 5.27 log points[12] fewer *revenues per employee* compared to an MNE located in a county without a university. After controlling for the additional effect of universities on MNEs the overall university coefficient increases. This increase could emerge due to the fact that NEs provide less knowledge spillovers internally (Markusen and Trofimenko, 2009) and might therefore be more susceptible to knowledge spillovers from universities.

Yet, to explain why the absolute effect of universities is actually negative for the performance of MNEs, I take up the explanation from the last subsection. Firms in close proximity to research-intensive universities are more exposed to knowledge spillovers (see Glaeser et al. 1992, 1127). Therefore, these firms' private R&D activities are more efficient and they could be more focused on private research activities than generating *revenues* (see Griliches 1979, Jaffe 1989). In the long-run, such a specialisation on R&D at the costs of firm performance could drive firms out of the market. However, it might be a reasonable strategy for members of business groups, since the gains from private R&D activities benefit the entire business group. Firms, which are specialised in R&D, hire more *employees* for university-industry cooperation. If the multinational business group engages in transfer pricing, i.e. shifting profits to low tax jurisdictions, the researching firms are not fully compensated for their activities by the other members of the business group (see Davies et al. 2018; Fuest et al. 2022). In this case, the *revenues* of researching firms do not increase as fast as the number of *employees*, which leads to decreasing *revenues per employee*.

To elaborate on this explanation, I analyse the channels via knowledge spillovers and local labour supply. In column 4 of Table 3.6, I include a dummy variable whether a firm is located in a county with a *Fraunhofer* institute as well as an interaction term for MNEs and proximity to a *Fraunhofer* institute. In contrast to Table 3.5, proximity to a *Fraunhofer* research institute and its knowledge spillovers has no longer a significant effect on firm performance. The effect is likely taken up by the additional interaction term between MNEs and *Fraunhofer* institutes, which is highly significant and negative. Overall MNEs, which are located in counties with a *Fraunhofer* institute, perform significantly worse than other MNEs, while there is no such effect for NEs. Since these MNEs in proximity to *Fraunhofer* institutes and their knowledge spillovers are more likely to engage in private R&D, their lower performance is in line with the explanation formulated above.

[12]For this comparison, one needs to add the general university effect of .99 log points to the additional interaction term for MNEs of −6.26 log points.

The results are further substantiated by the estimations in column 5. Here I subdivide the university variable and the MNE-university interaction term into *classical* or *applied universities*. *Applied universities*, which are not focused on knowledge spillovers, but on high-skilled local labour supply, have a positive impact on all types of firms. The general effect of *applied universities* on firm performance is positive and highly significant but the additional interaction term for MNEs is insignificant. The general effect of *classical universities* on firm performance is still negative but weaker than in Table 3.5. In addition, the interaction term shows a negative and highly significant effect of *classical universities* on MNEs, which is way stronger than the general effect. MNEs located in a county with a *classical university* generate 9.92 log points fewer *revenues per employee* compared to MNEs in counties without a university or *Fraunhofer* institute. Overall these findings are in line with the explanation that firms, which perform private R&D for the entire business group, are less focused on directly generating revenues. It can explain why MNEs perform particularly worse in proximity to *classical universities* or *Fraunhofer* institutes and their knowledge spillovers. Thereby, it can at least partly, but not fully explain why the overall firm performance is worse when firms are located in proximity to a *classical university*.

In this subsection, I have shown, that universities are more important for the performance of NEs than MNEs in Germany. The positive impact of *applied universities* and their high-skilled local labour supply is beneficial for firm performance in general. Yet, the negative relation between firm performance and proximity to *classical universities* is particularly pronounced for firms, which are members of multinational business groups. Within these business groups, it is possible to shift some of the gains from private R&D activities to low-tax jurisdictions. This can explain the lower remaining firm performance of the firms in close proximity to *classical universities* and their knowledge spillovers.

3.5 Concluding remarks

This chapter analyses the impact of universities on firm performance. My findings are based on the URS, which covers the universe of firms in Germany, and other data sets of the Destatis for the observation period from 2013 to 2017. I find that firms in vicinity of universities generate ceteris paribus and on average 0.92 % more *revenues per employee* compared to firms located in regions without a university. This effect is related to around EUR 12.8 billion in firm revenues per year.

There are several potential obstacles to a causal interpretation of this result. To address concerns about reverse causality I focus on a subsample of universities, which were founded for political reasons, namely to ensure equal access to tertiary education. The results for this specific subsample, where reverse causality can be ruled out, are rather close to the overall sample. In order to deal with concerns about an omitted variable bias, I include fine-grained industry fixed effects and dummies of population thresholds to control for any potentially omitted variables correlated with industrial composition or a specific city size. Both alternations do not change the impact of universities significantly.

Furthermore, I control for the performance of start-ups to address the question about self-selection of high-performing firms into university counties. During the period of observation, there is no self-selection, but I cannot rule it out for the past. In conclusion, there is an impact of universities on the performance of firms within the same county, which is likely driven by an effect on all firms. From a regional policy perspective, this is a rather encouraging result.

When it comes to analysing the underlying channels of this effect, I focus on universities' impact via knowledge spillovers and high-skilled local labour supply - especially since my robustness checks show that the potential channels via amenities or social capital are quantitatively negligible. For the two channels via high-skilled local labour supply or knowledge spillovers, the analysis shows a strong impact of local labour supply. Analysing the mobility patterns of graduates from classical and applied universities substantiates the findings for the local labour supply channel. The negative relationship between proximity to the knowledge spillovers of *classical universities* and firm performance is a counter-intuitive result, which has been noticed in the literature before. Analysing the structure of business groups gives at least a partial explanation for this result.

Therefore, I group all firms into either NEs or MNEs. Universities have a positive overall impact on firm performance, but the additional impact on MNEs is negative and even outweighs the general positive impact. MNEs in vicinity to universities perform worse than MNEs located in regions without a university. Distinguishing between the two types of universities shows that this result is driven by the interaction between MNEs and *classical universities*. This supports the explanation that firms which are embedded in a business group and additionally located in proximity to *classical universities* and their knowledge spillovers serve as public goods providers within their business group. If the researching firms are not fully compensated for their activities, due to transfer pricing, this can explain their lower performance.

Overall, the results in this chapter show that universities have a positive impact on firm performance, which is driven by a high-skilled labour supply. The negative relation between proximity to research-intensive universities and firm performance can be partly explained by specific members of (multinational) business groups. Yet, further research is needed to disentangle this effect fully. From a policy perspective, my analysis for Germany is in line with the examples from the beginning: Universities can be a valuable element of regional planning policy. If politicians want to foster firm performance, universities with a strong focus on the provision of human capital are important regional institutions. Yet, the concrete effect depends on the specific mobility of graduates and the respective size of the desired region.

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Appendix

3.A Results for multi-plant firms

Modelling the proximity between a university and a firm is challenging as both can have multiple locations. A firm can possess multiple plants and a university multiple faculties each in a distinct location. For universities, the Federal Statistical Office reports every single location individually. Unfortunately, for firms *revenues* are not reported at the plant, but only aggregated at the firm level. Therefore, it is not feasible for the context of this chapter to simply model the proximity between plants and university locations.

In Table 3.A1 I show the results for five ways to model the proximity between firms and university locations. Column 1 shows the baseline estimation for single-plant firms and serves as a reference point. The relationship between firm variables such as *revenues per employee* and proximity to a university can then be modelled with a dummy variable (*university*). This way of modelling proximity provides the most comprehensive interpretation, which is why I use it throughout the chapter.

The other four ways of modelling the relationship between multi-plant firms' performance and proximity to universities show similar results. One way of modelling proximity is to focus on the firm's headquarter. A dummy variable measures whether the firm's headquarter is located close to a university or not. As shown in column 2 of Table 3.A1 firms whose headquarter is located close to a university generate 0.86 log points more *revenues per employee* compared to firms whose headquarter is not located close to a university. This way of modelling proximity includes multi-plant firms to the sample but ignores the information about other plant locations.

To address this problem and use all plants of a firm the next way of modelling proximity is a university index for multi-plant firms:

$$Uni\_Index_i = \frac{\sum_{p=1}^{P} University_p}{P} \tag{3.2}$$

The index measures the share of plants p, located in a university region. Throughout all regressions, with multi-plant firms, regional control variables are indexed in the same way. For single plant firms, where the number of plants P is 1, the university index coincides with the university dummy. The same applies to all other regional control variables, which are modelled in the same way for this estimation. Changing the university index from 0 to 1 means that the share of plants located in university regions increases from 0 to 100 %. This rise in the university index is related to an increase in *revenues per employee* by 0.89 log points, as shown in column 3.

Yet, the relationship between the share of plants located in a university region and firm performance could be non-linear. To address this one can model various thresholds whether the university index is larger than a given number, e.g. 0.5:

$$Uni\_Half_i = \begin{cases} 0 & \text{if } Uni\_Index_i < 0.5 \\ 1 & \text{if } Uni\_Index_i \geq 0.5 \end{cases} \tag{3.3}$$

As shown in column 4 of Table 3.A1 this way of modelling the relationship leads to a university coefficient of 0.82 log points.

Another concern might be that not all plants are equally important for the overall performance of the firm. One way to address this concern is an employee-weighted university index:

$$Weighted\_Uni\_Index_i = \frac{\sum_{p=1}^{P} University_p \cdot Employees_p}{\sum_{p=1}^{P} Employees_p} \tag{3.4}$$

This index measures the share of *employees* who work in plants located in proximity to a university. Column 5 displays the coefficient for this index. With 0.70 log points, the index shows a somewhat weaker effect. This result could be explained by the fact that proximity to a university is less important for larger plants, where only production takes place, compared to those plants, which are specialised in R&D.

All four ways of modelling the proximity between multi-plant firms and a university location show results similar to the baseline regression for single-plant firms. As shown in Table 3.A1 the coefficients are always positive and highly significant with somewhat varying strength.

Table 3.A1: *Baseline regression for multi-plant-firms*

	Dep Var: Log(Revenues per Employee)				
	(1)	(2)	(3)	(4)	(5)
Uni_Index	0.0092***	0.0086***	0.0089***	0.0082***	0.0070***
	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0018)
Observations	4,530,150	4,705,020	4,705,020	4,705,020	4,701,304
Adj R^2	.1915	.2024	.2024	.2024	.2026
Control Vars	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private firms in manufacturing or services. Constants and control variables are estimated, but not shown. The control variables are *Employees*, *Employees squared*, *Firm Age<5*, *Firm Age 5−10*, *Firm Age>10*, *MAR-Spillovers*, *Density*, *Unemployment rate* and *Trade tax rate*. Column (1) shows the estimation for the single-plant firms. Column (2) includes multi-plant firms and uses a university dummy for the main plant. Column (3) models proximity with a university-index aggregating the university dummies for all plants per firm and dividing it through the total number of plants. Instead of this index column (4) uses a dummy, whether the university-index is larger than .5 and column (5) uses a worker weighted university-index. Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

3.B Further estimations

Table 3.B1: *Further regressions*

	Dep Var: Log(Revenues per Employee)				
	(1)	(2)	(3)	(4)	(5)
University	0.0092***	0.0106***			0.0110***
	(0.0017)	(0.0018)			(0.0017)
Number of Unis			0.0018***	0.0017***	
			(0.0004)	(0.0006)	
City: 50K- 75K					−0.0216***
					(0.0065)
City: 75K-100K					0.0082
					(0.0067)
City:100K-125K					−0.0143***
					(0.0055)
City:125K-150K					−0.0429***
					(0.0081)
City:150K-175K					−0.0127**
					(0.0059)
City:175K-200K					−0.0073
					(0.0084)
City:200K-300K					0.0055
					(0.0045)
City:300K-400K					−0.0343***
					(0.0056)
City:400K-500K					0.0063
					(0.0096)
City:500K- 1M					0.0206***
					(0.0057)
City:>1M					0.0415***
					(0.0086)
Observations	4,530,150	4,212,838	4,530,150	4,530,150	4,530,150
Adj R^2	.1915	.2057	.1915	.1937	.2546
Control Vars	Yes	Yes	Yes	Yes	Yes
2Digit-Industry-Year FE	Yes	Yes	Yes	Yes	No
5Digit-Industry-Year FE	No	No	No	No	Yes
State-Year FE	Yes	Yes	Yes	No	Yes
County FE	No	No	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel-OLS estimates based on the URS data set of Destatis. The data set is limited to private single-plant firms in manufacturing or services. Constants and control variables are estimated, but not shown. The control variables are *Employees*, *Employees squared*, *Firm Age<5*, *Firm Age 5−10* and *Firm Age>10*, *MAR-Spillovers*, *Density*, *Unemployment rate* and *Trade tax rate*. Column (1) shows the baseline estimation. Column (2) excludes firms founded during the period of observation or those firms located in counties, where the *university* dummy changed. Column (3) includes the *number of universities* and column (4) county and year fixed effects instead of state-year fixed effects. Column (5) adds eleven city-size dummies to the baseline model. Standard errors are clustered at the firm level. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

Table 3.B2: *Regressing graduates on job entrants*

	Dep Var: Job entrants w university degree			
	(1)	(2)	(3)	(4)
$\text{Graduates}_{ClassicalUni}$	0.1935***	0.1955***	0.1957***	0.1974***
	(0.0607)	(0.0593)	(0.0594)	(0.0055)
$\text{Graduates}_{AppliedUni}$	0.4396***	0.4480***	0.4468***	0.2570***
	(0.1133)	(0.1123)	(0.1125)	(0.0119)
Observations	1,979	1,979	1,979	1,979
Adj R^2	.7446	.7527	.7530	.8174
Control Vars	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
State FE	No	No	No	Yes

Notes: Panel-OLS estimates based on the Regionaldatenbank of Destatis. The data is aggregated at the county level and ranges from 2013 to 2017. Constants and control variables are estimated, but not shown. The control variables include the *Unemployment rate* at the county level. Column (1) shows the relationship between graduates from classical and applied universities and job entrants with a university degree. Column (2) includes controls for unemployment. Column (3) includes year and (4) state fixed effects. *** indicates significance at the 1 % level, ** at the 5 % level and * at the 10 % level.

3.C German university foundations during the 1960s & 70s

To address concerns about endogeneity I focus on historic universities, which were founded during the 1960s and 70s in West-German federal states. I exclude all firms located in East Germany as well as firms located in counties where universities were either founded before 1960 or only after 1980. The main endogeneity concern I want to address with this approach is reverse causality, i.e. the possibility that high-performing firms play a role in the foundation of universities. Therefore, I limit my sample to historic universities founded half a century ago during the 1960s and 70s. Back in the day, there were almost no private universities in Germany, except for a few clerical ones (see Buschle and Haider 2016).

Under the institutional setting of German tertiary educational policy, the foundation of any university needs the approval of the respective federal state's government, which ensures a well-documented parliamentary debate for most university foundations (see §70 HRG). In the plenary protocols, there is no anecdotal evidence of high-performing firms playing a role in the foundation of a university at the time. Independent of party lines government and opposition almost always agreed about the location decision of universities and fierce disputes mostly took place about institutional questions. If one assumes the system of checks and balances to be working, the opposition would have made it a subject of discussion, in case there would have been a systematic priority of regions with high-performing firms in the decisions about university locations.

The following table shows an overview of universities founded in West-Germany during the 1960s and 70s and their respective parliamentary debates. For each county, I discuss only one university (e.g. Regensburg or Breme) and universities located in regions with significant historic universities are excluded (e.g. Bamberg, Bielefeld or Konstanz):

Table 3.C1: *West-German university foundations during the 1960s and 70s*

University	**Reasons for location**	**Source**
Hochschule Flensburg	Restructuring of an existing institution	Off report of the 46th session of the 6th parl of Schleswig Holstein 09.06.1969, 1969
U of PubAdmin and Services	Municipalities concede existing buildings	Off record 7/990 about a bill of the gov of Schleswig Holstein 05.04.1974, 7
Hochschule Braunschweig-Wolfenbüttel	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978

Hochschule Hildesheim-Holzminden-Göttingen	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of Hildesheim	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of AppSc Hannover in Nienburg	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of AppSc Northeast Lower Saxony	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of AppSc Ottersberg	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of AppSc Ostfriesland in Emden	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of Oldenburg	Equal access to higher edcuation	Off report of the 84th session of the 7th Lower Saxonian parl 26.11.1973, 8476
U of Osnabrück	Equal access to higher edcuation	Off report of the 84th session of the 7th Lower Saxonian parl 26.11.1973, 8476
Hochschule Osnabrück	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of AppSc Ostfriesland in Leer	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of Vechta	Equal access to higher edcuation	Off report of the 84th session of the 7th Lower Saxonian parl 26.11.1973, 8476
U of AppSc Oldenburg in Elsfleth	Increasing number of students	See Valero and Van Reenen 2019, iv and the off report of the 93rd session of the 8th Lower Saxonian parl 11.05.1978
U of Breme	Increasing number of students	Off report of the 20th session of the 5th Bremische Bürgerschaft 28.06.1961, 515
Hochschule Bremerhaven	Restructuring of an existing institution	Off record 8/1437 about a memorandum of the Bremen Senate 03.06.1975, 5
U of AppSc Niederrhein in Krefeld	Equal access to tertiary education	Off report of the 17th session of the 7th parl of NRW 04.05.1971, 597
Hochschule of the Fed Gov for PubAdmin	Admin U, which only educates future civil servants of the FedGov	
U of AppSc for Jurisdiction in NRW	Restructuring of an existing institution	Off report of the 122nd session of the 7th parl of NRW 05.02.1975, 5178
U of AppSc Cologne Campus Gummersbach	Equal access to education	Off report of the 17th session of the 7th parl of NRW 04.05.1971, 597
U of PubAdmin and Finance NRW	Restructuring of an existing institution	Off report of the 122nd session of the 7th parl of NRW 05.02.1975, 5178
U of AppSc of Finance NRW	Restructuring of an existing institution	Off report of the 122nd session of the 7th parl of NRW 05.02.1975, 5178
U of AppSc Münster Campus Steinfurt	Equal access to tertiary education	Off report of the 17th session of the 7th parl of NRW 04.05.1971, 597
U of Paderborn	Equal access to tertiary education	Written statement of the state chancellery to the bill of the Minister of Science and Research 19.04.1971
U of AppSc Ostwestfalen-Lippe	Equal access to tertiary education	Off report of the 17th session of the 7th parl of NRW 04.05.1971, 597

U of AppSc Bielefeld Campus Minden	Equal access to tertiary education	Off report of the 17th session of the 7th parl of NRW 04.05.1971, 597
U of Bochum	Available building land; opposition would have favoured Dortmund, which is 17km away	Off report of the 66th session of the 4th parl of NRW 18.07.1961, 2370-2373
U of Siegen	Equal access to tertiary education	Written statement of the state chancellery to the bill of the Minister of Science and Research 19.04.1971
Hochschule RheinMain	Increasing number of students	Off report of the 78th session of the 6th Hessian parl 08.07.1970, 4115
U of AppSc Dieburg	Restructuring of an existing institution	Off report of the 78th session of the 6th Hessian parl 08.07.1970
Technische Hochschule Mittelhessen	Increasing number of students	Off report of the 78th session of the 6th Hessian parl 08.07.1970, 4115
U of Kassel	Equal access to tertiary education	Off report of the 76th session of the 6th Hessian parl 18.06.1970, 3996
Hochschule Trier Campus Birkenfeld	Increasing number of students	Off report of the 62nd session of the 6th parl of Rhineland-Palatinate 20.05.1970, 2299
Hochschule of the Deutschen Bundesbank	Admin U, which only educates future civil servants of the central bank	
TU Kaiserslautern	Increasing number of students	Off report of the 67th session of the 6th parl of Rhineland-Palatinate 13.07.1970, 2537
Hochschule Ludwigshafen am Rhein (FH)	Increasing number of students	Off report of the 62nd session of the 6th parl of Rhineland-Palatinate 20.05.1970, 2299
U of Mainz Campus Germersheim	Restructuring of an existing institution	Off report of the 74th session of the 6th parl of Rhineland-Palatinate 22.12.1970, 2791
Hochschule Ludwigsburg	Increasing number of students	Off report of the 75th session of the 5th parl of Baden-Württemberg 08.07.1970, 4271, 4282
Hochschule for Technic and Business Heilbronn	Increasing number of students	Off report of the 75th session of the 5th parl of Baden-Württemberg 08.07.1970, 4271, 4282
U of PubAdmin Kehl	Increasing number of students	Off report of the 75th session of the 5th parl of Baden-Württemberg 08.07.1970, 4271, 4282, 4288
Hochschule for Technic and Business Albstadt-Sigmaringen	Increasing number of students	Off report of the 75th session of the 5th parl of Baden-Württemberg 08.07.1970, 4271, 4281, 4282
U of AppSc Nürtingen in Geislingen	Equal access to tertiary education; economic improvment	Off record 12/419 about a statement of the Ministry of Science in Baden-Württemberg 21.11.1996, 3
U of Ulm	Increasing number of students	Memorandum on the establishment of U in Baden-Württemberg 16.04.1963
U of AppSc Biberach an der Riss	Increasing number of students	Off report of the 75th session of the 5th parl of Baden-Württemberg 08.07.1970, 4271, 4282
Pedagogical U Weingarten	Increasing number of students	Off report of the 75th session of the 5th parl of Baden-Württemberg 08.07.1970, 4271, 4282
U of AppSc Rosenheim	Increasing number of students	See Valero and Van Reenen (2019, iv) and the off report of the 101st session of the 6th Bavarian parl 30.09.1970
Hochschule Weihenstephan-Triesdorf	Increasing number of students	See Valero and Van Reenen (2019, iv) and the off report of the 101st session of the 6th Bavarian parl 30.09.1970
U of PubAdmin in Bavaria	Admin U, which only educates future civil servants	

U of AppSc Landshut	Equal access to tertiary education; increasing student numbers	Off report of the 79th session of the 8th Bavarian parl 24.05.1977, 4232
U of Passau	Equal access to tertiary education; economic improvement	Off report of the 49th session of the 7th Bavarian parl 07.12.1972, 2607
East Bavarian TU Regensburg	Increasing number of students	See Valero and Van Reenen (2019, iv) and the off report of the 101st session of the 6th Bavarian parl 30.09.1970
East Bavarian TU Amberg-Weiden	Increasing number of students	See Valero and Van Reenen (2019, iv) and the off report of the 101st session of the 6th Bavarian parl 30.09.1970
U of Bayreuth	Equal access to tertiary education; economic improvement	Off report of the 98th session of the 6th Bavarian parl 16.07.1970, 4603
U of AppSc Würzburg-Schweinfurt	Increasing number of students	See Valero and Van Reenen (2019, iv) and the off report of the 101st session of the 6th Bavarian parl 30.09.1970
U of AppSc Kempten	Equal access to tertiary education; increasing student numbers	Off report of the 79th session of the 8th Bavarian parl 24.05.1977, 4232

Notes: This table includes West-German university foundations during the 1960s and 70s. The founding date is taken from the historic literature (see Verger 1992, 62-65; Frijhoff 1996, 90-94; Rüegg 2004, 2011) or the German rectors conference (Hochschulrektorenkonferenz 2022). The sample is based on the universities present in the regression of Table column 3. I only list one university per county and exclude universities from counties with already existing universities. **Abbreviations**: AppSc Applied Sciences; FedGov Federal Government; Off Official; NRW North Rhine-Westphalia; parl parliament; PubAdmin Public Administration; TU Technical University; U University

In line with previous literature the main reasons for university foundations are the increasing number of students and the political aim to provide equal access to tertiary education (see Andersson et al. 2009; Valero and Van Reenen 2019, iv; Neave 2011, 52). There is no anecdotal evidence in the plenary protocols about high-performing firms or regions, which need a complementary university.

Chapter 4

Multinational Firms and the Urban-Rural Productivity Gap

"When an industry has thus chosen a locality for itself, it is likely to stay there long: so great are the advantages [...] Good work is rightly appreciated, inventions and improvements in machinery, in processes and the general organization of the business have their merits promptly discussed: if one man starts a new idea, it is taken up by others and combined with suggestions of their own; and thus it becomes the source of further new ideas." (Marshall, 1890, Book IV, Chapter X, §3)

4.1 Introduction

Multinational firms (MNFs) are widely perceived to be exceptional producers. While scarce in number, they outperform their competitors in terms of economic activity and contribute a significant share to overall production (see, e.g., OECD, 2007; Yeaple, 2013). German firm-level data presented in our analysis confirms these findings. Although MNFs only account for about 2.1 percent of firms, they employ more than 14.6 percent of the total workforce and generate more than 31.4 percent of the revenues of private companies in manufacturing and services. In this chapter, we show that MNFs are not only exceptional regarding their performance, but they also differ considerably from national firms (NFs) in terms of their locational patterns.

To study the differences in the location of national and multinational firms, we employ information from the German business register, which contains administrative data, covering the universe of firms with at least one establishment in Germany. The dataset is provided by the German Federal Statistical Office and contains for the period 2013 to 2019 detailed information on ownership, revenues, employees, industry affiliation, and the municipality of firms. We use the ownership information to distinguish

MNFs and NFs in our analysis. Moreover, we use the municipality information to assign firms to urban and rural areas and to draw a detailed picture of the differences of firms between geographic regions in Germany. Regarding the performance of NFs, our analysis thereby reproduces previous findings of an urban productivity premium that is commonly attributed to positive technology spillovers between firms in close spatial proximity or, more generally, to agglomeration effects in urban areas (see Rosenthal and Strange, 2004; Ciccone and Hall, 1996).[1] However, our data does not show a comparable urban productivity premium for MNFs. In fact, MNFs have considerably higher productivity in rural areas compared to urban ones.

To motivate our empirical analysis and provide a theoretical explanation for the observed productivity differences between MNFs and NFs, we consider a linear city model with price-location competition between two firms, as proposed by Hotelling (1929) and d'Aspremont et al. (1979). We extend this textbook model of spatial competition in two important dimensions. Firstly, we allow firms to locate both inside and outside the city borders to distinguish producers in urban and rural areas (see Lambertini, 1994; Matsumura and Matsushima, 2012). Secondly, we consider production costs to be location-specific and increasing in the distance between firms to capture the role of technology spillovers (see Mayer, 2000; Piga and Poyago-Theotoky, 2005; Colombo and Lambertini, 2023).

In the theoretical model, we first examine the consequences of technology spillovers and demonstrate that their existence induces firm to locate closer to the centre. For sufficiently low transport costs, firms in this augmented framework find it advantageous to locate inside the city borders. In a second step, we analyse the incentive of an MNF that utilises superior technology to acquire an NF inside or outside the city borders. We show that the MNF prefers an acquisition outside the city borders to reduce its outgoing spillovers and the associated profit loss from making the remaining NF more competitive. This incentive differs considerably from a setting without spillovers, where the MNF, due to its technological leadership, prefers acquiring an NF within the city borders. Our theoretical model provides a simple framework that can produce the productivity patterns observed in our data. It leads to an urban productivity premium in terms of lower unit production costs for NFs and shows that MNFs prefer rural locations if they are the technology leader and risk losing part of their cost advantage over NFs through outgoing technology spillovers. To the extent that MNFs with a

[1] An early discussion of agglomeration effects and their potential sources can be found in Marshall (1890). He mentions technology spillovers, labour pooling, and intermediate input connections as three important sources for their existence, whereas the more recent literature distinguishes sharing, matching, and learning effects (see Duranton and Puga, 2004).

small cost advantage are not prone to technology dissipation and therefore prefer urban locations, our model also accounts for a rural productivity premium for them.

Based on these theoretical insights, we then perform an empirical analysis in three steps. In a first step, we employ an OLS estimator to show evidence for three empirical regularities, namely *(i.)* a multinational productivity premium of 36.1 log points that is similar in size to the estimates reported, e.g., by Griffith (1999), Girma et al. (2015), and Fons-Rosen et al. (2021); *(ii.)* a moderate urban wage premium for NFs of 0.9 log points, which lies in the range of estimates reported by other studies (see Melo et al., 2009, for an overview); and *(iii.)* a so far unexplored rural productivity premium of MNFs of 4.4 log points. To make sure that our findings are not subject to an omitted variable bias, we consider a large set of firm and regional controls as well as state, industry, and time dummies, with the estimated effects being robust to changes in the set of explanatory variables. We also demonstrate that the identified rural productivity premium is specific to firms within *foreign* multinational business groups, whereas no similar effect is observed for firms within domestic business groups. Conversely, for firms within domestic groups, we find an urban productivity premium that exceeds that of standalone producers not affiliated with any business group. Although data availability restricts us to measuring firm productivity by revenues per employee for large parts of our analysis, we show that our results remain robust when using estimated levels of total factor productivity as the endogenous variable in a subsample of firms with more detailed information.

In a second step of our empirical analysis, we look into the question whether the observed productivity differences can be attributed to firm owners, as suggested by a large body of research emphasising the technology advantage of MNFs over NFs (see, e.g., Helpman et al., 2004; Ramondo, 2009; Arnold and Javorcik, 2009; Yeaple, 2013). As it is common practice in the literature, we use methods from programme evaluation to isolate the productivity stimulus due to takeover by a foreign multinational business group (foreign takeover, in short) from pre-existing heterogeneity in the productivity levels of acquired and non-acquired firms. We thereby estimate the average treatment effect on a pre-matched sample of acquired and non-acquired firms, following Abadie and Imbens (2016). For the full sample of all acquired firms and their matched control units we confirm the important insight from previous research that the multinational productivity premium estimated with OLS is to a considerable part explained by an instantaneous productivity stimulus from foreign takeover in the period of ownership change. This is conducive to the idea of a technology transfer in the process of acquisition (see Mansfield and Romeo, 1980; Keller and Yeaple, 2013).

Estimating foreign takeover effects separately for firms in urban and rural areas, we show that the productivity stimulus is not equally strong in all regions. Instead, we find a stronger productivity stimulus from foreign takeover in rural than in urban areas, largely consistent with our OLS estimates. The results from the two-stage treatment effects estimator provide additional insights into a potential mechanism explaining the rural productivity premium for MNFs in our data. MNFs, which are commonly seen as technology leaders, may prefer rural areas to avoid knowledge dissipation through *outgoing technology spillovers*. While there exists clear evidence that MNFs pay higher wages to reduce turnover and to avoid such dissipation (see, e.g., Glass and Saggi, 2002; Girma and Görg, 2007), our results indicate that choosing larger distance to potential competitors may provide an alternative way to achieve this goal. By choosing a rural location over an urban one, firms naturally forgo potential agglomeration benefits from positive *incoming technology spillovers* from nearby competitors (see Duranton and Puga, 2004, for a review of possible explanations for their existence). However, for technology leaders, the benefits associated with incoming spillovers are in general lower than the losses from outgoing spillovers, making a location in rural areas more attractive for them.[2]

Following this line of reasoning, we analyse in a third step to what extent the presence of competitors influences the productivity premium of MNFs. In an OLS regression, we demonstrate that a rural productivity premium for MNFs exists in our data when the rural labour market region not only has a lower population density but also hosts fewer competitors from the same or other industries. To see whether this finding is the result of location choice, we re-estimate the treatment effect of foreign takeover, when conditioning the matching of acquired and non-acquired firms also on the number of competitors active in the same industry and labour market region. While considering the additional matching covariate does not change the important insight that foreign takeover increases the productivity of German firms through technology transfer, the stronger rural productivity stimulus from ownership change disappears in this case. We consider this result as evidence for the location of MNFs in rural areas being indeed a strategic choice to lower potential losses from outgoing technology spillovers. By matching on the number of competitors, we eliminate this motive and

[2] In distinguishing between incoming and outgoing spillovers, we adopt the classification used by Cassiman and Veugelers (2002). Building on this framework, Cappelli et al. (2014) demonstrate that spillovers from competitors lead to increased imitation, which is particularly advantageous for NFs not operating at the technological frontier. Combes and Duranton (2006) explore labour poaching as a specific channel through which both incoming and outgoing technology spillovers occur, showing that such spillovers influence strategic location decisions.

thereby prevent the excessively strong productivity transfer typically observed during ownership changes in rural areas. In this respect, our results align well with the findings of Mariotti et al. (2010, 2019), who show that in Italy, MNFs avoid regional proximity to their national competitors to reduce outgoing technology spillovers. We also investigate whether foreign parents choose to transfer less technology to their German affiliates in regions with a higher number of competitors, but we do not find evidence supporting this.

Providing a detailed analysis of the productivity premium of foreign-owned multinational firms in Germany, our analysis contributes to various strands of research. For instance, the productivity transfer from the acquiring to the acquired firm in the process of takeover is extensively discussed in the literature on multinational firms (see Girma et al., 2015; Fons-Rosen et al., 2021). In comparison to existing studies from this literature, we look more closely into the role of locational factors for explaining the strength of productivity transfer. The rural productivity premium of MNFs identified in our analysis points to the location decision as a so far largely unexplored strategic choice of foreign investors for lowering the risk of technology dissipation.[3] In this respect, our analysis contributes to a growing literature in economics and business administration studying regional location decisions of MNFs (see Alfaro and Chen, 2014; Hervás-Oliver, 2015; Tomás-Miquel et al., 2018).

Our analysis also contributes to research on the urban productivity premium. This premium is usually explained by agglomeration effects (see Henderson, 2003; Moretti, 2004, for two early empirical contributions). We show that evidence for an urban productivity premium cannot be equally found for all producers, which is in line with recent evidence emphasising that the productivity increase caused by agglomeration in urban areas may be exaggerated if one does not account for firm heterogeneity and the self-selection of more productive firms into cities (see Combes et al., 2012; Behrens et al., 2014; Gaubert, 2018). The varying strength of agglomeration effects for ex ante heterogeneous firms is often explained by the appealing notion that the most productive firms have much to offer in terms of knowledge provision but little to gain from learning from others (see Shaver and Flyer, 2000; Bloom et al., 2013, among others). This notion helps explain the observed clustering of high-productivity (multinational) firms, as reported by Alfaro and Chen (2014, 2019) and Baum-Snow et al. (2024), which aligns well with our findings.

Finally, our proposed model contributes to the theoretical literature that utilises

[3]For an overview of other strategies to prevent technology dissipation, see de Faria and Sofka (2010); Egger et al. (2020).

the Hotelling model for studying the role of technology spillovers in the price-location competition of firms. Examples include Piga and Poyago-Theotoky (2005) and, more recently, Colombo and Lambertini (2023), with the latter providing an excellent review of this literature. Unlike this body of research, we do not focus on R&D investment strategies in the presence of spillovers. Instead, we examine the role of technology spillovers in the acquisition of a national firm by a foreign multinational producer that has access to a superior technology. Since the existence of spillovers makes production costs location-specific, our model is related to Mayer (2000), who, in contrast to us, considers quantity competition in the absence of spillovers. By emphasising the role of spillovers, we highlight a mechanism for explaining agglomeration effects in the Hotelling model that differs from Egger and Egger (2007), who consider the role of a common input producer.

The remainder of the chapter is organised as follows. In Section 4.2, we introduce an augmented Hotelling model to examine the role of technology spillovers in the location choices of two national firms and the incentive for a multinational firm to acquire one of them. In Section 4.3, we introduce our dataset, define urban and rural areas, and present descriptive statistics. Section 4.4 outlines the empirical methodology, discussing the OLS regression model and a two-stage procedure that combines propensity score matching with an average treatment effects estimator. We also present and discuss our findings on the multinational productivity premium, the urban productivity premium for national firms, and the rural productivity premium for multinational firms. In Section 4.5, we explore a mechanism that may explain the rural productivity premium for MNFs. Finally, Section 4.6 summarises the key results of our analysis.

4.2 Theoretical motivation

We consider a linear city whose geography is represented by the unit interval. Consumers have mass one, live within the city borders and are distributed uniformly over the unit interval. There are two firms, indexed $j = 1, 2$, who first choose their location and afterwards set their prices and produce (see Hotelling, 1929; d'Aspremont et al., 1979). The two firms produce at constant marginal costs, c_j, and set uniform mill prices for all consumers. However, the existence of transport costs, which are quadratic in the distance to producers, imply differences in the prices actually paid by consumers.

Following the common approach, we assume that each consumer purchases at most one unit of the product offered by the two firms and has a maximum willingness to pay of $v_0 > 0$ for it. Since distance and prices matter, utility depends on the firm, the product is purchased from. For a consumer with address $x \in (0,1)$, utility is given by

$$v_x = \begin{cases} v_0 - t(x-x_1)^2 - p_1 & \text{if product is purchased from firm 1} \\ v_0 - t(x-x_2)^2 - p_2 & \text{if product is purchased from firm 2} \end{cases}, \tag{4.1}$$

where x_j is the location of firm j and p_j its mill price. Moreover, $t > 0$ is a common transport cost parameter. Using Eq. (4.1) we can determine the address of a consumer that is indifferent between purchasing from the two firms as

$$\overline{x} = \frac{x_1 + x_2}{2} + \frac{p_1 - p_2}{2t(x_1 - x_2)}. \tag{4.2}$$

Of course, existence of an indifferent consumer requires that *(i.)* both firms are active and serve some consumers and that *(ii)* the two firms jointly serve the whole market. This is guaranteed if cost differences are not too large and, compared to production and trade costs, the willingness to pay of consumers is sufficiently high. We consider both of these requirements to be fulfilled and focus without loss of generality on an outcome with $x_1 \leq 1/2 \leq x_2$. In this case, consumer demand for production of the two firms can be derived in a straightforward way as

$$d_1 = \frac{x_1 + x_2}{2} + \frac{p_2 - p_1}{2t(x_2 - x_1)}, \qquad d_2 = 1 - \frac{x_1 + x_2}{2} - \frac{p_2 - p_1}{2t(x_2 - x_1)}, \tag{4.3}$$

respectively. This concludes the discussion of the consumers' problem.

Firms face a two-stage maximisation problem, involving the choice of location in stage 1 and the choice of prices in stage 2. We can solve their problem through backward induction, beginning with the determination of mill prices. Maximising profits $\pi_j = p_j d_j - c_j d_j$ subject to the consumer demand in Eq. (4.3) yields the preferred price choices:

$$\begin{aligned} p_1 &= \frac{t(x_2 - x_1)(2 + x_1 + x_2) + 2c_1 + c_2}{3}, \\ p_2 &= \frac{t(x_2 - x_1)(4 - x_1 - x_2) + c_1 + 2c_2}{3}. \end{aligned} \tag{4.4}$$

Substituting these prices into π_j, $j = 1, 2$, establishes firm-level profits as function of location choices:

$$\begin{aligned} \pi_1^* &= \frac{t(x_2 - x_1)}{2} \left[\frac{t(x_2 - x_1)(2 + x_1 + x_2) + c_2 - c_1}{3t(x_2 - x_1)} \right]^2, \\ \pi_2^* &= \frac{t(x_2 - x_1)}{2} \left[\frac{t(x_2 - x_1)(4 - x_1 - x_2) + c_1 - c_2}{3t(x_2 - x_1)} \right]^2. \end{aligned} \tag{4.5}$$

The profits in Eq. (4.5) are well-known in the literature on linear cities for producing strong differentiation of producers regarding their location choices. In the case of symmetric firms with exogenous unit costs $c_1 = c_2$, two producers that are constrained to stay inside city borders (and thus in the urban area) will locate at the bounds of the unit interval at $x_1 = 0$, $x_2 = 1$ and make profits $\pi_1^* = \pi_2^* = t/2$. In contrast, firms that are unconstrained in their location choice would prefer to locate in the rural area outside the city borders at $x_1 = -1/4$, $x_2 = 5/4$ and achieve profits $\pi_1^* = \pi_2^* = 3t/4$ (see d'Aspremont et al., 1979; Lambertini, 1994; Matsumura and Matsushima, 2012, for a discussion).

We consider an augmented framework in which firm location also influences production costs through technology spillovers. Spillovers have two forms. On the one hand, they lead to technology dissipation, making the competitor more productive (*outgoing spillovers*). On the other hand, they allow to learn from the competitor making the firm more productive (*incoming spillovers*). To capture both forms of spillovers and link them to the locations of the two firms, we choose the following cost specification:

$$c_j \equiv \begin{cases} \frac{1+|1/2-x_j|}{\Pi_{k=1,2}(1-|1/2-x_k|)} & \text{if} \quad -\frac{1}{2} < x_1 \leq x_2 < \frac{3}{2} \\ \infty & \text{otherwise} \end{cases}.$$

These unit costs have two elements. The first element is $\Pi_{k=1,2}\left([1-|1/2-x_k|\right)^{-1}$, which captures the extent to which the two firms contribute to the common knowledge stock through outgoing spillovers. Knowledge production features strong complementarity between the outgoing spillovers of firms and takes place in the city centre. The second element is $1+\left|\frac{1}{2}-x_j\right|$, which captures the extent to which firm j can benefit from the common knowledge stock through incoming spillovers. Acknowledging our assumption that $x_1 \leq 1/2 \leq x_2$, we can write[4]

$$c_1 = \frac{3/2 - x_1}{(1/2+x_1)(3/2-x_2)}, \qquad c_2 = \frac{1/2 + x_2}{(1/2+x_1)(3/2-x_2)}. \tag{4.6}$$

Substituting Eq. (4.6) into the firm-level profits π_j^* from Eq. (4.5) and differentiating with respect to x_j yields the first-order conditions for the profit-maximising location

[4]To keep the analysis simple, we do not give firms access to an alternative production technology without spillovers. Provided that this alternative technology features unit costs larger than 28, its existence would not matter for our analysis.

choices of the two firms as

$$\begin{aligned}\frac{d\pi_1^*}{dx_1} &= \frac{d_1}{3(x_2-x_1)}\left[t(x_2-x_1)(x_2-3x_1-2)+\frac{2x_1^2-4x_2^2+6x_1x_2+7x_2-7x_1-1}{2(1/2+x_1)^2(3/2-x_2)}\right],\\ \frac{d\pi_2^*}{dx_2} &= \frac{d_2}{3(x_2-x_1)}\left[t(x_2-x_1)(4-3x_2+x_1)+\frac{4x_1^2-2x_2^2-6x_1x_2+3x_2+5x_1-3}{2(1/2+x_1)(3/2-x_2)^2}\right],\end{aligned} \quad (4.7)$$

respectively.[5] Evaluating Eq. (4.7) at location choices $x_1 = -1/4, x_2 = 5/4$ we find that $d\pi_1^*/dx_1 = 16/3 > 0$ and $d\pi_2^*/dx_2 = -16/3 < 0$. This implies that, compared to a model without technology spillovers, moving closer to the city centre is attractive for both producers. The following proposition provides further details on the profit-maximising location choices.

Proposition 1 *If $t > 1$, there exists a Nash equilibrium in pure strategies, in which the two firms maximise their profits by choosing locations with symmetric distance to the city centre, $x_1 = x_1^*$ and $x_2 = 1 - x_1^*$. These locations are implicitly determined by $t(1+2x_1^*)^2(1+4x_1^*) = 8$ and lie in the interval $(-1/4, 4/10)$ for firm 1 and in the interval $(6/10, 5/4)$ for firm 2. The two firms locate outside the city borders if f $t > 8$, while they locate inside the city borders if $t < 8$.*

Proof See Appendix 4.A.1.

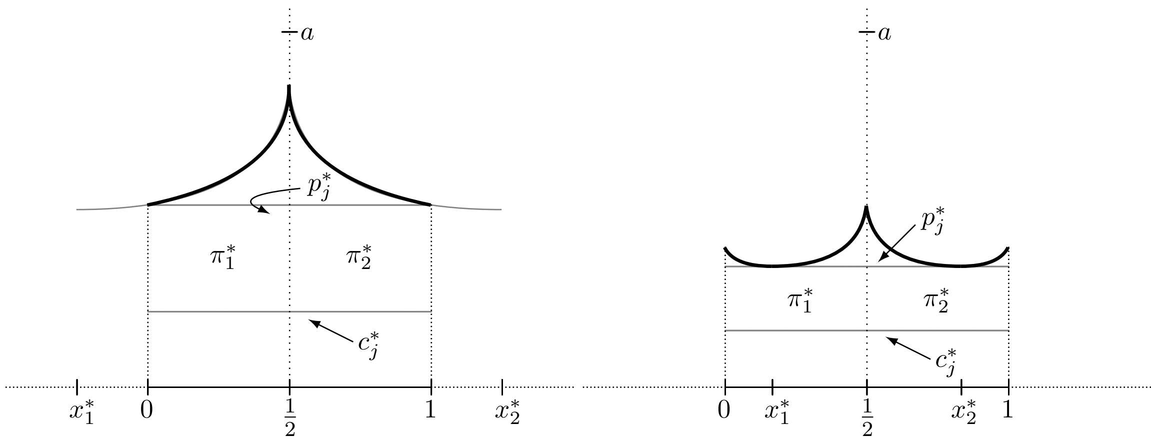
Proposition 1 highlights the role of transport cost parameter t in determining the profit-maximising location choices. A lower level of t makes cost saving more attractive, as the resulting decrease in prices leads to a significant increase in market size. This enhances the appeal for firms to move closer to the city centre. However, if the transport cost parameter t is too low, the incentive of firms to move closer to the city centre to gain a cost advantage over their competitor becomes too strong, so that the increased competition between nearby firms makes relocating away from the city centre an attractive choice, thereby eliminating the Nash equilibrium in pure strategies.

In the price-location equilibrium characterised by Proposition 1, firms have the same unit costs and hence their profits are given by $\pi_1^* = \pi_2^* = t(1-2x_1^*)/2$, according to Eq. (4.5). Therefore, choosing locations $x_1 > -1/4$ and $x_2 < 5/4$ does not increase firm-level profits despite the cost reduction from spillovers. This is because the potential profit gain from cost reduction is offset by stronger product market competition, implying that profits are lower for locations inside the city borders than outside of

[5] Using Eqs. (4.3) and (4.4), we compute $d_1 = (2 + x_1 + x_2)/6 + (c_2 - c_1)/[6t(x_2 - x_1)]$ and $d_2 = (4 - x_1 - x_2)/6 - (c_2 - c_1)/[6t(x_2 - x_1)]$.

Figure 4.1: *Price-location equilibrium in a Hotelling model with technology spillovers*

Panel A: Location outside of city borders   Panel B: Location inside of city borders



them. Figure 4.1 illustrates the possible outcomes of profit-maximising price-location choices.[6]

We next analyse the incentives of a foreign multinational firm (MNF) to acquire one of the two national firms (NFs). The MNF is a technology leader with zero production costs and can either acquire an NF outside the city border (Panel A in Figure 4.1) or acquire an NF inside the city borders (Panel B of Figure 4.1). For a successful acquisition, the MNF must buy out the previous owners by compensating them for their forgone profits. Thus, it must pay $t(1-2x_1^*)/2$ for a successful acquisition. Without loss of generality, we assume that firm 1 is the acquisition target. Due to incoming spillovers, firm 2 can (imperfectly) learn about the MNF's technology, leading to production costs $c_2 = 2a_c(3-2x_1^*)/(1+2x_1^*)^2$, where $a_c \in (0,1)$ measures the absorptive capacity of firm 2 to implement the better technology and therefore determines the degree of technology dissipation.

Using Eqs. (4.3)-(4.5), we compute the profit stimulus for firm 1 from a foreign acquisition as[7]

$$\Delta\pi_1^* = \begin{cases} \frac{a_c}{36}\frac{(3-2x_1^*)\left[24(1-2x_1^*)+a_c(1+4x_1^*)(3-2x_1^*)\right]}{(1+2x_1^*)^2(1-2x_1^*)} & \text{if} \quad \frac{a_c(3-2x_1^*)(1+4x_1^*)}{12(1-2x_1^*)} < 1 \\ 2\frac{a_c(3-2x_1^*)(1+4x_1^*)-6(1-2x_1^*)}{(1+2x_1^*)^2(1+4x_1^*)} & \text{otherwise} \end{cases}, \tag{4.8}$$

[6]In the Nash equilibrium characterised by Proposition 1, production costs and prices of firm $j = 1,2$ are given by $c_j^* = 2(3-2x_1^*)/(1+2x_1^*)^2$ and $p_j^* = t(1-2x_1^*) + c_j^*$, respectively.

[7]Derivation details are deferred to Appendix 4.A.2.

where the second line refers to a case in which the foreign firm acts as a monopolist and serves all consumers: $d_1 = 1$. The following proposition summarises our main finding regarding the relative attractiveness for the MNF to acquire an NF inside or outside of city borders.

Proposition 2 *In the presence of technology spillovers, a multinational firm with zero production costs always prefers acquiring a national firm outside the city borders over acquiring a national firm inside the city borders.*

Proof See Appendix 4.A.3.

Since location outside of city borders is associated with higher ex ante profits for national firms, acquiring a firm from the rural area is more expensive for the MNF. However, this higher acquisition cost is immaterial for the MNF because a larger distance to its remaining rival reduces the negative effects of outgoing spillovers which make firm 2 more competitive and reduce the MNF's profits. The negative effect of outgoing spillovers persists even if the MNF's cost advantage is sufficient to monopolise the market. This is because moving closer to the then inactive competitor lowers the maximum price that the MNF can be set to keep firm 2 out of the market.

To see that our finding of the MNF preferring the acquisition of an NF outside the city borders is indeed a consequence of technology spillovers, we can examine the acquisition incentives in an alternative model lacking them. Denoting the then exogenous unit production cost disadvantage of firm 2 by $\bar{c}_2 > 0$, we can make use of Eq. (4.3)-(4.5) to express firm 1's profit stimulus from acquisition as

$$\Delta\tilde{\pi}_1^* = \begin{cases} \frac{\bar{c}_2}{18}\left[6 + \frac{\bar{c}_2}{t(1-2x_1^*)}\right] & \text{if} \quad \frac{\bar{c}_2}{3t(1-2x_1^*)} < 1 \\ \bar{c}_2 - \frac{3t(1-2x_1^*)}{2} & \text{otherwise} \end{cases}, \tag{4.9}$$

where the second line again refers to an outcome, in which the MNF monopolises the market.[8] Eq. (4.9) reveals that $\Delta\pi_1^*/dx_1^* > 0$ if technology spillovers do not exist. This implies that the MNF prefers acquisition of an NF inside the city borders to one outside the city borders in this case. Of course, in the absence of technology spillovers, location inside the city borders requires constrained location choices, as considered, for instance, by d'Aspremont et al. (1979).

The theoretical model outlined above, while parsimonious in various dimensions, provides a straightforward framework to illustrate potential productivity differences between firms in urban and rural areas. It demonstrates an urban productivity premium

[8] In the derivation of Eq. (4.9) a location pattern with $x_2^* = 1 - x_1^*$ has been considered.

through lower unit production costs for NFs and indicates that MNFs, when they are technology leaders, tend to favour rural locations to avoid losing their cost advantage to outgoing technology spillovers. Additionally, for MNFs the model suggests a rural productivity premium, as foreign multinationals with a minor cost advantage who are less susceptible to technology dissipation may prefer urban locations. This completes our theoretical analysis.

4.3 Data input

For our empirical analysis, we use business register data from the *Unternehmensregisterstatistik* covering the universe of firms with at least one establishment in Germany. This dataset is provided by the Federal Statistical Office of Germany (Destatis) and available for the years 2002 to 2019 (see Destatis, 2021, for further details). It contains administrative data on revenues, employees, industry affiliation, and the municipality of firms. Beyond that Destatis has added ownership information from commercial suppliers, which allows us to identify ownership of firms belonging to business groups. Data on multinational ownership is available from 2007 onwards. However, structural breaks in the ownership variable limits our analysis to the period 2013 to 2019.

Since revenues are reported at the firm level rather than the establishment level and since reliable employment information is not available for all establishments in our data, we use the firm as our preferred unit of observation. Moreover, since firms with multiple establishments cannot be uniquely assigned to a single municipality, we focus on single-establishment firms in our analysis, while considering firms with multiple establishments in a robustness check. Firms can operate independently as standalone producers or they can belong to business groups, whose ultimate owners may be German or foreign. If the ultimate owner is foreign, firms are classified as (foreign) multinational firms (MNFs). While we can also identify firms belonging to domestic business groups, we lack information on foreign affiliates of these groups and hence are not able to distinguish domestic business groups only active at home and domestic business groups also active abroad. In the interest of a clean identification, we therefore eliminate firms belonging to domestic business groups from our data in large parts of our analysis, while considering them in robustness checks. Regarding firms in German ownership, we therefore focus on standalone producers, which are classified as national firms (NFs). We eliminate in a final step all public firms as well as firms, whose main activity is either in agriculture, hospitality, housing or retail, and we drop firms lacking information on their industry, annual revenues, number of employees, or municipality.

Using the municipality information we can link the business register data to additional datasets with detailed regional information. The first one is the administrative regional statistics (*Regionaldatenbank*) from the Federal Statistical Office, which provides information on population, density, local taxes and unemployment rates (see Destatis, 2025, for further details). The second dataset comes from the Federal Office of Cartography and Geodesy and provides information on local infrastructure (see BKG, 2025). The third dataset is from the Federal Institute for Research on Building, Urban Affairs and Spatial Development and provides information on the division of Germany into various regional subdomains, including states, labour market regions, counties and municipalities (see BBSR, 2012, for an overview). The ultimate regional firm database used for our analysis (RegFiD, in short) covers a sample of 1,169,131 distinct firms and 5.2 million firm-year observations in manufacturing and services.

Since spillovers have been identified as a key factor explaining regional patterns of firm location (Mariotti et al., 2010; Baum-Snow et al., 2024) and since the literature highlights worker mobility as a crucial factor in the existence of spillovers (e.g., Song et al., 2003; Combes and Duranton, 2006; Stoyanov and Zubanov, 2012), we select the 257 labour market regions of Germany as the primary spatial units for our analysis. These labour market regions comprise various counties connected by workers' commuting behaviour (see BBSR, 2025). Given the lack of consensus on distinguishing urban from rural areas (see, e.g., Dauth et al., 2022; Hirsch et al., 2022, for discussions of various concepts), we adopt a straightforward approach: regions with population densities above the national median of 162 people per square kilometre are classified as urban areas. To ensure consistency over time, we base the urban-rural classification on population densities observed in 2007.[9] Figure 4.2 provides an overview of the urban and rural areas in Germany resulting from this classification.[10]

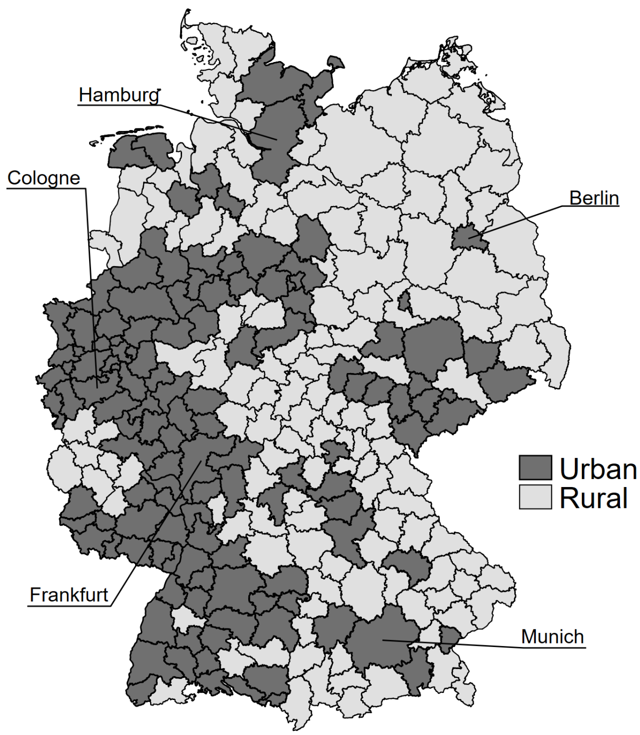
In Table 4.1 we show the descriptive statistics for our dataset. The average single-establishment firm in Germany generates 12.708 log (Euro) revenues and has 1.225 log regular employees.[11] This leads to an average productivity of 11.483 log revenues per employee. All three measures of economic activity show considerable variation between firms. While the German business register data lacks detailed information on workforce

[9] The population density shows sizeable differences among labour market regions ranging from around 40 people per square kilometre in rural regions to around 3,600 people per square kilometre in Berlin.

[10] Given the somewhat arbitrary distinction between urban and rural areas, we demonstrate the robustness of our results using various alternative definitions in the Appendix.

[11] Regular employees are defined as workers who are subject to social security payments.

Figure 4.2: *Urban and rural areas in Germany*



Notes: The figure depicts the 257 labour market regions. Regions with a population density above the national median are defined as urban areas, the others as rural ones. The five largest urban areas are indicated by name.

composition, it does report the number of marginal employees, who are not subject to social security payments and are typically less skilled and perform more routine tasks compared to regular employees (see Garz, 2013, for a discussion). We utilise the information on marginal employees in two ways. First, we calculate the share of regular employees as a simple measure of skill composition and create a dummy variable that takes a value of one if the share of regular employees exceeds 0.75. We use this dummy variable to identify firms with high skill intensity in their production processes. Second, we consider the log number of marginal employees as a proxy for workplace complexity, with a higher number of marginal employees indicating lower complexity.[12] Overall, marginal employees appear to play a minor role in our dataset, because a majority of firms have skill-intensive and relatively complex production processes, according to our proxy variables. Regarding regular employees, we observe strong employment growth of more than 10 percent over the previous business year in almost 30 percent

[12]Since not all firms employ marginal employees, we add one to each observation before taking logs to avoid a significant drop in the number of observations.

of firm-year observations. Approximately 1.7 percent of the firm-year observations are classified as multinationals.

In addition to the firm descriptives, Table 4.1 also reports six regional variables. The first is population density, measured as the number of inhabitants per square kilometer (in units of 100) at the level of labour market regions, which is used to classify these regions as urban or rural. Nearly 75 percent of the firm-year observations are from urban areas with a population density above the median labour market region. Regional economic factors are captured by two variables. First, we consider the trade tax multiplier, which is set by German municipalities to generate tax revenues. Since the multiplier is missing in our dataset for some municipalities and years (a problem more pronounced in Eastern Germany), we report its employment-weighted average at the county level (in units of 100). The mean multiplier level is 3.99 and thus lower than the (unweighted) German-wide average of 4.35 reported in official statistics by Destatis. This indicates that counties hosting more firms set lower trade tax multipliers. Second, we consider the unemployment rate at the municipality level, which is 8.4 percent and thus significantly higher than the German-wide rates reported by official statistics (Destatis, 2025). This discrepancy exists because firms cluster in larger cities, which also have a higher unemployment rate than rural municipalities. At the municipality level, we also construct dummies for local airports and highway connections to capture the ease of access to firms.

Table 4.1 also presents the variable means for four subdivisions of our data. These subdivisions are based on two dimensions: the firm's location (urban or rural) and their status as either MNFs or NFs. There are notable differences in the performance of firms between these subdivisions. In terms of log revenues and the log number of employees MNFs are larger than NFs in both urban and rural areas. They are also more productive according to their log revenues per employee. Moreover, contrasting NFs between labour market regions, we observe an urban productivity premium in line with previous research (see Henderson, 2003; Moretti, 2004). However, for MNFs we find productivity to be higher on average for firms in rural than in urban areas. With respect to regional factors, we observe that unemployment rates are lower and accessibility is better for the average MNF in our data. Tax differences seem to be of similar relevance for the location choices of MNFs and NFs.

Compared to other data used for estimating productivity differences between MNFs and NFs, the RegFiD database has both advantages and disadvantages. One advantage is the high credibility of administrative data compared to widely used survey data. Additionally, having access to the universe of German firms allows us to observe

Table 4.1: *Descriptive statistics*

	Full sample					MNFs in Rural-areas	NFs in Rural-areas	MNFs in Urban-areas	NFs in Urban-areas
	Mean	SD	P1	P50	P99	Mean	Mean	Mean	Mean
					Firm-level variables				
Log Revenues	12.708	1.275	10.127	12.605	16.257	15.092	12.682	14.625	12.670
Log No. of Employees	1.225	1.119	0.000	1.099	4.454	3.262	1.206	2.841	1.192
Log Revenues/Employee	11.483	0.836	9.043	11.503	13.611	11.830	11.476	11.784	11.478
High Skill Intensity	0.722	0.448	0.000	1.000	1.000	0.942	0.732	0.940	0.714
Low Workplace Complexity	0.518	0.678	0.000	0.000	2.708	0.587	0.500	0.450	0.526
Strong Employment Growth	0.228	0.420	0.000	0.000	1.000	0.217	0.224	0.255	0.230
Multinational Firm	0.017	0.130	0.000	0.000	1.000	1.000	0.000	1.000	0.000
					Regional variables				
Population Density	6.127	8.308	0.545	3.164	40.902	1.164	1.138	9.201	7.789
Urban Area	0.747	0.435	0.000	1.000	1.000	0.000	0.000	1.000	1.000
Trade Tax Multiplier	3.979	0.786	0.000	3.970	5.200	3.543	3.519	4.233	4.132
Unemployment Rate	0.086	0.066	0.017	0.075	0.269	0.077	0.094	0.073	0.084
Airport	0.129	0.336	0.000	0.000	1.000	0.003	0.002	0.296	0.170
Highway Connection	0.613	0.487	0.000	1.000	1.000	0.466	0.375	0.804	0.691
Observations			5,192,864			15,311	1,298,302	73,928	3,805,323

Descriptive statistics are based on the RegFiD database, covering the period from 2013 to 2019 and limited to private single-establishment firms in manufacturing and services. Domestic business groups are excluded. High skill intensity, low workplace complexity, and strong employment growth are proxied by a dummy variable for the share of regular employees exceeding 0.75, the log number of marginal employees (plus one), and a dummy variable for the relative increase in the number of regular employees over the previous business year exceeding 0.1, respectively. Population density and the trade tax multiplier are reported in units of 100. Due to strict data protection rules, we report the values for the 1^{st} and 99^{th} percentiles instead of minima and maxima.

a substantial number of 3,184 NFs newly acquired by foreign multinational business groups over our observation period. This makes it easier to identify a productivity stimulus during the takeover process than in many other studies, where the number of newly acquired firms is typically small. However, a disadvantage of our dataset is the lack of information on capital stocks, workforce composition, and intermediate consumption. Consequently, we proxy firm productivity by log revenues per employee (see Girma et al., 2015; Fons-Rosen et al., 2021). To address this limitation, we focus on a subsample of manufacturing firms, for which Destatis provides additional information through the AFiD panel of industrial firms in Germany (see Destatis, 2023). For this subsample, we can estimate total factor productivity using standard procedures outlined in the literature (Olley and Pakes, 1996; Levinsohn and Petrin, 2003) and assess the robustness of our results using a more sophisticated measure of firm productivity, which aligns more closely with the unit cost perspective presented in our theoretical analysis.

There are three important lessons to learn from Table 4.1, namely *(i.)* MNFs are more productive on average than NFs; *(ii.)* NFs are more productive on average in urban than in rural areas; and *(iii.)* MNFs are more productive on average in rural than in urban areas. To make sure that these patterns are indeed due to firms differing in their status as multinational or national producers and do not erroneously pick up heterogeneity of firms in other dimensions, we employ proper econometric methods to further investigate these patterns in the next section.

4.4 Productivity differences between MNFs and NFs

In this section, we delve deeper into the productivity differences between MNFs and NFs observed in the descriptives reported in Table 4.1. In Subsection 4.4.1, we run OLS regressions with varying sets of explanatory variables to determine whether the productivity differences between MNFs and NFs persist when controlling for other firm heterogeneity. In Subsection 4.4.2, we take this analysis further by employing a two-stage estimator. First, we match firms acquired by foreign multinational business groups with similar firms that are not acquired, based on their propensity score of acquisition. Then, we estimate the average treatment effect of foreign takeover on firm productivity for the matched sample. This procedure allows us to separate the effect of productivity transfer from a foreign parent to its German affiliate during the acquisition process from pre-existing firm heterogeneity.

4.4.1 OLS estimation

To analyse the productivity differences between MNFs and NFs, on the one hand, and the productivity differences between firms from urban and rural areas, on the other hand, we estimate a model of the following form:

$$\log rev/emp_{jirt} = \alpha_0 + \alpha_1 MNF_{jt} + \alpha_2 Urban_r + \alpha_3 MNF_{jt} \times Urban_r + \alpha_4 \mathbf{X}_{jrt} + \nu_i + \mu_{s(r)} + \zeta_t + \varepsilon_{jirt}, \quad (4.10)$$

where $\log rev/emp_{jirt}$ is the log revenue per employee of firm j in industry i, labour market region r and year t, MNF_{jt} is a dummy variable taking a value of one if the firm belongs to a multinational business group in the respective observation year, $Urban_r$ is a dummy variable taking a value of one if a labour market region is classified as urban, and $MNF_{jt} \times Urban_r$ is an interaction term taking a value of one if the firm belongs to a multinational business group and at the same time is located in an urban region. Moreover, $\mathbf{X}_{jrt}$ is a column vector of explanatory variables at the firm and the region level (with α_4 as the corresponding row vector of coefficients). Firm-level controls include eight dummy variables for firm size, measured by the number of employees subject to social security payments,[13] a dummy variable for skill-intensive production equal to one if the share of regular employees exceeds 0.75, a proxy for low workplace complexity measured by the log number of marginal employees (plus one), and a dummy variable for strong employment growth of more than 10 percent over the previous business year. At the region level, we consider the trade tax multiplier at the county level and the unemployment rate at the municipality level to control for local economic conditions and, in addition, we include dummy variables for the existence of a local airport and the connection to highways at the municipality level to control for differences in the accessibility of firms. Moreover, we also control for industry fixed effects at the two-digit NACE Rev. 2 sector level, ν_i, for state fixed effects covering multiple labour market regions, $\mu_{s(r)}$, and for year fixed effects, ζ_t. Finally, ε_{jirt} is the error term.

The results from estimating Eq. (4.10) are reported in Table 4.2. In the first column, we present the findings for a baseline specification, controlling only for MNF status, location in an urban area, and their interaction term. For this parsimonious model, we find evidence of a productivity premium for MNFs, which is more pronounced in rural areas than in urban areas. We also identify a small urban productivity premium

[13] We distinguish firms with 5 to 9, 10 to 19, 20 to 49, 50 to 99, 100 to 199, 200 to 499, 500 to 999, or 1000 and more employees, while considering firms with 1 to 4 employees as a reference group.

for NFs. These results are robust to the inclusion of other firm and regional controls and remain qualitatively unchanged in our preferred specification in Column 4, where we control for industry, state, and year fixed effects, thereby eliminating much of the variation in our data across these dimensions.[14] For this model, we estimate a *marginal effect* for the productivity premium of MNFs amounting to 36.1 log points, which is within the range of parameter estimates reported in the literature using similar proxies for firm productivity (see Griffith, 1999; Girma et al., 2015). Additionally, we estimate a moderate urban productivity premium for NFs of 0.9 log points, consistent with previous research (see Rosenthal and Strange, 2004; Melo et al., 2009). Finally, we estimate a *marginal effect* for the rural productivity premium of MNFs, which, at 4.4 log points, is relatively large and, to the best of our knowledge, previously unobserved in the literature.[15]

In the remaining two columns, we present the results from two robustness checks. In the first one, we include firms belonging to domestic business groups (domestic group firms, DGFs), which were excluded from the previous analysis. We introduce a dummy variable, DGF_{jt}, to indicate these firms, as well as an interaction term, $DGF_{jt} \times Urban_{jt}$, to distinguish DGFs located in rural and urban areas. As shown in Column 5, adding these two dummy variables moderately reduces the estimated productivity premium of MNFs and the rural productivity premium observed for them. Our estimates also reveal a productivity premium for DGFs over NFs, which is lower than that of MNFs, and a sizable urban productivity premium for firms belonging to German groups. From this, we infer that a rural productivity premium exists in our data only for firms belonging to multinational business groups owned by foreign investors.

We finally consider the subsample of industrial firms covered by the AFiD panel. For this subsample, Destatis provides additional firm-level data, including information on capital stocks and intermediate consumption, which we use to estimate total factor productivity following the control function approach suggested by Levinsohn and Petrin (2003). We then use these productivity estimates, instead of log revenues per employee, as the dependent variable in our OLS estimation. The results for the pre-

[14] We have also estimated a more restrictive model that includes industry-year and state-year fixed effects. However, since the results of this alternative specification are similar to those reported in Column 4, we only show them in Appendix 4.C.

[15] While the marginal effects for the (rural) productivity premium of MNFs are not displayed in Table 4.2, they can be computed straightforwardly by combining the coefficient estimates in Column 4 with the frequency of MNFs and NFs in urban and rural areas from Table 4.1. Applying the delta method to compute the standard errors of these marginal effects confirms that the reported effects are statistically significant.

Table 4.2: *Productivity of MNFs and NFs in urban and rural areas*

	(1)	(2)	(3)	(4)	(5)	(6)
MNF	0.354***	0.589***	0.589***	0.400***	0.371***	0.074***
	(0.016)	(0.018)	(0.018)	(0.016)	(0.016)	(0.008)
Urban	0.002*	0.001	0.038***	0.008***	0.008***	−0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)
MNF×Urban	−0.048***	−0.100***	−0.089***	−0.051***	−0.042**	−0.019**
	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)	(0.008)
DGF					0.237***	
					(0.004)	
DGF×Urban					0.038***	
					(0.005)	
Firm Controls		Yes	Yes	Yes	Yes	Yes
Region Controls			Yes	Yes	Yes	Yes
Industry FE				Yes	Yes	Yes
State FE				Yes	Yes	Yes
Year FE				Yes	Yes	Yes
Observations	5,192,864	5,192,864	5,192,864	5,192,864	5,893,991	35,467
Adj R^2	0.002	0.035	0.040	0.199	0.211	0.378

Notes: OLS estimates based on the RegFiD and the AFiD databases covering the years 2013 to 2019. The data is limited to private single-establishment firms in manufacturing or services. The dependent variable is log revenues per employee. Firm controls include eight dummy variables for firm size categories, dummy variables for high skill intensity and strong employment growth as well as a proxy for low workplace complexity. Regional controls include the trade tax multiplier, the unemployment rate and dummy variables for the presence of local airports and highway connections. Columns 1 to 4 explain the log revenues per employee by MNF status, urban location and their interaction term as well as varying covariates and fixed effects. Column 5 includes firms that are members of domestic business groups (DGFs), whereas Column 6 considers the subsample of firms covered by the AFiD panel and uses estimated values of total factor productivity instead of log revenues per employee as dependent variable. Standard errors in parentheses are clustered at the firm level. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

ferred specification, which includes industry, state, and year fixed effects, are reported in Column 6. Focusing on the AFiD panel and excluding all firms lacking the required information on capital stocks or intermediate consumption results in a significant drop in the number of observations and substantially reduces the estimated effects. However, the main insights from our analysis remain mostly unchanged: we still observe a productivity premium of MNFs over NFs and a rural productivity premium for MNFs, while the urban productivity premium for NFs disappears.

In Appendix 4.B, we show robustness of our results reported in Column 4 of Table 4.2 in multiple further dimensions. In particular, our findings are qualitatively unchanged if we add multi-establishment firms, consider alternative definitions of urban areas, or exclude East Germany from our analysis. We also show that a rural productivity premium cannot be identified for the subsample of service firms, while the results from our preferred specification change only moderately if we control for attrition and thus the selective dropout of firms from our sample. Finally, we also discuss robustness of our results reported in Column 6 of Table 4.2, considering an alternative way of estimating total factor productivity.

4.4.2 A two-step treatment effects estimator

By controlling for explanatory variables at the firm and region levels, as well as for industry, state, and time fixed effects, we eliminate much of the observed and unobserved heterogeneity in our preferred OLS specification. However, concerns about endogeneity may persist if the observed productivity patterns reflect not only the impact of ownership but also other unobserved firm heterogeneity that our covariates do not fully capture. To address this and achieve causal inference, we narrow our focus to analyse the impact of the acquisition of German firms by foreign multinational business groups (foreign takeover, in short). For this purpose, we employ methods from the literature on programme evaluation. Specifically, we implement a two-stage estimator. In the first stage, we balance the distribution of covariates between treated and untreated firms using *nearest-neighbour* propensity score matching (Rosenbaum and Rubin, 1983; Imbens and Wooldridge, 2009; Imbens, 2015). In the second stage, we estimate the average treatment effect of foreign takeover for the matched sample of firms, following Abadie and Imbens (2016).

For our analysis, we define the treatment event as the acquisition of an NF by a multinational business group between two consecutive years t and $t+1$, leading to a status change for this firm from $MNF_{jt} = 0$ to $MNF_{jt+1} = 1$. Similarly, an untreated firm is an NF that does not change its status, implying $MNF_{jt} = MNF_{jt+1} = 0$. Based on these definitions, we can then capture the two possible treatment realisations by a dummy variable of the following form:

$$D_{jt} = \begin{cases} 1 & \text{if} \quad MNF_{jt} = 0 \quad \text{and} \quad MNF_{jt+1} = 1 \\ 0 & \text{if} \quad MNF_{it} = 0 \quad \text{and} \quad MNF_{jt+1} = 0 \end{cases} \tag{4.11}$$

To determine an appropriate control group for the treated firms in our dataset, we match treated and untreated firms based on their propensity scores for acquisition. For this purpose, we create a two-year window around the acquisition event for each firm in the treatment group and estimate the probability of takeover within this window based on observable characteristics.[16] Relying on a logit estimator, we can express the

[16] Treated firms are only included for the two years around the treatment effect and are eliminated

probability of treatment conditional on observables as

$$P\left(D_{jt} = 1 \middle| \mathbf{Z}_{j(ir)t}\right) = \frac{\exp\left(\beta \mathbf{Z}_{j(ir)t}\right)}{1 + \exp\left(\beta \mathbf{Z}_{j(ir)t}\right)}. \tag{4.12}$$

where $\mathbf{Z}_{j(ir)t}$ is a row vector of covariates in period t, including at the firm level dummy variables for eight size categories, a dummy variable for strong employment growth over the previous business year, a proxy for low workplace complexity as well as a dummy variable for high skill intensity, and including at the region level dummy variables for the presence of local airports and highway connections at the municipality level as well as two dummy variables taking a value of one if the unemployment rate or the trade tax multiplier are above the economy-wide median.[17] We also control for industry, state, and year fixed effects.

After the construction of a matched sample with balanced covariates of acquired and non-acquired firms, we can determine the average treatment effect, τ_{ATE}, of foreign takeover on firm productivity as the expected difference of the log revenues per employee under treatment (y_1) and non-treatment (y_0): $\tau_{ATE} = \mathbb{E}(y_1 - y_0)$ of a randomly drawn firm from the overall firm population.[18] Estimating the average treatment effect is challenging because it is impossible to observe the outcome under both treatment realisations simultaneously for a single firm. However, it is well established in the literature on programme evaluation that under the assumptions of *(i.)* unconfoundedness and *(ii.)* common support one can estimate the expected realisations of y_1 and y_0 conditional on the covariates used for matching, $\mathbf{Z}_j$. As pointed out by Rosenbaum and Rubin (1983), under these two assumptions propensity scores based on Eq. (4.12) can be used to eliminate *all* confounding factors.

From Eq. (4.12), we can infer that conditional on covariates $\mathbf{Z}_j$, the propensity score of firm j to receive treatment $D_j = 1$ is given by the conditional probability $P\left(D_j = 1 \,|\mathbf{Z}_j\right)$, whereas its the propensity score to receive treatment $D_j = 0$ equals $P\left(D_j = 0 \,|\mathbf{Z}_j\right) = 1 - P\left(D_j = 1 \,|\mathbf{Z}_j\right)$. To simplify notation, we denote by $\widehat{p}_j(D)$ the estimated propensity score of firm j to receive treatment $D \in \{0, 1\}$. Following Abadie and Imbens (2016), we can then express for the case of nearest-neighbour matching

from the sample in all other years.

[17] In the two-stage treatment effects estimation, we include all regional controls as dummy variables to enhance matching quality. We have re-estimated the empirical model outlined in Eq. (4.10) using the same set of controls. Since this leaves our OLS results largely unchanged, we do not report them here.

[18] Time indices are neglected to facilitate readability.

with replacement the estimator for the average treatment effect as follows:

$$\widehat{\tau}_{ATE} = \frac{1}{N}\sum_{j=1}^{N}(2D_j - 1)\left(y_j - \frac{1}{M_j}\sum_{\ell\in\mathcal{J}_M(j)} y_\ell\right), \tag{4.13}$$

where N is the total number of treated and untreated observations and

$$\mathcal{J}_M(j) \equiv \left\{\ell = 1, ..., N \middle| D_\ell = 1 - D_j, |\widehat{p}_j(D_j) - \widehat{p}_\ell(D_\ell)| \leq \min_{\substack{k\neq\ell,\\ D_k=1-D_j}} \{|\widehat{p}_j(D_j) - \widehat{p}_k(D_k)|\}\right\}$$

is the set of M_j matches for firm j on estimated propensity scores. Considering nearest-neighbour matching, we have $M_j \geq 1$, with $M_j > 1$ possible if there are ties among matching candidates in terms of their propensity scores. An important implication of Eq. (4.13) is that the average treatment effect estimator considers all treated and untreated observations (with differing weights) in the determination of $\widehat{\tau}_{ATE}$. This results from matching with replacement and leads to significantly higher observation numbers than an estimator for the average treatment effect on the treated, where the number of treated observations determines the sample size if the set of untreated firms is large.[19]

We use the two-stage treatment effects estimator for three different applications. First, we consider a pooled sample of all acquired firms and their matched non-acquired counterparts. In the other two applications, we estimate the average treatment effect for the subsamples of firms in urban and rural areas, respectively. The results for the logit model at stage one are reported in Table 4.B2 of Appendix 4.B. For the pooled sample, we show that firms are more likely to be treated if they are larger, experience stronger employment growth over the previous business year, and are located in municipalities with a local airport or highway connection. These results are largely confirmed when examining urban and rural areas separately. In Table 4.B3 of Appendix 4.B, we demonstrate that matching successfully improves the balancing of covariates between the treatment and control groups for the pooled sample of all firms. For instance, the mean bias between treated and untreated firms declines from an already low level of

[19] Abadie and Imbens (2016) also discuss the estimation of standard errors and the correction thereof to account for the fact that the propensity scores for matching are estimated. Since the suggested correction is time-consuming and available software packages that allow for it are limited in terms of admissible observation numbers, we do not implement this correction here. Readers interested in this topic are referred to Abadie and Imbens (2016) for further details. Without the proposed correction, reported standard errors and test statistics should be interpreted with caution. However, we are not overly concerned about this limitation, as various experiments on simpler problems suggest that uncorrected standard errors are typically overestimated.

3.5 percent before matching to 2.2 percent after matching.

The average treatment effects of foreign takeovers are presented in Table 4.3. Column 1 displays the treatment effect for the pooled sample of all firms. Acquisition by a foreign multinational business group results in a productivity increase of 8.1 log points compared to non-acquired firms in the control group. Although this increase is lower than the premium reported in our preferred OLS specification, it remains substantial, as it captures only the immediate effect of the takeover, triggered by the technology transfer from the foreign parent to the German affiliate during the acquisition process. Columns 2 and 3 indicate that the average treatment effect of acquisition by a foreign multinational business group is 4.8 log points lower in urban areas compared to rural areas, consistent with the rural productivity premium observed in Table 4.2. An unreported χ^2-test confirms that the difference in treatment effects between urban and rural areas is significant at the one percent level.

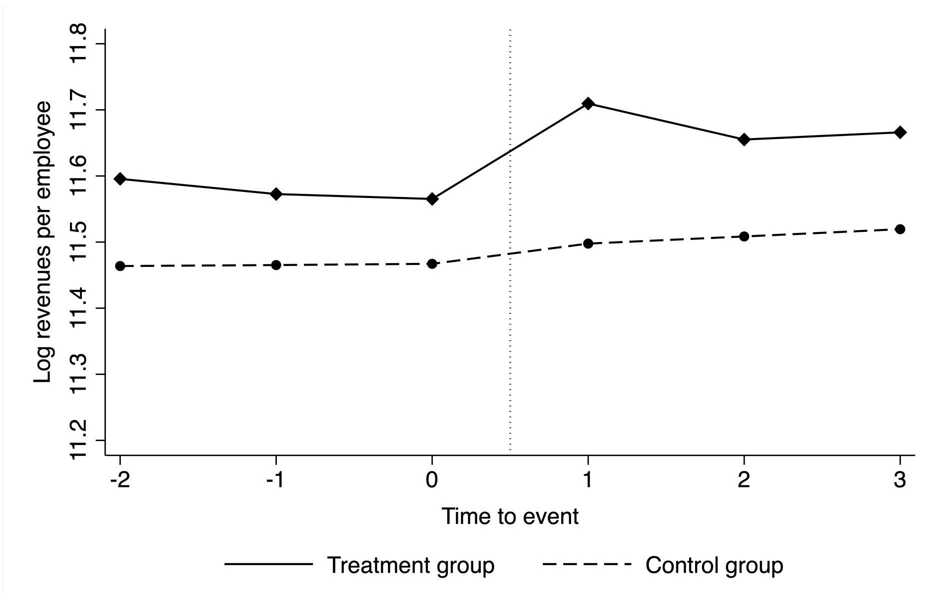
Table 4.3: *Average treatment effect of foreign and domestic takeover*

	Foreign takeover			*Domestic takeover*		
	All firms	Urban	Rural	All firms	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Dummy	0.081***	0.070**	0.118**	0.018**	0.009	−0.003
	(0.030)	(0.034)	(0.060)	(0.008)	(0.009)	(0.020)
Matched obs.	3,576,625	2,644,227	921,105	3,605,200	2,667,206	937,884

Notes: Results of the treatment effects estimation on a matched sample of firms from the RegFiD database. The dependent variable is log revenues per employee. The dataset is restricted to private single-establishment firms in manufacturing or services. Columns 1 to 3 exclude domestic business groups, while Columns 4 to 6 exclude foreign ones. Propensity scores for matching are explained by eight dummy variables for firm size categories, a dummy variable for high skill intensity, a proxy for low workplace complexity, a dummy variable for strong employment growth over the previous business year, and dummy variables for the presence of local airports, highway connections, an unemployment rate or a local trade tax multiplier above the economy-wide median, and an urban location. Industry, state, and year fixed effects are also included. Column 1 shows the treatment effect of foreign takeovers for the pooled sample of all firms. Columns 2 and 3 report the average treatment effects of foreign takeovers for urban and rural areas, respectively. Columns 4 to 6 display the corresponding treatment effects for domestic takeovers. Analytical standard errors computed from influence functions are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Beyond the two assumptions of *unconfoundedness* – which states that, after conditioning on observed covariates, there are no unobservable factors simultaneously influencing the treatment assignment and the outcome – and *common support* – which bounds the propensity score away from its two limits of zero and one – there are two additional assumptions necessary for claiming causal inference. The first is the assumption of a *stable unit treatment value*, which postulates that the treatment of firm j only affects firm j. The second is the assumption of a *common trend*, which postulates that the treatment assignment is independent of prior developments in the mean of the potential outcome under non-treatment.

Figure 4.3: *Common trend of treatment and control group*



Notes: This figure depicts the dependent variable for firms in the treatment and in the control group before and after treatment. To construct this figure firms' revenues per employee are also used for years outside the sample period.

Whereas the assumptions of unconfoundedness and a stable unit-treatment value are untestable, a combination of considering a large set of matching covariates and noting that each single-establishment firm is small relative to the whole economy makes us confident that these assumptions are justified in our case. Moreover, as pointed out by (Imbens and Wooldridge, 2009, p.26) "common parametric models, such as probit and logit, ensure that all estimated probabilities are strictly between zero and one". Therefore, the assumption of common support can be ensured by eliminating observations that are too close to the propensity limits of zero or one. Finally, while it is possible to test for common trends (as suggested by Autor, 2003), it is common practice to assess the common trend assumption graphically in the case of a binary treatment (see, e.g., Card and Krueger, 2000; Pischke, 2007). We follow this practice and report in Figure 4.3 the log revenues per employee in treatment and control group around the treatment event. Important for our analysis, the displayed evolution of the log revenues per employee does not suggest a positive pre-trend, i.e., an increase in the productivity of treated firms prior to their actual treatment.

To complete the discussion in this section, we also consider the takeover of German firms by domestic business groups as an alternative treatment event. This allows us to determine whether the important insight from Table 4.2 that the rural productivity premium is specific to firms in foreign multinational business groups still holds when considering the immediate productivity stimulus from acquisition. We follow the same procedure as outlined above and estimate the average treatment effect of takeover by a domestic business group based on a pre-matched sample of treated and untreated firms. The results of the second-stage average treatment effect estimator are reported in Columns 4 to 6 of Table 4.3. These results show that the productivity stimulus is less pronounced for domestic takeovers compared to foreign ones. Moreover, the two forms of takeover differ considerably in their effects on urban and rural areas. While foreign takeovers exert a stronger productivity stimulus in rural areas than in urban areas, this pattern is not observed for domestic takeovers.

4.5 A mechanism explaining the rural productivity premium for MNFs

Whereas Section 4.4 has shown clear evidence for a rural productivity premium among MNFs, it does not provide an explanation for its existence. To address this, we draw on insights from the literature on regional economics, which emphasises the heterogeneity in technology spillovers for firms with different productivity levels and the resulting heterogeneity in location choices (see Shaver and Flyer, 2000; Mariotti et al., 2010; Bloom et al., 2013). The core finding from this literature is that high-productivity firms gain less from incoming technology spillovers than they lose from outgoing technology spillovers, reducing their incentive to locate near lower-productivity competitors. Since MNFs are typically associated with high productivity levels, the rural productivity premium observed for them could therefore be the result of a strategic location choice to avoid regions with a high density of low-productivity NFs.

To empirically assess this specific channel, we include the number of competitors and its interaction with dummy variables for urban labour market regions and the MNF status of firms in our OLS regression outlined in Section 4.4.1. We consider only competitors active in the same labour market region, as technology spillovers are localised and diminish significantly over larger distances (Ellison et al., 2010; Baum-Snow et al., 2024). Additionally, we distinguish competitors from different industries to reflect the empirical finding that firms generally view vertical outgoing technology spillovers to their input suppliers from other industries more favourably than horizontal outgoing technology spillovers to their rivals in the same industry (Havranek and Irsova, 2011; Bloom et al., 2013). Table 4.4 presents the estimation results for the augmented OLS model. Column 1 repeats the findings from our preferred specification in Table

4.2 to facilitate comparison between different empirical models. Column 2 reports the regression results when including controls for the number of competitors from the same broad 1-digit sector, their interaction terms with the dummy variable for an urban labour market region, and the dummy variable for MNF status, as well as a triple interaction term among these covariates. Adding these controls provides a more nuanced understanding of the productivity differences of MNFs located in urban and rural areas. MNFs exhibit lower productivity in urban areas compared to rural areas only if they face stronger rivalry from nearby competitors in the same sector.

Table 4.4: *Productivity of MNFs and NFs in urban and rural areas*

		Intra-Industry Competitors		Inter-Industry Competitors
	(1)	(2)	(3)	(4)
MNF	0.400***	0.349***	0.283***	0.471***
	(0.016)	(0.033)	(0.032)	(0.040)
Urban	0.008***	0.009***	0.008***	0.009***
	(0.002)	(0.003)	(0.002)	(0.003)
MNF × Urban	−0.051***	0.102***	0.045	−0.009
	(0.022)	(0.036)	(0.034)	(0.042)
# Competitors		0.094***	4.796***	0.004***
		(0.004)	(0.113)	(0.000)
Urban × # Competitors		0.018***	−1.879***	0.001***
		(0.000)	(0.020)	(0.000)
MNF × # Competitors		0.247***	6.672***	−0.003
		(0.066)	(2.252)	(0.004)
MNF × Urban × # Competitors		−0.024***	−1.722***	−0.001***
		(0.004)	(0.169)	(0.000)
Firm & Regional Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5,192,864	5,192,864	5,192,864	5,192,864
Adj R^2	0 .199	0.200	0.475	0.200

Notes: OLS estimates are based on the RegFiD database covering the years 2013 to 2019. The data is limited to private single-establishment firms in manufacturing or services. The dependent variable is log revenues per employee. Firm and region controls are the same as those reported in Table 4.2. Column 1 reproduces the estimation results from Column 4 of that table. Column 2 controls for the number of competitors (in units of thousand) from the same 1-digit sector and labour market region, as well as the interaction of this variable with the MNF and urban dummy variables. Column 3 also controls for the number of competitors (and its interaction with MNF and urban dummy variables) but distinguishes competitors from three productivity groups: those with higher productivity, lower productivity, or similar productivity as the observational unit. Column 3 only displays the results for competitors with similar productivities. Finally, Column 4 considers a similar specification as Column 2 but includes competitors from other sectors instead of the same one. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

In Column 3, we consider recent evidence on the role of absorptive capacity in learning new technologies from nearby competitors (see Lychagin, 2016; Bisztray et al., 2018). This evidence supports the idea that firms cannot fully benefit from incoming technology spillovers if their pre-existing productivity backlog is too large. For MNFs, this implies that the potential harm from outgoing technology spillovers may be greatest in labour market regions with many competitors from the same sector with similar technology, as these firms have the absorptive capacity to effectively utilise the acquired knowledge from technology spillovers. To address this issue, we formulate a regression model similar to the one reported in Column 2. However, we now separately control for competitors from three distinct categories based on productivity levels: higher than, lower than, or similar to the respective firm. Similar productivity levels are confined to productivities within the same ventile. For readability, we only report the coefficients for the number of competitors with similar productivity levels in the table. Consistent with the notion that absorptive capacity plays a role, we find that MNFs have lower productivity in urban areas with a higher number of similarly productive competitors.[20]

Finally, in Column 4, we consider a model similar to that in Column 2, but now we control for the number of nearby competitors from other sectors. This specification is added to evaluate the common view that outgoing technology spillovers to firms from other sectors are potentially less harmful for MNFs. Our results do not strongly support this view. Specifically, the key insight from Column 2 that a rural productivity premium is largely explained by more local competition in urban areas also extends to competition with firms from other industries.[21]

While the OLS estimates in Table 4.4 confirm that the number of local competitors is an important factor explaining the rural productivity premium of MNFs, they do not directly address the channel through which the number of competitors influences firm productivity. There are two competing explanations for the identified effects. Firstly, a larger number of competitors can make the respective labour market region less attractive for a foreign technology leader. Secondly, given the location choice, there

[20] The results for competitors from other productivity categories are shown in Appendix 4.C. There are two notable insights regarding the estimated coefficients for these categories. First, the triple interaction term is positive for competitors with lower productivity and negative for competitors with higher productivity, consistent with the findings of Lychagin (2016) on absorptive capacity. Second, the (non-interacted) direct effect of the number of competitors is positive for the lower-productivity category and negative for the higher-productivity category. This is intuitive, as a larger number of competitors with comparably low productivity indicates that the observed firm is a high-productivity producer, and vice versa. Hence, adding the number of competitors from different productivity categories provides more information about the productivity of firms, leading to a higher adjusted R^2.

[21] The sizeable differences in the estimated coefficients for the competition effect reported in Columns 2 to 4 can be explained by strong differences in the number of competitors covered in the three specifications reported in Table 4.4. Therefore, we do not contrast the strength of the estimated effects between the three scenarios.

could be reduced technology transfer from the foreign parent to the German affiliate. We can assess these two alternative explanations using our two-step estimator outlined in Section 4.4.2. For instance, we can control for the number of local competitors in the matching of treated and untreated firms to determine whether a stronger productivity stimulus of foreign takeover in rural areas is still observed when eliminating strategic location choice. The results of estimating this modified regression model are reported in Columns 3 and 4 of Table 4.5.

Table 4.5: *Average treatment effects and the role of competitors*

	Baseline specification		*Matching on* # of competitors		*Interaction with* # of competitors	
	Urban	Rural	Urban	Rural	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Dummy	0.070**	0.118**	0.092**	0.036	0.066	0.112*
	(0.034)	(0.060)	(0.042)	(0.096)	(0.046)	(0.063)
# Competitors					0.001***	0.002***
					(0.000)	(0.001)
Treatment					0.002	0.012
× # Competitors					(0.010)	(0.106)
Matched obs.	2,644,227	921,105	2,644,227	921,105	2,644,227	921,105

Notes: Results of the treatment effects estimation on a matched sample of firms from the RegFiD database. The dependent variable is log revenues per employee. The dataset is restricted to private single-establishment firms in manufacturing or services. Domestic business groups are excluded. Columns 1 and 2 as well as Columns 5 and 6 consider the matching covariates outlined in Table 4.3. Columns 3 and 4 consider the number of competitors in the same industry and the same labour market region as additional matching covariate. Analytical standard errors computed from influence functions are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

We observe that, compared to the results from Table 4.3 (reproduced in Columns 1 and 2 for comparison), the stronger productivity stimulus of foreign takeovers in rural areas disappears once we condition the matching of treated and untreated firms on the local number of competitors from the same industry. Finally, in Columns 5 and 6, we use the same matching covariates as in Table 4.3 but allow for interaction between the treatment dummy and the number of competitors. Since the main finding of a stronger productivity stimulus from foreign takeovers in rural areas remains unchanged in this specification, we consider the second channel – postulating that stronger local competition lowers parent-affiliate technology transfers during the acquisition process – to be less relevant in our case.

4.6 Conclusion

This chapter analyses the productivity differences of German firms in manufacturing and services, using administrative data provided by the Federal Statistical Office of Germany. We thereby show three important observations that are characteristic for productivity patterns in Germany using OLS regressions. First, there is a considerable productivity premium of multinational over national firms. Second, there is a comparably small urban productivity premium for national firms. Third, there is a sizeable rural productivity premium for multinational firms. While the first two observations align with evidence reported by previous research for many countries, the finding of a rural productivity premium for multinational firms is novel in the literature. We demonstrate that this premium is very robust in our data, appearing across different measures of firm productivity and various sets of control variables. Additionally, we show that the rural productivity premium is specific to foreign multinational business groups, as a similar premium does not exist for domestic business groups. We also present an augmented Hotelling model with technology spillovers to provide a theoretical explanation for the observed productivity patterns.

To rule out the possibility that our findings are due to unobserved heterogeneity from omitted variables and to achieve causal inference regarding the role of ownership in the observed productivity differences, we analyse the average treatment effect of the acquisition of German firms by foreign multinational business groups (foreign takeover). Using a two-stage estimator, we determine the average treatment effect based on a pre-matched sample of treated and untreated German firms. This procedure reveals a sizeable productivity stimulus from foreign takeovers during the acquisition period, underscoring the significant role of technology transfer from the foreign parent to its German affiliate. Consistent with the OLS results, we also find that the productivity effect of foreign takeovers is stronger in rural areas than in urban areas, whereas a comparable effect is not observed for domestic takeovers by German investors.

Based on our insights from OLS and the two-stage treatment effects estimator, we search in the final step of our analysis for a possible explanation for the rural productivity premium of MNFs. Drawing on insights from regional economics, we focus on the role of competitors and find evidence that the rural productivity premium results from the strategic location choices of foreign technology leaders who aim to reduce the risk of technology dissipation. We also find some evidence that the absorptive capacity of local competitors plays a role in firms' location decisions. However, our results do not indicate a differential effect of competitors within and between industries, nor do they suggest that the number of competitors influences the strength of technology transfer given the location choice.

Using administrative data on German firms provides a rich dataset with a relatively high number of ownership changes over the observation period from 2013 to 2019. This feature of our data allows us to study the differential effects of foreign and domestic ownership in urban and rural areas. However, due to the lack of detailed information on workforce composition, we cannot address the equally important question of how and to what extent the observed productivity patterns affect employees with different skill levels. It is a natural next step to extend the analysis in this direction to learn more about the role of workers in *(i.)* takeover decisions and *(ii.)* the strategic location choices of firms in urban and rural areas. Given the limitations of our dataset regarding information on workers, we leave these fruitful extensions for future research.

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Appendix

4.A Theoretical Appendix

4.A.1 Proof of Proposition 1

We first rewrite the first-order conditions in Eq. (4.7) as follows:

$$\frac{d\pi_1^*}{dx_1^*} = \frac{d_1(x_1,x_2)g_1(x_1,x_2)}{3(x_2-x_1)(1+2x_1)^2(3-2x_2)}, \qquad \frac{d\pi_2^*}{dx_2^*} = \frac{d_2(x_1,x_2)g_2(x_1,x_2)}{3(x_2-x_1)(1+2x_1)(3-2x_2)},$$

with

$$\begin{aligned} g_1(x_1,x_2) \equiv{}& (x_2-x_1)(1+2x_1)\left[t(1+2x_1)(3-2x_2)(x_2-3x_1-2)+8\right] \\ &+4(x_2+x_1-1)(-4x_2+6x_1+1), \\ g_2(x_1,x_2) \equiv{}& (x_2-x_1)(3-2x_2)\left[t(1+2x_1)(3-2x_2)(4-3x_2+x_1)-8\right] \\ &+4(x_2+x_1-1)(4x_1-6x_2+3), \end{aligned}$$

respectively. Adding up $g_1(\cdot)$ and $g_2(\cdot)$, we compute

$$g_1(x_1,x_2)+g_2(x_1,x_2) = 2(x_2+x_1-1)\Bigg\{t(x_2-x_1)(1+2x_1)(3-2x_2)(3x_2-3x_1-5) \\ -4(3x_2-3x_1-2)\Bigg\}.$$

If both firms are at their profit maximum, the following conditions must hold: *(i.)* $g_1(x_1,x_2)=0$, *(ii.)* $g_2(x_1,x_2)=0$, and *(iii.)* $g_1(x_1,x_2)+g_2(x_1,x_2)=0$ must hold. It is easily confirmed that $g_1(x_1,x_2)+g_2(x_1,x_2)=0$ is fulfilled if $x_2+x_1-1=0$ and we focus on this solution with symmetric distances of firms to the city centre in our analysis.

For $x_1+x_2-1=0$, we can rewrite the first-order conditions for profit-maximising location choices as

$$g_1(x_1,1-x_1) = -g_2(x_1,1-x_1) = \left(1-4x_1^2\right)\left[-t(1+2x_1)^2(1+4x_1)+8\right] = 0. \quad (4.14)$$

We find that $f_0(x_1) \equiv -t(1+2x_1)^2(1+4x_1)+8$ has a unique solution $x_1^* > -1/4$. To guarantee that the solution is a maximum, we have to check the second-order conditions. Differentiating $g_j(x_1,x_2)$, $j=1,2$, with respect to x_j and evaluating the resulting derivative at $x_1 = x_1^*, x_2 = 1 - x_1^*$, we compute $\partial g_1(x_1^*, 1 - x_1^*)/\partial x_1 = \partial g_2(x_1^*, 1 - x_1^*)/\partial x_2 = -t(1 + 2x_1^*)^2[13 + 10x_1^* - 96(x_1^*)^2]/2$. For $t > 1$, we find that $x_1^* < 0.4$ and this is sufficient for $13 + 10x_1^* - 96(x_1^*)^2 > 0$ and thus for $x1 = x_1^*$, $x_2 = 1 - x_1^*$ to be a maximum. Finally, we can conclude that $f_0(0) >, =, < 0$ if $8 >, =, < t$. This implies that the two firms locate outside (inside) the city borders if $t > (<)8$, completing the proof.

4.A.2 Derivation of Eq. (4.8)

To derive Eq. (4.8), we first note that by using $x_1 = x_1^*$, $x_2 = 1 - x_1^*$, we compute

$$d_2 = \begin{cases} \frac{1}{2}\left(1 - \frac{c_2 - c_1}{3t(1-2x_1^*)}\right) & \text{if } \frac{c_2 - c_1}{3t(1-2x_1^*)} < 1 \\ 0 & \text{otherwise} \end{cases}, \tag{4.15}$$

according to Eqs. (4.3) and (4.4). To proceed, we can use $t(1+2x_1^*)^2(1+4x_1^*) = 8$ from Proposition 1 to substitute for t. Acknowledging $c_1 = 0$, $c_2 = 2a_c(3 - 2x_1^*)/(1 + 2x_1^*)^2$, it follows that $d_2 > 0$ if and only if $a_c(3 - 2x_1^*)(1 + 4x_1^*)/[12(1 - 2x_1^*)] < 1$. In this case, we compute the profit of firm 1 after acquisition as:

$$\begin{aligned}
\pi_1^* &= \frac{t(1-2x_1^*)}{2}\left[1 + 2a_c\frac{3-2x_1^*}{3t(1-2x_1^*)(1+2x_1^*)^2}\right]^2 \\
&= \frac{t(1-2x_1^*)}{2} + \frac{2a_c}{3}\frac{(3-2x_1^*)}{(1+2x_1^*)^2}\left[1 + \frac{a_c(3-2x_1^*)}{3t(1-2x_1^*)(1+2x_1^*)^2}\right] \\
&= \frac{t(1-2x_1^*)}{2} + \frac{a_c}{36}\frac{(3-2x_1^*)\left[24(1-2x_1^*) + a_c(3-2x_1^*)(1+4x_1^*)\right]}{(1+2x_1^*)^2(1-2x_1^*)},
\end{aligned}$$

where Eq. (4.5) and $t = 8/[(1+2x_1^*)^2(1+4x_1^*)]$ have been used. Subtracting $t(1-2x_1^*)/2$ for the price of acquisition gives the first line in Eq. (4.8). We can further note that $a_c(3 - 2x_1^*)(1 + 4x_1^*)/[12(1 - 2x_1^*)] \geq 1$ implies $d_2 = 0$ and thus $p_1 = c_2 - t(1 - 2x_1) = 2a_c(3-2x_1^*)/(1+2x_1^*)-8(1-2x_1^*)/[(1+2x_1^*)^2(1+4x_1^*)]$, according to Eqs. (4.3) and (4.4). Noting that $\pi_1^* = p_1$ and subtracting $t(1 - 2x_1^*)/2 = 4(1 - 2x_1^*)/[(1 + 2x_1^*)^2(1 + 4x_1^*)]$ establishes the second line in Eq. (4.8).

4.A.3 Proof of Proposition 2

To simplify notation, we introduce two auxiliary functions:

$$h(x_1^*, a_c) \equiv \frac{(3-2x_1^*)\left[24(1-2x_1^*) + a_c(1+4x_1^*)(3-2x_1^*)\right]}{(1+2x_1^*)^2(1-2x_1^*)},$$
$$b(x_1^*, a_c) \equiv \frac{a_c(3-2x_1^*)(1+4x_1^*) - 6(1-2x_1^*)}{(1+2x_1^*)^2(1+4x_1^*)},$$

such that $\Delta\pi_1^* = (a_c/36)h(x_1^*, a_c)$ if $a_c(3-2x_1^*)(1+4x_1^*)/[12(1-2x_1^*)] < 1$ and $\Delta\pi_1^* = 2b(x_1^*, a_c)$ if $a_c(3-2x_1^*)(1+4x_1^*)/[12(1-2x_1^*)] \geq 1$. Moreover, we can note that $a_c(3-2x_1^*)(1+4x_1^*)/[12(1-2x_1^*)] = 1$ establishes a negative relationship between x_1^* and a_c, which we denote by $x_1^0 = \gamma(a_c)$, with $\gamma'(a_c) < 0$. Using $a_c^0 = \gamma^{-1}(x_1^0)$, we can note that the condition $x_1^* < 0.4$ imposes a lower threshold for a_c^0, which we denote by $\underline{a}_c^0 = 60/143 \approx 0.41958$. Therefore, $a_c \leq \underline{a}_c^0$, implies that $a_c(3-2x_1^*)(1+4x_1^*)/[12(1-2x_1^*)] < 1$ holds for all possible $x_1^* < 0.4$. Moreover, there exists a lower threshold of x_1^0 which is imposed by $a_c \leq 1$, given by $\underline{x}_1^0 = \left(17 - \sqrt{217}\right)/8 \approx 0.283635$. This implies that $a_c(3-2x_1^*)(1+4x_1^*)/[12(1-2x_1^*)] < 1$ holds for all $x_1^* < \underline{x}_1^0$, irrespective of a_c.

Next, we determine the properties of $h(x_1^*, a_c)$. Differentiation with respect to x_1^* gives

$$h'_{x_1^*}(x_1^*, a_c) = -\frac{2\left\{[24 - a_c(5-8x_1^*)](3-2x_1^*)^2 + 4\left[1 - 5x_1^* + 2(x_1^*)^2\right](1+2x_1^*)h(x_1^*, a_c)\right\}}{(1+2x_1^*)^2(1-2x_1^*)(3-2x_1^*)},$$

$$\begin{aligned} h''_{x_1^*x_1^*}(x_1^*, a_c)\Big|_{h'_{x_1^*}(x_1^*, a_c)=0} &= -\frac{8(3-2x_1^*)\left[a_c(11-12x_1^*) - 24\right]}{(1+2x_1^*)^2(1-2x_1^*)(3-2x_1^*)} \\ &\quad + \frac{8h(x_1^*)\left\{(5-4x_1^*)(1+2x_1^*) - 2\left[1 - 5x_1^* + 2(x_1^*)^2\right]\right\}}{(1+2x_1^*)^2(1-2x_1^*)(3-2x_1^*)}. \end{aligned}$$

For $1 - 5x_1^* + 2(x_1^*)^2 \geq 0$ (and thus for x_1^* sufficiently small), $h(x_1^*, a_c)$ is monotonically decreasing in x_1^*, which establishes $h(x_1^*, a_c) > h(0, a_c)$ for all $x_1^* < 0$. For high levels of x_1^*, we have $1 - 5x_1^* + 2(x_1^*)^2 < 0$ and in this case, we cannot rule out that $h'(x_1^*) > 0$ holds for high levels of x_1^*, implying that $h(x_1^*, a_c)$ has an extremum in the interval $(0, 0.4)$. However, if such an extremum exists, it must be a minimum, because of $h''_{x_1^*x_1^*}(x_1^*, a_c)\Big|_{h'_{x_1^*}(x_1^*, a_c)=0} > 0$ is imposed by $1 - 5x_1^* + 2(x_1^*)^2 < 0$.

We now turn to the properties of $b(x_1^*, a_c)$. First, we know from above that $\Delta\pi_1^* = 2b(x_1^*, a_c)$ requires $a_c \geq a_c^0$, which establishes $b(x_1^*, a_c) > 0$. Differentiating $b(x_1^*, a_c)$

with respect to x_1^* gives

$$b'_{x_1^*}(x_1^*, a_c) = \frac{2\left[a_c(5-8x_1^*)+6-4(1+3x_1^*)(1+2x_1^*)b(x_1^*,a_c)\right]}{(1+2x_1^*)^2(1+4x_1^*)},$$

$b''_{x_1^* x_1^*}(x_1^*, a_c)\Big|_{b'_{x_1^*}(x_1^*,a_c)=0} < 0$ if $b(x_1^*, a_c) > 0$. Acknowledging $a_c \geq a_c^0$, we can conclude that if $b(x_1^*, a_c)$ has an extremum, it must be a maximum. We denote by x_1^1 the value of x_1^* that maximises $b(x_1^*, a_c)$, which is implicitly determined by $a_c = 6[5 + 10x_1^* - 16(x_1^*)^2]/[(7-2x_1^*)(1+4x_1^*)^2]$ and is decreasing in a_c. We can write $x_1^1 = \delta(a_c)$, with $\delta'(a_c) < 0$. Using $a_c^1 = \delta^{-1}(x_1^1)$, it follows that $x_1^1 < 0.4$ imposes a lower threshold on a_c, which we denote by $\underline{a}_c^1 = 4830/5239 \approx 0.91932$. Therefore, $b(x_1^*, a_c)$ is monotonically increasing in $x_1^* \geq x_1^0 = \gamma(a_c)$ if $a_c \in (\underline{a}_c^0, \underline{a}_c^1)$.

Moreover, noting that $a_c > \underline{a}_c^1$ implies $x_1^0 < \gamma(a_c^1) = (14503 - \sqrt{158411289})/6440 \approx 0.297647$ and $x_1^1 > \delta(1) \approx 0.374498$, it follows that $x_1^1 > x_1^0$. Hence, $b(x_1^*, a_c)$ has a unique maximum at $x_1^1 \in (x_0^1, 0.4)$ if $a_c > \underline{a}_c^1$.Substituting $x_1^* = x_1^1$ and $a_c^1 = \delta^{-1}(x_1^1)$ into $b(x_1^*, a_c)$ gives

$$b\left(x_1^1, \delta^{-1}(x_1^1)\right) = \frac{48[1 - x_1^1 + (x_1^1)^2]}{(7-2x_1^1)(1+2x_1^1)(1+4x_1^1)^2},$$

$$b'\left(x_1^1, \delta^{-1}(x_1^1)\right) = -\frac{48(3-2x_1^1)[25 + 48x_1^1 - 24(x_1^1)^2 + 16(x_1^1)^3]}{(7-2x_1^1)^2(1+2x_1^1)^2(1+4x_1^1)^3}$$

and thus $b'\left(x_1^1, \delta^{-1}(x_1^1)\right) < 0$ for $x_1^1 \in (\delta(1), 0.4)$. Thus, $b\left(x_1^1, \delta^{-1}(x_1^1)\right) \leq b\left(\delta(1), 1\right)$.

Having charaterised them, we can now analyse the three segments of a_c in further detail. Let us first consider $a_c < \underline{a}_c^0 = 60/143$. Then, $\Delta\pi_1^* = (a_c/36)h(x_1^*, a_c)$ holds for all possible $x_1^* < 0.4$. In this case, $h(0.4, a_c) = (1320 + 1573a_c)/81 < (5832 + 729a_c)/81 = h(0, a_c)$ is sufficient for $\Delta\pi_1^* < (a_c/36)h(0, a_c)$ to hold for all $x_1^* > 0$. Second, $a_c \in (\underline{a}_c^0, \underline{a}_c^1)$ implies $b(0.4, a_c) > b(x_1^*, a_c) > b(x_1^0, a_c)$ for all $x_1^* \in (x_1^0, 0.4)$. Noting $2b(x_1^0, a_c) = (a_c/36)h(x_1^0, a_c)$, it follows that $(a_c/36)h(0, a_c) = 2a_c + a_c^2/4 > 10(143a_c - 30)/1053 = 2b(0.4, a_c)$ is sufficient for $\Delta\pi_1^* < (a_c/36)h(0, a_c)$ to hold for all $x_1^* > 0$. Third, for $a_c \in (\underline{a}_c^1, 1)$, we have $2b(x_1^*, a_c) < 2b\left(\delta(1), 1\right) \approx 0.538776$, which is smaller than $(a_c/36)h(0, a_c) = 2a_c + a_c^2/4$ if $a_c > \underline{a}_c^1 = 4830/5239$. This is sufficient for $\Delta\pi_1^* < (a_c/36)h(0, a_c)$ to hold for all $x_1^* > 0$. Finally, noting that $h'_{x_1^*}(x_1^*, a_c) < 0$ if $x_1^* < 0$, we can safely conclude that $\Delta\pi_1^* > (a_c/36)h(0, a_c)$ holds for all $x_1^* < 0$. This completes the proof.

4.B Data Appendix

4.B.1 Robustness of OLS estimates

In this Appendix, we discuss various robustness checks regarding the OLS estimations reported in Column 4 of Table 4.2, which we reproduce in the first column of Table 4.B1 to facilitate comparison across models. In Columns 2 and 3, we expand our sample in two different dimensions. In Column 2, we include firms from the agriculture, retail, hospitality, and housing sectors, while in Column 3, we add multi-establishment firms.[22] None of these modifications change the results of our analysis. In Columns 4 and 5, we restrict our sample to firms in the service sector and to labour market regions in West Germany, respectively. From these robustness checks, we conclude that a rural productivity premium for MNFs cannot be observed in the service sector, while focusing on West Germany has only minor effects on our results. Column 6 shows that excluding multinational business groups from non-OECD countries does not change our results.

In Column 7, we address the potential bias of our results from attrition due to the selective dropout of firms from our sample. Following Wooldridge (2002a,b), we proceed in two steps. First, we estimate the survival probabilities for firms existing in 2013 to remain in our dataset for subsequent years. We estimate these probabilities separately for each observation year using the same set of controls as in our preferred OLS specification reported in Column 4 of Table 4.2, excluding the year fixed effects. We then use the inverse of these computed probabilities as weights in our OLS regression to control for selection on observables. The results in Column 7 suggest that attrition bias is not a major problem in our case.[23]

In Columns 8 to 10, we examine the robustness of our results by considering alternative methods for distinguishing urban from rural areas. In Column 8, we focus on labour market regions in the upper and lower quartiles of the population density distribution. While this approach eliminates one-third of the firm-year observations in our data and creates a more pronounced distinction between urban and rural areas,

[22]For multi-establishment firms, we face the issue that their establishments may be located in different municipalities, counties, and labour market regions, and therefore be subject to various regional factors. To aggregate these factors, we compute their arithmetic mean over establishments and assign them to the firm. For binary indicators, we set a value of one if the computed average is above one-half and zero otherwise.

[23]As outlined in Wooldridge (2002a), there are two possible methods to construct the inverse probability weights. One method estimates survival probabilities for 2014 to 2019 based on covariates in 2013. The other method estimates survival probabilities for 2014 to 2019 based on covariates from the previous year and then multiplies the estimated probabilities. Both procedures have advantages and disadvantages, but they lead to similar results in our case, so we only report the findings from the second method in Table 4.B1.

Table 4.B1: *Productivity of MNFs and NFs in Urban and Rural Areas – Robustness*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
MNF	0.400***	0.432***	0.476***	0.240***	0.397***	0.415***	0.397***	0.426***	0.398***	0.459***	0.141***
	(0.016)	(0.019)	(0.015)	(0.029)	(0.019)	(0.021)	(0.021)	(0.027)	(0.010)	(0.011)	(0.009)
Urban	0.008***	0.011***	0.009***	0.007***	0.020***	0.008***	0.012***	0.028***			−0.007
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)			(0.005)
MNF×Urban	−0.051***	−0.052**	−0.051***	0.057*	−0.053**	−0.052**	−0.060**	−0.090***			−0.020**
	(0.018)	(0.021)	(0.016)	(0.031)	(0.021)	(0.023)	(0.023)	(0.029)			(0.010)
Density									0.008***	−0.005***	
									(0.002)	(0.001)	
MNF×Density									−0.052***	−0.063***	
									(0.008)	(0.006)	
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,192,864	6,335,248	5,316,612	4,368,296	4,381,535	5,184,911	4,082,845	3,435,691	5,192,864	5,192,864	35,356
Adj R^2	0.199	0.181	0.208	0.201	0.194	0.199	0.215	0.189	0.199	0.199	0.716

Notes: OLS estimates based on the RegFiD and the AFiD databases covering the years 2013 to 2019. The data is limited to private single-establishment firms in manufacturing or services. The dependent variable is log revenues per employee. Firm and region controls are the same as those reported in Table 4.2. Column 1 reproduces the estimation results from Column 4 of that table. Column 2 includes firms from the agriculture, hospitality, retail and housing sectors, while Column 3 also considers multi-establishment firms. Columns 4 and 5 restrict the sample to firms in the service sector and labour market regions in West Germany, respectively. Column 6 excludes multinational business groups with headquarters in non-OECD countries. In Column 7, we estimate the survival probabilities of firms and use the inverse of these probabilities as weights to correct for potential attrition bias. Column 8 considers only labour market regions in the top and bottom quartiles of population densities. In Columns 9 and 10, we replace the binary indicator for urban and rural areas with population densities for labour market regions and municipalities, respectively. Finally, in Column 11, we estimate total firm productivity based on log value added instead of log revenues for the AFiD panel and use these estimates as the dependent variable. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

the effects on our parameter estimates are moderate. In Columns 9 and 10, we replace the binary urban indicator with population densities for labour market regions and municipalities, respectively. Although this modification has naturally strong quantitative effects on the parameter estimates, it does not qualitatively alter the main insights from our analysis.[24] Finally, in Column 11, we estimate total factor productivity for the AFiD sample using log value added instead of log revenues as the firm-level outcome. We find that this does not qualitatively change the main results reported in Column 6 of Table 4.2.

4.B.2 First-stage results and balancing test for the treatment effects estimator

Table 4.B2 presents the results for the first-stage logit model of the treatment effects estimator introduced in Section 4.4. The estimated probabilities from the logit model were used to match treated and untreated firms, determining the average treatment effects reported in Table 4.3. As mentioned in the main text, we find that, in the pooled sample, larger firms with strong employment growth located in easily accessible municipalities are more likely, ceteris paribus, to be acquired by foreign multinational business groups. This finding is largely confirmed when examining urban and rural areas separately.[25]

Table 4.B3 presents balancing tests for the pooled sample of all firms. These diagnostics assess whether the matching process has been successful. The table shows variable means before and after matching, as well as the standardised differences between these means (see Rosenbaum and Rubin, 1985). Due to space constraints, we do not report balancing tests for industry, state, and year dummy variables. However, these are included in the computation of the mean and median bias reported at the bottom of the table.[26] Matching further reduces the already low mean bias from 3.5 percent to 2.2 percent. The low mean bias suggests that the covariates are balanced (see Caliendo and Kopeinig, 2008).

[24] In Appendix 4.C we graphically illustrate how the three alternative definitions of urban and rural areas affect their allocation in Germany.

[25] For the subsample of firms in rural areas, we do not report standard errors for the firm size category of 500 to 999 employees, as Destatis has censored them due to low observation numbers.

[26] Due to small observation numbers, Destatis does not allow reporting means (or standardised differences for the largest two firm size categories. However, these differences are included in the computation of mean and median bias.

Table 4.B2: *Results of nearest neighbour matching*

	All firms		Urban areas		Rural areas	
5 to 9 Employees	0.543***	(0.053)	0.609***	(0.057)	0.122	(0.148)
10 to 19 Employees	1.041***	(0.060)	1.067***	(0.065)	0.898***	(0.155)
20 to 49 Employees	1.859***	(0.064)	1.837***	(0.070)	1.968***	(0.158)
50 to 99 Employees	2.598***	(0.091)	2.589***	(0.100)	2.665***	(0.223)
100 to 199 Employees	3.020***	(0.125)	2.804***	(0.147)	3.737***	(0.251)
200 to 499 Employees	3.377***	(0.216)	3.288***	(0.247)	3.646***	(0.449)
500 to 999 Employees	4.669***	(0.483)	4.979***	(0.454)	0.000	
≥1000 Employees	5.930***	(0.763)	5.151***	(1.043)	9.287***	(2.191)
High Skill Intensity	−0.007	(0.067)	0.013	(0.073)	−0.142	(0.164)
Low Workplace Complexity	−0.521***	(0.038)	−0.501***	(0.042)	−0.647***	(0.094)
Strong Employment Growth	0.129***	(0.040)	0.122***	(0.044)	0.151	(0.104)
Trade Tax Multiplier	0.153***	(0.055)	0.186***	(0.064)	−0.054	(0.122)
Unemployment Rate	−0.011	(0.055)	−0.016	(0.064)	−0.089	(0.120)
Highway Connection	0.308***	(0.049)	0.323***	(0.059)	0.250***	(0.095)
Airport	0.592***	(0.063)	0.587***	(0.064)	0.259	(1.012)
Urban Area	0.171***	(0.063)				
Industry FE	Yes		Yes		Yes	
Region FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
Observations	3,576,625		2,644,227		921,144	

Notes: Logit estimations based on the RegFiD database. The data set is limited to private single-establishment firms in manufacturing or services. Domestic business groups are excluded. The dependent variable is a binary indicator for changing from NF status to MNF status. This binary indicator is regressed on eight dummy variables for firm size categories, a dummy variable for high skill intensity, a proxy for low workplace complexity, a dummy variable for strong employment growth over the previous business year, and dummy variables for the presence of local airports, highway connections, an unemployment rate or local trade tax multiplier above the economy-wide median, and location in an urban area. Industry, state, and year fixed effects are also included. Column 1 shows the estimation results for a pooled sample of all firms, while Columns 2 and 3 present results for urban and rural areas separately. Robust standard errors are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table 4.B3: *Balancing test for matching procedure*

	Before matching			After matching		
	Treated	Untreated	Std. diff.	Treated	Untreated	Std. diff.
5 to 9 Employees	0.186	0.199	−0.033	0.177	0.075	0.259
10 to 19 Employees	0.151	0.100	0.152	0.103	0.028	0.227
20 to 49 Employees	0.149	0.043	0.367	0.053	0.011	0.146
50 to 99 Employees	0.058	0.008	0.283	0.010	0.002	0.049
100 to 199 Employees	0.027	0.002	0.205	0.003	0.001	0.022
200 to 499 Employees	0.008	0.001	0.111	0.001	0.000	0.015
500 to 999 Employees	0.002	0.000	0.053			
≥1000 Employees	0.001	0.000	0.034			
High Skill Intensity	0.861	0.707	0.380	0.668	0.910	−0.599
Low Workplace Complexity	0.455	0.520	−0.095	0.651	0.097	0.814
Strong Employment Growth	0.302	0.236	0.149	0.236	0.082	0.349
Trade Tax Multiplier	0.707	0.592	0.242	0.628	0.751	−0.260
Unemployment Rate	0.259	0.268	−0.018	0.305	0.343	−0.087
Highway Connection	0.774	0.622	0.337	0.603	0.801	−0.437
Airport	0.274	0.122	0.389	0.113	0.240	−0.325
Urban	0.840	0.740	0.247	0.762	0.929	−0.412

Sample	**Mean bias in %**	**Median bias in %**
Unmatched	3.466	2.698
Matched	2.226	2.195

Notes: Balancing test for propensity score matching based on the RegFiD database. The data set is limited to private single-establishment firms in manufacturing or services. Domestic multinational business groups are excluded. All variables are measured in the pre-treatment period t. Balancing properties of industry, state, and year dummy variables are computed but not displayed.

4.C Appendix II

Table 4.C1 documents how the respective variables change, when specific firms are excluded from the sample. The first two columns display means and standard deviations of the respective variables for the full sample of firms. Columns 3 and 4 focus on firms in manufacturing and services and exclude firms whose main activity is either in agriculture, hospitality, housing or retail. By excluding those firms the number of firm-year observations decreases by about a third, the average firm becomes larger, but makes less profits. Columns 5 and 6 further limit the sample by excluding multi-plant firms. The number of observations decreases only slightly, but the workforce of the average firm becomes significantly smaller. Finally columns 7 and 8 show the preferred sample by excluding domestic firm groups.

Table 4.C2 displays further robustness checks. Column 1 shows the preferred reduced form specification as a point of comparison. As an extension the next column estimates a more restrictive model with industry-year and state-year fixed effects and documents that these fixed effects do not substantially alter the results. Column 3 takes up the results of Table 4.4 and displays the coefficients for all types of competitors. Columns 4 and 5 limit the baseline regression to firms that are active in manufacturing or located in East-Germany.

Figure 4.C1 illustrates three alternative methods for distinguishing urban and rural areas. Panel A shows urban and rural areas using a binary distinction similar to the main text, but only includes labour market regions in the top and bottom quartiles of the population density distribution. Panel B presents a scenario where population densities are considered instead of a binary urban indicator. For better readability of the figure, eight categories are distinguished, with darker areas representing higher population densities. Finally, Panel C adopts a similar approach to Panel B, but computes population densities at the municipality level.

Table 4.C1: *Further descriptive statistics*

	Full data set		Spec. sectors		Standalone firms		Preferred sample	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
				Firm-level variables				
Log Revenues	13.011	(1.520)	12.962	(1.512)	12.885	(1.422)	12.708	(1.275)
Log No. of Employees	1.377	(1.305)	1.463	(1.354)	1.378	(1.251)	1.225	(1.119)
Log Revenues/Employee	11.634	(0.968)	11.499	(0.910)	11.507	(0.883)	11.483	(0.836)
High Skill Intensity	0.831	(0.226)	0.857	(0.207)	0.856	(0.208)	0.850	(0.213)
Low Workplace Complexity	0.631	(0.776)	0.573	(0.753)	0.547	(0.717)	0.518	(0.678)
Strong Employment Growth	0.227	(0.419)	0.231	(0.422)	0.231	(0.421)	0.228	(0.420)
Multinational Firm	0.018	(0.134)	0.019	(0.135)	0.015	(0.122)	0.017	(0.130)
				Regional variables				
Population Density	0.609	(0.832)	0.619	(0.837)	0.616	(0.834)	0.613	(0.831)
Urban Area	0.742	(0.438)	0.750	(0.433)	0.750	(0.433)	0.747	(0.435)
Trade Tax Multiplier	3.979	(0.786)	3.988	(0.785)	3.986	(0.785)	3.979	(0.786)
Unemployment Rate	0.085	(0.064)	0.085	(0.064)	0.085	(0.064)	0.086	(0.066)
Airport	0.132	(0.340)	0.135	(0.342)	0.133	(0.340)	0.129	(0.336)
Highway Connection	0.617	(0.486)	0.622	(0.485)	0.619	(0.486)	0.613	(0.487)
Observations	9,444,571		6,106,721		5,893,991		5,192,864	

Descriptive statistics are based on the RegFiD database, covering the period from 2013 to 2019. The first two columns show the mean and standard deviations for the full data set. Columns 3 and 4 limit the data set to firms in manufacturing and services. Column 5 and 6 further limit the sample to single-establishment firms and columns 7 and 8 exclude publicly owned firms as well as domestic business groups. High skill intensity, low workplace complexity, and strong employment growth are proxied by a dummy variable for the share of regular employees exceeding 0.75, the log number of marginal employees (plus one), and a dummy variable for the relative increase in the number of regular employees over the previous business year exceeding 0.1, respectively. Population density and the trade tax multiplier are reported in units of 100. Due to strict data protection rules, we report the values for the 1^{st} and 99^{th} percentiles instead of minima and maxima.

Table 4.C2: *Productivity of MNFs and NFs in urban areas – Robustness II*

	(1)	(2)	(3)	(4)	(5)
MNF	0.400***	0.400***	0.283***	0.363***	0.435***
	(0.016)	(0.016)	(0.032)	(0.026)	(0.034)
Urban	0.008***	0.011***	0.008***	0.010**	−0.016***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
MNF×Urban	−0.051***	−0.051***	0.045	−0.002	−0.125***
	(0.018)	(0.018)	(0.034)	(0.029)	(0.039)
# Competitors with equal productivity			4.796***		
			(0.113)		
Urban × # Competitors with equal productivity			−1.879***		
			(0.020)		
MNF × # Competitors with equal productivity			6.672***		
			(2.252)		
MNF × Urban × # Competitors with equal productivity			−1.722***		
			(0.169)		
# Competitors with higher productivity			−1.636***		
			(0.009)		
Urban × # Competitors with higher productivity			−0.135***		
			(0.001)		
MNF × # Competitors with higher productivity			−3.437***		
			(0.227)		
MNF × Urban × # Competitors with higher productivity			−0.258***		
			(0.009)		
# Competitors with lower productivity			1.362***		
			(0.008)		
Urban × # Competitors with lower productivity			0.389***		
			(0.002)		
MNF × # Competitors with lower productivity			2.180***		
			(0.130)		
MNF × Urban × # Competitors with lower productivity			0.433***		
			(0.013)		
Firm & Regional Controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes		Yes	Yes	Yes
State FE	Yes		Yes	Yes	Yes
Year FE	Yes		Yes	Yes	Yes
Industry-Year FE		Yes			
State-Year FE		Yes			
Observations	5,192,864	5,192,853	5,192,864	819,488	1,035,406
Adj R^2	0.199	0.199	0.475	0.137	0.196

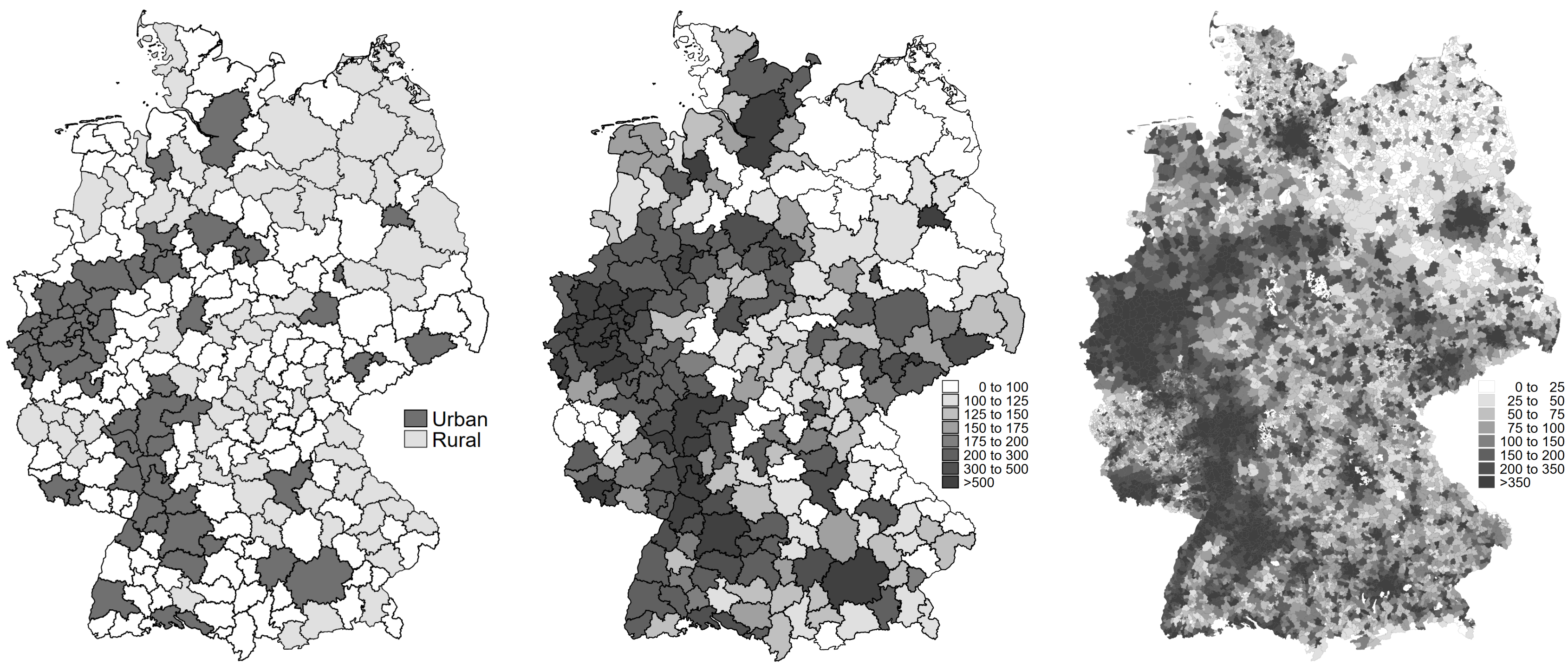
Notes: OLS estimates based on the RegFiD and the AFiD databases covering the years 2013 to 2019. The data is limited to private single-establishment firms in manufacturing or services. The dependent variable is log revenues per employee. Firm controls include eight dummy variables for firm size categories, dummy variables for high skill intensity and strong employment growth as well as a proxy for low workplace complexity. Regional controls include the trade tax multiplier, the unemployment rate and dummy variables for the presence of local airports and highway connections. Column 1 displays the baseline regression. Column 2 includes Industry-Year and State-Year Fixed Effects. Column 3 controls for the number of competitors from different productivity levels as well as an interaction of this number with being part of a multinational business group and being located in an urban area. Column 4 limits the baseline regression to firms from the manufacturing sector and 5 to firms that are located in East-Germany. Standard errors in parentheses are clustered at the firm level. Significance levels: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Figure 4.C1: *Urban and rural areas in Germany – alternative concepts*

Panel A: Upper and lower quartiles of population density

Panel B: Population densities of labour market regions (population/km^2)

Panel C: Population densities of municipalities (population/km^2)



Notes: *Panel A uses the 257 labour market regions. Regions with a population density in the upper quartile are defined as urban, those with a population density in the lower quartile as rural ones. The other regions are excluded. Panel B displays the population density in people per square kilometre for labour market regions and Panel C displays the population density for municipalities.*

Chapter 5

Conclusion

The purpose of this thesis was to analyse the interplay between regional economic structures with three different subjects, namely the exogenous population shock from an inflow of expellees, the foundation of universities for political reasons and the acquisition of local firms by foreign multinational business groups.

Chapter 2 analyses the inflow of eight million expellees to different parts of West Germany after World War II, thereby documenting the importance of local labour markets for the persistence of a major population shock. The chapter uses regional variation in the number of incoming expellees in a reduced form regression as well as an instrumental variable approach, which was based on expellees using the shortest escape routes. Within local labour markets the instrumental variable regression shows an insignificant effect of expellee inflows on the subsequent population growth of this region. Yet, between local labour markets a one percentage point increase in the expellee inflow rate in 1950 reduced the population growth of that region from 1950 to 1970 by 0.671 percentage points. Jointly the results document a strong regional persistence within local labour markets, which however was reversed between labour markets. To substantiate this finding the chapter uses insights from urban economics and argues that expellees did not need to move within local labour markets, i.e. commuting zones, but may have needed to move between local labour markets for economic reasons. This argument was substantiated empirically by an analysis of road network construction within local labour markets, which shows stronger suburbanisation in regions within regions with many expellees.

Chapter 3 studies the impact of universities on firm performance. The analysis is based on data of the Federal Statistical Office for the full universe of firms and universities in Germany for the years 2013 to 2019. The main finding is that firms in vicinity of universities generate 0.92 % more revenues per employee compared to firms located in regions without a university, which is related to around EUR 12.8 billion in firm revenues per year. To address concerns about reverse causality, the chapter focuses on a subsample of universities, which were founded during the 1960s and 70s for political reasons. To address concerns about omitted variables the chapter includes fine-grained industry fixed effects and dummies of population thresholds to the main regression. Both alternations do not change the impact of universities significantly. Furthermore the two underlying impact-channels of universities via knowledge spillovers and high-skilled local labour supply were analysed. The chapter distinguishes between groups of classical and applied universities and documents a strong impact of local labour supply. It further documents a negative relationship between proximity to the knowledge spillovers of classical universities and firm performance. This counter-intuitive result is at least partly explained by analysing the structure of business groups.

Chapter 4 analyses the productivity differences of German firms in urban and rural areas, using administrative data provided by the Federal Statistical Office of Germany. In line with the literature, the analysis documents a considerable productivity premium of multinational over national firms, as well as an urban productivity premium for national firms. Yet, in addition to these findings the analysis also documents a rural productivity premium for multinational firms, which is novel in the literature. To substantiate the findings and rule out unobserved heterogeneity, the average treatment effect of the acquisition of German firms by foreign multinational business groups (foreign takeover) is estimated. The analysis uses a two-stage estimator with an average treatment effect based on a pre-matched sample of treated and untreated German firms. Thereby, it confirms the OLS results and shows a larger productivity stimulus from foreign takeovers for firms in rural areas compared to firms in urban areas. To explain these findings the chapter analyses the role of competitors and finds evidence that the rural productivity premium results from the strategic location choices of foreign technology leaders who aim to reduce the risk of technology dissipation. Overall, the chapter contributes to the literature on urban agglomeration effects as well as the literature on multinational firms' location decision.

This thesis was motivated by the increased interest and surge in empirical research in spatial economics. The three articles in this thesis contribute to different literature strands in this subdiscipline, ranging from the effects of population shocks on regional economic equilibria, to the role of universities in a local knowledge, to the role of regional characteristics for the location decision of multinational firms. Of course even within these specified literature strands this thesis cannot address all possible factors that shape regional economic structures. By shedding light on some of these factors, I hope that my work encourages further research in spatial economics.