Essays in Political Economy: Institutions and Persistence

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Submitted to
Central European University
Department of Economics

In partial fulfillment of the requirements for the degree
Doctor of Philosophy in Economics

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Vienna, March 2026

AUTHOR'S DECLARATION

I, the undersigned, Philipp Hilmbauer-Hofmarcher, candidate for the PhD degree in Economics declare herewith that the present thesis titled *"Essays in Political Economy: Institutions and Persistence"* is exclusively my own work, based on my research and only such external information as properly credited in notes and bibliography.

Chapter 1 and 3 are solo-authored works, Chapter 2 was co-authored with Martin Halla (Vienna University of Economics and Business) and Christoph Eder (Independent, formerly JKU Linz).

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I used ChatGPT (March 2026 version) to conduct spelling and grammatical checks. The tool was used to correct any inaccurate grammar and punctuation. All affected passages were reviewed afterwards.

Acknowledgments

First and foremost, I am indebted to my advisor Ádám Szeidl for his guidance and support. His advice and encouragement were invaluable throughout the research process. I thank my co-author Martin Halla for his mentorship and extremely helpful comments. This dissertation has profoundly benefited from discussions (as well as classes) with all our faculty at the Department of Economics at CEU. Their door was always open whenever I ran into trouble or had a question about my research or writing.

I am grateful beyond words to my peers, who made my years at CEU constantly enjoyable and insightful and who helped me tirelessly at all stages of this project.

Finally, I need to express my profound gratitude to my family for providing me with unfailing support and their continuous encouragement throughout my studies and through the process of researching and writing this thesis.

Introduction

Institutions lie at the heart of political economy. They shape incentives, distribute power and resources, and influence how societies respond to shocks. Yet institutions themselves are not immutable: They are disrupted and replaced through war, occupation, and reconstruction. Understanding how such disruptions translate into long-run outcomes, and why some institutional effects persist while others fade, is central to the study of comparative development. The recent convergence between political economy and economic history has deepened our understanding of these dynamics by treating historical episodes as natural experiments that reveal the mechanisms linking institutions to behavior, production, and the spatial organization of economic activity.

A key mechanism through which institutional shocks can generate long-run effects operates through the geography of economic activity. Firms and workers benefit from proximity through agglomeration economies, which make dense economic environments more productive. Importantly, these benefits decay rapidly with distance. Often, agglomeration forces operate over extremely short spatial ranges (Rosenthal and Strange 2020), sometimes within just a few city blocks or neighborhoods. Because these effects decline steeply with distance, even relatively small shifts in the location of people and firms can have large consequences for the spatial distribution of economic activity.

Recent work in spatial economics highlights how such mechanisms can generate path dependence. Large shocks may push an economy from one spatial equilibrium to another, and the resulting allocation of population and economic activity can persist long after the original disturbance has disappeared. When agglomeration forces interact with frictions to migration, investment, or coordination, economies may not revert to their pre-shock configuration (Allen and Donaldson 2022). Instead, historical disruptions can alter development trajectories by shifting the spatial equilibrium itself. In this sense, institutions may leave lasting legacies not only through policies that endure, but through the spatial reallocation of people and firms that institutional shocks set in motion.

This thesis follows in that tradition. It combines the tools of empirical political economy with newly assembled historical data to study how institutional and physical shocks shape long-term political, economic, and urban outcomes.

Austria's post–World War II experience provides an ideal setting for studying these questions. In the aftermath of the war, the country was partitioned into four occupation zones administered by the Allied powers. For ten years, Austrians lived under two distinct institutional regimes: A Soviet-controlled zone in the east and Western-controlled zones in the rest of the country. The Soviet occupation was extractive and interventionist, marked by nationalization, expropriation, and state-run enterprises. The Western zones, by contrast, followed a policy orientation more supportive of markets, private enterprise, and democratic governance. This division created a temporary but profound institutional shock within a single national and cultural framework. When the occupation ended in 1955, Austria was reunified under a common political and economic system, allowing researchers to observe how regions exposed to different institutions evolved once those regimes were removed. This natural experiment makes Austria an exceptional case for studying institutional persistence: It combines variation in institutional exposure with a return to a unified national setting that eliminates many of the confounding factors that often complicate cross-country comparisons.

The first chapter investigates whether temporary exposure to different political and economic institutions can generate lasting changes in political preferences. It examines how the Soviet occupation influenced electoral outcomes and ideological alignment in the short and long run. Using a spatial regression discontinuity design coupled with a difference-in-differences approach, the analysis identifies short-term increases in support for the Communist Party in areas formerly under Soviet control, driven in part by the presence of Soviet-run enterprises and their role as workplaces and social environments. Yet these effects did not persist. Once the occupation ended and national institutions converged, voting patterns quickly reverted to their pre-war alignments. The chapter thus highlights an important form of non-persistence: Temporary institutional exposure can alter behavior under specific conditions but may fail to generate enduring ideological change when underlying political institutions and incentives shift.

The second chapter moves from political preferences to economic development, asking whether the same institutional division left deeper and more persistent marks on the Austrian economy. Using the same natural experiment and a combination of historical and contemporary data, it shows that areas formerly under Soviet occupation remain less economically developed today. These regions have lower population density, fewer firms, and lower-paying jobs; their residents are more likely to commute to workplaces outside the former Soviet zone. The analysis attributes these enduring disparities to a migration shock that accompanied the end of the war and the early occupation period. Large numbers of people fled westward to escape Soviet control, shifting population and economic activity toward regions under Western administration.

From the perspective of spatial equilibrium models, such a shock can move an economy onto a new development path. When workers and firms relocate, the productivity advantages associated with dense economic environments tend to follow them, reinforcing growth where activity concentrates while leaving other regions behind. Because the benefits of agglomeration operate over short distances, even relatively modest shifts in population distribution can alter local economic trajectories. The results therefore suggest that the institutional shock did not merely create temporary disruption. Instead, it triggered a spatial reallocation of people and firms that moved the Austrian economy toward a new spatial equilibrium. Once agglomeration forces reinforced these patterns, economic activity remained concentrated in the locations that initially benefited from migration flows. As a result, economic outcomes did not revert to their pre-war spatial distribution even after the institutional divide itself disappeared.

The third chapter broadens the scope to consider the resilience of urban systems in the face of physical destruction. It studies Vienna's reconstruction after extensive bombing during World War II, using newly digitized, building-level data on wartime damage linked to modern records on property values, firm activity, and demographics. The analysis finds that while heavily bombed areas experienced intense post-war reconstruction and population growth, there were no enduring differences in income, education, or real estate prices compared to less affected areas. Urban recovery, in this context, was rapid and remarkably complete. The findings point to a recovery process characterized by rebuilding and re-densification rather than socioeconomic divergence.

Together, these chapters use Austria's mid-twentieth-century history to explore the boundaries of institutional persistence. The results reveal a nuanced picture: Political attitudes appear highly adaptive, economic geography can retain deep institutional imprints through migration and agglomeration, and cities can display remarkable resilience under stable post-war institutions. Across these cases, the thesis underscores the value of economic history as an empirical foundation for political economy. Archival records, the geography of occupation, and digitized historical data make it possible to trace the mechanisms through which institutions affect behavior, population movements, and spatial organization, and to test whether these effects persist once the original institutional environment disappears.

By integrating detailed historical evidence with modern econometric methods, this work contributes to a broader conversation about the dynamics of persistence and change. It suggests that institutional legacies depend less on the duration of an institutional regime than on the mechanisms through which its effects are transmitted. Institutional shocks that alter beliefs may fade quickly once incentives change, whereas shocks that trigger migration and reshape the geography of economic activity may persist for decades because agglomeration forces reinforce the resulting spatial patterns.

Methodologically, the thesis combines the empirical nature of modern political economy with the archival depth of economic history. Each chapter leverages Austria's post-war setting as a quasi-experimental framework, exploiting sharp institutional and spatial boundaries created by the Allied occupation and the destruction of war. The use of spatial regression discontinuity designs, paired with difference-in-differences strategies, enables identification of causal effects within historically specific contexts. At the same time, the thesis makes a contribution through the construction and digitization of novel historical datasets ranging from detailed occupation-zone maps and archival records of Soviet-run enterprises to building-level data on wartime destruction and post-war reconstruction in Vienna. These data provide the empirical foundation necessary to link micro-level historical variation to contemporary political and economic outcomes.

To sum up, this dual focus on institutional mechanisms and empirical innovation highlights the complementarity between political economy and economic history. The former provides the methodological tools to identify and interpret causal effects, while the latter supplies the context and data that reveal how such effects unfold over time. By combining the two, this thesis seeks not only to document Austria's post-war transformation, but also to contribute to a more general understanding of how institutional shocks are transmitted or mitigated. In doing so, it underscores the broader potential of historically grounded political economy: To use the lessons of the past to better understand the persistence, or non-persistence, of institutions in shaping the economic and political landscapes of the present.

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1 The Soviet Occupation and Voting for the Far-Left: Evidence from Austrian Elections*

Abstract

This chapter investigates the effect of temporary institutional change on electoral outcomes. Austria's situation after World War II offers an interesting setting to study this question. The country was divided into different occupation zones for ten years. The Soviet occupation was violent, extractive and featured large-scale state intervention, while the occupation by the Western Allies was more supportive, both economically and politically. After ten years with different institutions, the regions returned to one unified state. Combining a regression-discontinuity design in space with a difference-in-differences approach, I document the existence of short-run positive effects of the Soviet occupation on the vote share of the (ideologically aligned) Communist Party, while I fail to detect any meaningful long-run changes. These results are robust to the use of different specifications and distances to the occupation zone border as well as placebo tests. In terms of mechanisms, I find novel evidence that the differential effect is, at least in part, driven by the presence of Soviet-run businesses and their impact as a workplace on the local population.

1.1 Introduction

A long-standing question in political economy is how political regimes and institutional shocks shape citizens' political preferences and electoral behavior. A large literature has documented persistent effects of major institutional changes, such as the establishment of socialist regimes in Eastern Europe, on political attitudes and voting outcomes (Vlachos 2022; Martinez, Jessen, and Xu 2023; Cantoni, Hagemeister, and Westcott 2019). While this research shows that deep and long-lasting institutional transformations can have substantial effects, less is known about the specific channels through which regimes influence political behavior, particularly in contexts where political control is temporary and institutions ultimately revert to their previous form.

Episodes of temporary territorial control provide a useful setting to study these mechanisms. In such situations, occupying powers often attempt to shape local political attitudes through economic policies, propaganda, and control over key institutions. Yet

* I am indebted to my advisor Ádám Szeidl for his guidance and support. I am grateful to Martin Halla (Vienna University of Economics and Business) for sharing his dataset of geo-coded historic demographics of Austria and for his helpful feedback. I thank Zsofia Barany, Ruben Enikolopov, Mats Köster, Alice Kügler, Robert Lieli, Arieda Muco, Stephanos Vlachos and Andrea Weber and participants at the Vienna Economics PhD Workshop and the CIVICA Workshop on Historical Persistence Mechanisms for their insightful comments. For their help with archival data I thank Rita Tezzele and Birgitt Rupp (Austrian Chamber of Commerce), Sabine Lichtenberger (Austrian Chamber of Labor) and Ernst Bezemek (fmrly Lower Austrian State Archive).

the effects of these policies on electoral behavior remain unclear, especially when the occupation ends and the pre-existing political system is restored. Recent developments have demonstrated the prevalence of short-term territorial control by state and non-state actors, such as the rise of the Islamic State and more recent inter-state conflict but also the US invasion of Iraq.[1] These cases highlight situations in which external actors temporarily control territory but do not permanently transform political institutions. In this chapter I examine whether and through which channels the Allied occupation of Austria after World War II affected the Communist Party's (KPÖ) vote share. Compared to cases of deep institutional transformation, the Austrian occupation provides a setting in which political influence occurred without a persistent change to the national institutional framework.

Austria's situation after World War II offers an informative setting to study this question. From 1945 to 1955 Austria was partitioned into four Allied occupation zones allocated to the United States, the Soviet Union, the United Kingdom, and France. Different from the German case, the Soviet occupation forces did not implement a socialist system of government. Despite the territorial partition during the occupation, Austria remained a single political entity and a prospective sovereign state. However, the treatment by occupying forces differed substantially. While the Western Allies generally supported a rapid return to an independent and economically prosperous state, the Soviets pursued different objectives. Their stated goals were primarily extracting rents as reparations for World War II, supplying their soldiers and the local population, and promoting their political ideology (Klambauer 1978; Klambauer and Bezemek 1983). Importantly, after ten years of occupation all regions returned to the same institutional framework in 1955. This setting therefore allows me to study how *differences in occupation policies*, rather than *permanent institutional change*, affected political behavior.

How the Soviet occupation affected the Communist Party's election performance is ex-ante unclear because the occupation exposed the Austrian population to competing influences. On the one hand, antipathy toward the occupiers could have reduced support for the Communist Party. Economic hardship, looting, and violence by occupying soldiers (Stelzl-Marx 2012; Johr and Sander 2002) may have generated negative attitudes toward the Soviet Union and communism more broadly. On the other hand, the Soviet administration also attempted to influence local attitudes through economic institutions and propaganda. In particular, Soviet-run enterprises and industrial estates - the so-called USIA system - offered relatively higher wages, workplace amenities, and access to consumer goods through a dedicated "supermarket" system. These policies can be interpreted as attempts to "win hearts and minds" (Alesina and Fuchs-Schündeln 2007; Dell and Querubin 2018). Anecdotal evidence suggests that employment conditions and material benefits in these establishments generated favorable perceptions among some

1. There has been a related policy discussion around occupation and military intervention in recent decades, see e.g. Bischof (2020).

workers (Klambauer 1978). At the same time, these enterprises served as platforms for Soviet propaganda. Soviet-run firms may therefore have constituted an important channel through which occupation policies influenced political attitudes and electoral behavior.

This chapter makes three contributions. First, it contributes to the literature on the political effects of regime exposure by examining a setting in which political control was temporary and did not lead to a permanent institutional transformation. Second, it highlights the role of economic institutions as a channel through which occupying powers may influence political attitudes. In particular, I study how exposure to Soviet-run enterprises affected local political behavior. Third, by combining spatial variation in occupation zones with within-zone variation in the presence of Soviet-run firms, the analysis provides evidence on the mechanisms through which economic experiences under occupation may shape electoral outcomes.

To study these questions, I first examine the discontinuity between municipalities located in the Soviet occupation zone and those in areas controlled by the Western Allies. I then explore the mechanisms behind these effects by analyzing the role of Soviet-run enterprises. Specifically, I compare municipalities with the presence of Soviet-run businesses to otherwise similar municipalities without such establishments. This allows me to assess whether economic exposure to Soviet institutions shaped local political attitudes and voting outcomes. To implement this analysis, I combine data on Austrian national election results from 1930–2019 (SORA 2019) with a newly compiled regional dataset based on multiple population censuses spanning the period before, during, and after World War II. The main outcomes are the vote share of the Communist Party of Austria (KPÖ), the vote shares of other major Austrian political parties, and voter turnout. To further explore the mechanisms operating through Soviet enterprises, I supplement these data with archival information on Soviet-run firms collected from the archives of the Austrian Chambers of Commerce and Labor and the state archives of Lower Austria and Vienna.

For the empirical strategy, I adapt the empirical approaches of Acemoglu, García-Jimeno, and Robinson (2012) and Dell (2010). I combine ideas from a Difference-in-Differences (DiD) approach with those from a Regression Discontinuity Design (RDD) in an analogous way to Eder, Halla, and Hilmbauer-Hofmarcher (2024). There are two main issues with choosing an RDD approach in this setting: Firstly, the pre-trends can not be taken into account, thus rendering it impossible to exclude the chance that these differences in vote shares already existed prior to the occupation. Secondly, the RDD would reduce two dimensions into one: While the zig-zag demarcation line creates a two-dimensional discontinuity, this method would reduce it to one dimension (the distance to the discontinuity). Thus, I utilize a Difference-in-Differences approach in space and time exploiting the discontinuities created by the border and the onset of the occupation. I include municipality-year pairs as fixed-effects for those municipalities adjacent to each other across the border to account for time-varying regional characteristics. This setup, using space and time in these regressions, allows me to demonstrate that the regions east and west of the border between Soviet and Western occupying powers were following parallel trends in their voting behavior prior to World War II, suggesting that the exact position of the demarcation line was plausibly exogenous.

I document the existence of short-run positive effects of the Soviet occupation on the vote share of the Communist Party, while I fail to detect any meaningful long-run changes. In particular, there is no significant difference before the war in the two elections around 1930, but then the difference increases from 1945 to 1949, where the vote share in the Soviet border municipalities is almost double that compared to the Western occupied border villages. This effect halves by 1956 and disappears completely after that. Examining the wider area around the discontinuity confirms these patterns. These results are robust to the inclusion of demographic controls. Analysis of the other parties show small, negative effects of the Soviet occupation at the discontinuity - the largest among those for the far-right Freedom party which is consistent with the findings by Ochsner and Roesel (2020) discussed further down below.

Lastly, I explore the mechanisms behind the difference in the vote share of the Communist Party. In particular, using recent historiographical and major own archival research, I examine how the Soviet state's role in businesses and the economy through the establishment of Soviet-run businesses and the direct Soviet military presence affected peoples' votes. For the examination of this effect, I use a Difference-in-Differences approach, just focusing on the Soviet occupation zone. I find suggestive evidence that the differential effect is in part driven by the presence of Soviet businesses and their impact as a workplace on the local population. There is an out-sized increase in vote share for the Communist Party in those municipalities which only decreases roughly two decades after the occupation ended. Comparatively little effect can be found for the presence of Soviet military camps. This suggests the mechanism to be economic - rather than physical coercion.

I contribute to the literature on shocks to institutions and societies. Cantoni, Hagemeister, and Westcott (2019) show the persistence of extreme right-wing beliefs in Germany by comparing vote shares of the NSDAP in 1933 to those of the modern-day right-wing populist party Alternative for Germany. Studying the short period before the division of Germany, Martinez, Jessen, and Xu (2023) show that even a short exposure to democracy can foster civilian opposition against an autocratic regime. Lichter, Löffler, and Siegloch (2021) find a long-run effect of Stasi surveillance in East Germany on economic performance and institutional trust after reunification. Vlachos (2022) provides evidence on the political consequences of wartime experiences. Using the differential introduction of forced conscription into the Wehrmacht in occupied France, he shows that conscripts and their descendants exhibit lower levels of trust in political institutions. These attitudes are not always reflected in aggregate electoral outcomes, however they become politically relevant with the emergence of large and radical political parties. While most of this literature focuses on long-lasting institutional exposure, my work studies a setting in which political control was temporary and did not lead to permanent institutional change. Moreover, I examine the mechanisms through which such exposure can influence political behavior, focusing on the role of economic institutions created by an occupying power.

This chapter is also related to the literature studying how exposure to political regimes and state-led persuasion can shape political preferences. Alesina and Fuchs-Schündeln (2007) show that individuals who lived under communist regimes exhibit systematically different preferences for redistribution and government intervention even decades later. Cantoni et al. (2017) document how ideological indoctrination through school curricula affected political attitudes in China. In a different context, Dell and Querubin (2018) study U.S. nation building efforts in Vietnam and show that the provision of public goods and local economic development programs influenced political support for the government. These studies highlight how political regimes can affect political behavior through exposure to institutions, propaganda, and economic policies. My chapter contributes to this literature by examining whether economic institutions created by an occupying power, specifically Soviet-run enterprises, shaped local political attitudes and electoral outcomes.

My work also contributes to the empirical literature on the economic history of Austria. Eder, Halla, and Hilmbauer-Hofmarcher (2024) show that before travel restrictions between occupation zones were introduced, about 11% of the population moved from the Soviet zone to the Western-occupied zones. This migration distorted the population distribution and contributed to persistent differences in economic activity across regions. Ochsner (2023) show that the difference between the initial line of contact and the later occupation borders created lasting differences in labor market outcomes in the Austrian state of Styria. My work contributes to this strand of the literature by examining the political consequences of the occupation, complementing existing work that focuses primarily on economic outcomes.

Finally, this chapter contributes to the literature studying how economic benefits and policy exposure shape political support. Manacorda, Miguel, and Vigorito (2011) examine the relationship between government spending and political support and find that recipients of a government transfer program are about 11% more likely to support the party in power. These findings highlight how material benefits provided by political actors can influence voting behavior through mechanisms of reciprocity or political persuasion. In a related context, Ochsner and Roesel (2020) show that municipalities in Upper Austria that experienced an inflow of Nazi migrants fleeing the Soviet advance after World War II still exhibit higher far-right vote shares today. This chapter contributes to this literature by studying whether economic exposure to Soviet-owned enterprises affected political support for the Communist Party. Soviet-run firms offered relatively higher wages, workplace amenities, and access to consumer goods, potentially generating political support through economic experiences or exposure to propaganda. I present suggestive evidence consistent with such economic channels of political influence, highlighting how occupation policies may shape local political behavior.

The remainder of the chapter is organized as follows: Section 1.2 presents the historical background on the Soviet occupation of Austria and on the Communist Party. In Section 1.3 I detail my research design, describe the data sources and present some descriptive statistics. Section 1.4 outlines the main results and describes evidence on the mechanisms. Section 1.5 offers concluding remarks.

1.2 Institutional Background

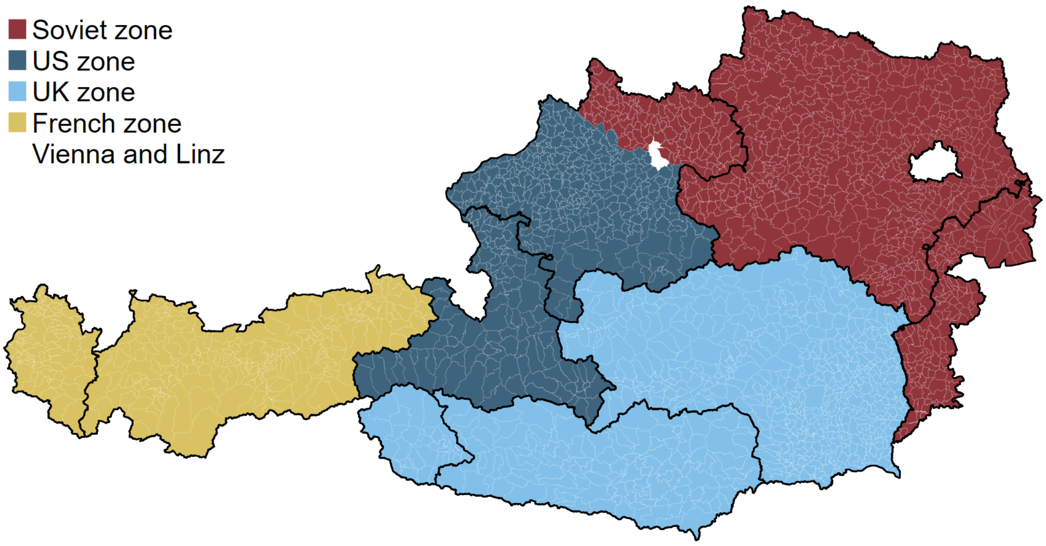
Following the end of World War II, Austria was occupied by the Allied forces of the United States, Soviet Union, Great Britain, and France. The country was divided into four zones of occupation, with Vienna also being divided into four sectors (see Figure 1.1). The occupation lasted from 1945 to 1955, during which the four Allies aimed to eradicate Nazi influence and rebuild democratic institutions. However, tensions between the Western Allies and the Soviet Union, as well as domestic political divisions, hindered the process of establishing a stable democratic government in Austria (Bader 1966).

Importantly, the political and legal principles underlying this occupation had been defined long before Allied troops entered Austria. Already in the Moscow Declaration of October 1943, the United States, the United Kingdom, and the Soviet Union declared the 1938 *Anschluss* null and void and committed themselves to the re-establishment of an independent Austrian state. This declaration required the occupying powers to treat Austria not merely as defeated territory but as a state whose sovereignty was to be restored, even while the declaration acknowledged Austria's responsibility for its participation in the war (Erickson 1950).

After negotiations were halted in the interim, finally in 1955 the Austrian State Treaty was signed, declaring Austria as a neutral and independent country, ending the occupation and restoring full sovereignty to Austria. Importantly, the occupation forces treated Austria as a sovereign state-to-be more and more, as time went on. In particular, the Allied forces handed over most of the responsibilities for important government functions like the welfare state, education, policing and security to the Austrian authorities early in the occupation and kept only a minimal military presence in Austria towards the end (Stelzl-Marx 2012).

This relatively light-handed approach was also anchored in early Allied planning. Beginning in January 1944, the European Advisory Commission developed detailed principles

Figure 1.1: Occupation Zones in Austria (1945-1955)



for the future occupation of Austria, with the explicit aim of creating central Austrian administrative authorities and preparing the ground for free elections. The zoning framework ultimately applied in 1945 drew on these negotiations, even though shifting military front lines necessitated several last-minute zone swaps before the final boundaries were agreed upon in July 1945.

1.2.1 The Soviet Occupation

The Soviet occupation of Austria was marked by a large range of misdeeds and abuses of power. These included the seizure of property and dismantling of machinery and resources. They also confiscated factories and other resources and incorporated those in a Soviet-run holding (the USIA system) that skimmed profits as reparations and had large leeway in business decisions (Klambauer 1978).

During the Soviet occupation of Austria, there were numerous reports of rapes committed by Soviet soldiers against Austrian women. These rapes were part of a larger pattern of sexual violence and abuse by Soviet forces throughout the Eastern European countries they occupied. Estimates of the number of rapes committed in Austria during this period vary. Contemporary sources suggest the number to be in the tens of thousands, especially concentrated at the beginning of the occupation. Women of all ages and backgrounds were targeted, and the rapes often involved extreme violence and brutality. It is important to note that misdeeds were not a uniquely Soviet phenomenon, however the frequency of these acts was much higher in their occupation zone. The best evidence on this issue was provided by Johr and Sander (2002), who study the number of rapes in post-WWII Berlin.

The Soviet authorities did little to address the issues of rape and looting by occupying soldiers, and in some cases, they actively protected those who had committed sexual crimes. The Austrian government, meanwhile, struggled to respond effectively to these issues, in part because of the broader challenges posed by the occupation.

These difficulties were exacerbated by the prolonged stalemate in Allied negotiations over Austria's final status. Ferring (1968) shows that State Treaty talks began in 1947 but remained stalled for years due to emerging Cold War tensions, only resuming meaningfully after 1953. The delay not only prolonged Soviet control over their zone, including the USIA economic empire, but also limited the Austrian government's ability to challenge abuses or renegotiate the conditions of occupation.

It was only in the years following the end of the occupation that these issues during the Soviet occupation of Austria began to receive greater attention and acknowledgment.

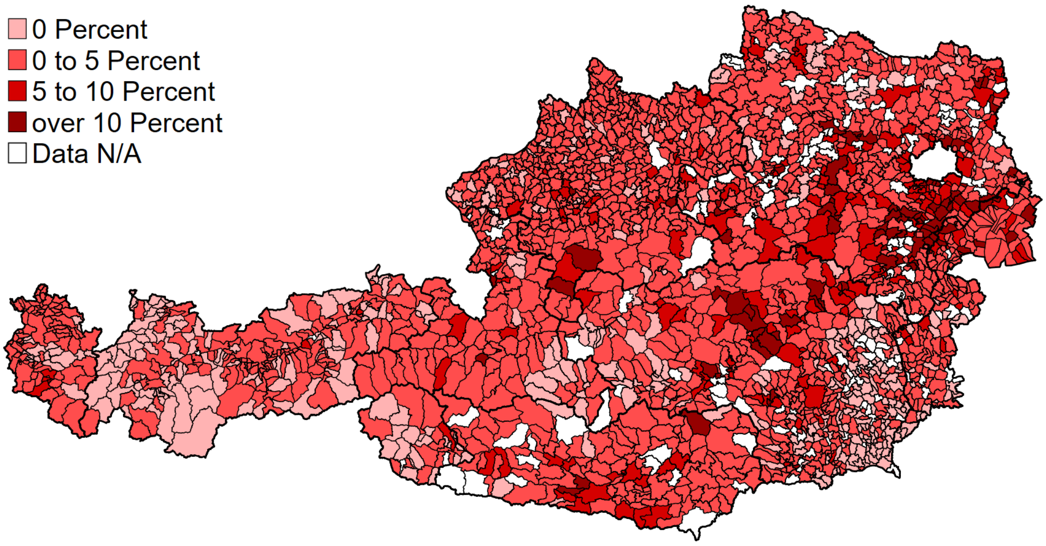
1.2.2 The Communist Party in Austria

The Communist Party of Austria (KPÖ) was founded in 1918, shortly after the end of World War I and the collapse of the Austro-Hungarian Empire. The party initially had strong support from industrial workers, and in the 1930s it emerged as a major political force. With the onset of Austria's authoritarian regime of Engelbert Dollfuss, the KPÖ was banned in 1934, and many of its leaders were arrested or forced into exile. During World War II, a lot of KPÖ members were active in the resistance against Nazi Germany, but were also targeted for persecution and many were sent to concentration camps. After the war, the KPÖ was reestablished and played a role in rebuilding Austria's political system.

In the postwar period, the KPÖ became a vocal critic of the Austrian government and remained a strong advocate for workers' rights. It advocated for socialist policies such as nationalization of key industries and a more equal distribution of wealth and later opposed Austria's participation in the European Union. However, the party struggled to gain significant electoral support and went through many leadership changes after the 1960s (Mueller 2017). Figure 1.2 shows the Communist Party's vote share in 1949, at the height of the Allied occupation.

A controversial topic regarding the Communist Party has been it's relationship to the Soviet administration of Austria. Multiple issues are of note regarding the setting studied here: There were both financial ties and personal contacts between the Soviet Union

Figure 1.2: Communist Party Vote Share in 1949



and the Communist Party. Whether through subsidiaries or directly, the Soviet administration supported Communist newspapers, their election campaign and unions (Mueller 2017). Even after the occupation ended some level of support persisted. The building where major party offices and media resided, for example, was owned by a company close to the government of the German Democratic Republic until the 1990s. However, on policy the Communist Party and the Soviet administration did not see eye-to-eye on many issues: While the KPÖ supported the government's goal to nationalize key industries, the Soviets decided to incorporate them into their own administrative structure.

1.2.3 Soviet Influence in Occupied Austria

There are multiple channels through which Soviet influence was exerted on Austria during my period of study. First is the Administration for Soviet Property in Austria (using the Russian acronym *"USIA"*), which was established by the Soviet Union in Austria after World War II, operating during the Soviet occupation of the country. The primary function of the agency was to manage and oversee Soviet-owned businesses and properties in Austria, which included factories, mines, other industrial assets and farms that had been confiscated or nationalized by the Soviet Union. Importantly, the mandate of the USIA only covered firms previously (at least partially) owned by German businesses or the Nazi regime. The establishment of the USIA was part of a broader Soviet effort to exert control over the economy of Austria, and to advance Soviet economic and political interests in the region. USIA was headed by a Soviet-appointed administrator, and was staffed by a combination of Austrian and Soviet employees (Bader 1966).

The USIA system was responsible for managing the day-to-day operations of these properties, including the hiring and management of employees, the purchase of supplies and equipment, and the negotiation of contracts with other businesses and governments. The agency also played a role in promoting Soviet interests in Austria, including the distribution of Soviet propaganda and the cultivation of political contacts with Austrian politicians and business leaders (Klambauer 1978; Klambauer and Bezemek 1983).

While the USIA was a powerful and influential agency in post-World War II Austria, it was controversial and subject to criticism from many quarters. Austrian business leaders and politicians were resentful of the agency's interference in the country's economy, especially as the Soviet leadership of USIA used a legal loophole to avoid paying taxes in Austria, which meant 1.1 Billion Schillings in foregone revenue according to Austrian authorities. There are also accounts that Austrian workers viewed the agency's management of factories and other assets in some cases as heavy-handed and oppressive (Williams 2007).

In addition to its management of Soviet-owned properties, USIA also played a role in the broader Austrian economy, with the agency engaging in trade and investment activities across a range of sectors. This included the purchase of Austrian goods and services for sale in their own USIA shops and export to the Soviet Union, as well as investments in Austrian businesses and infrastructure projects (Bischof, Pelinka, and Stiefel 2000). In terms of support for the Austrian economy, the Marshall-Plan support of the Western Allies stands out the most. While most of this funding was spent in Western-occupied Austria, a small fraction was also used on the Soviet-occupied territory. The Soviet economic support was on much lower levels compared to the US, as their goal of winning Austrians' hearts and minds was secondary to their main goal of extracting profits from the USIA businesses as reparations for World War II.

Second, more directly, Soviet influence was exerted through the presence of Soviet occupation forces: Recent research reveals the locations of historical encampments in the Soviet-occupied sector of Austria (LBI für Kriegsfolgenforschung 2024). Essentially, there are three types: (1) internment camps for former nazi party members, (2) refugee and POW camps and (3) camps for Soviet soldier (as part of the occupation). In particular, the presence of encampments for Soviet soldiers has been the topic of historical discussion. With the occupation also quickly came violence, rape and looting (Stelzl-Marx 2012). Violence was most intense in the chaotic early weeks of the occupation, especially in eastern Austria, where exhausted front-line Soviet units operated with minimal oversight. Conduct varied widely: Some troops committed extensive looting and sexual violence, while others, particularly later arrivals or those under stricter commanders, showed greater discipline. Although Soviet headquarters repeatedly ordered crackdowns and sometimes imposed harsh penalties, enforcement in spring 1945 was uneven. As command structures settled in the following months, abuses decreased but did not disappear entirely.

1.3 Data and Empirical Approach

The main aim of my empirical approach is to estimate the effect of the differential occupation by the Soviet and Western Allies on the political situation for the period during the occupation and after 1955, when the demarcation line became obsolete and one joint, sovereign state was restored.

1.3.1 Data Sources

To address my research questions, I combine data from three datasets: The first one, on national election results for Austria from 1927-2019 (SORA 2019), is merged with a dataset on historic demographics: This is based on a detailed, regional dataset from multiple censuses spanning the period before, during and after World War II. As outcomes, I mainly use the vote share of the Communist Party of Austria (KPÖ), the vote shares of other major Austrian political parties and the participation rate in elections. I augment these outcomes with further data to explore the mechanisms through Soviet businesses, which I collected from the archives of the Austrian Chambers of Commerce and Labor and the state archives of Lower Austria and Vienna.

The data on national election outcomes for Austria spans from 1927 to 2019 and comes from Austria's statistics office and Ministry of Interior. Before World War II, there are two elections (1927 and 1930) where data is available on the municipality level. Those vote shares are later used to examine the pre-trends before the Soviet occupation. During and shortly after the occupation, there are four elections: 1945, 1949, 1953 and 1956. Lastly, all national elections after the occupation period until today are in the dataset as well. The data contains vote totals on the municipality level and includes all major political parties (the social democrats, the conservatives, the communist party and the far-right freedom party) and turnout information as well.

I use municipality-level data on population size, share of male workers, share of workers in different sectors and other characteristics as controls. These data were collected in a project (see Eder, Halla, and Hilmbauer-Hofmarcher 2024) on the economic effects of the occupation of Austria. They combine data from different sources published by *Statistik Austria* (the Austrian statistical agency) and some of it's predecessor agencies. The majority of those originate from the decennial census. For the early years these data were collected from printed publications. For the purpose of this study, I will mainly use the rich dataset from the last complete census before the war in 1934.

The data available on the municipality level have one major constraint: The municipality borders have changed drastically over the sample period. Importantly, since 1934 the number of municipalities in Austria has dropped from $4,397$ to just $2,354$ in 2011. This is mainly caused by multiple rounds of administrative reform and declining rural population. For the main analysis sample which covers the *area along the border of the occupation zones*, I use the sample from Eder, Halla, and Hilmbauer-Hofmarcher (2024) to generate a time series for the smallest geographic unit one could cleanly trace over time. Thus, in the end there are 95 mutually exclusive geographic units which comprise 128 municipalities when considering the municipality borders from 2011. Of these newly created units, there are 50 in the Soviet occupation zone and 45 in the zone controlled by the Western Allies. Panel B of Table 1.1 shows pre-war summary statistics for this sample.

To further explore the mechanisms of the difference in the vote share of the Communist Party, I turn to the Soviet-run USIA businesses detailed above. In particular, the subject of major archival work was the Soviet state's role in businesses and establishments and how it affected peoples' attitudes. I collected data on those businesses from the archives of the Austrian Chambers of Commerce and Labor and the state archives of Lower Austria and Vienna. The information includes names and locations of these firms, supplemented by a short description of their sector and business activity. Figure 1.3 depicts the municipalities which host at least one USIA business. Panel C of Table 1.1 presents summary statistics on these municipalities in 1934 before the war and onset of the occupation. On some firms there is also information on capital stock, revenue and employees available. An example of the records held on the USIA businesses can be found in Appendix Figure A.1.[2]

Lastly, there is the dataset used to examine Soviet influence through their occupation forces: This data is part of a recent research project on the locations of historical encampments in the Soviet-occupied sector of Austria, published in LBI für Kriegsfolgenforschung (2024). In Figure 1.4 I display the different types of encampments presented in this project. In total these can be summarized as: (1) internment camps for former nazi party members, (2) refugee and POW camps and (3) camps for Soviet soldier (as part of the occupation). In my analysis, I will mainly focus on the last of these groups.

1.3.2 Estimation Strategy

To identify the differential effect of Soviet and Western occupation on vote shares, I implement two complementary Difference-in-Differences (DiD) strategies. The first relies

2. The dataset was obtained through multiple sources: A list of firms was obtained from the archives of the Austrian Chambers of Commerce and Labor. More detailed information on the firms was gathered on Lower Austria (Klambauer and Bezemek 1983) and Burgenland (Baumgartner 2005). Further background information come from the state archives of Lower Austria and Vienna, Steiner (1993) and Karner (2005).

Table 1.1: Descriptive Statistics

Group:	West. Occ.		Soviet Occ.		Difference	
	Mean	SD	Mean	SD	Diff.	p-val.
PANEL A:						
Municipalities within 50km of Zone border (N=609)						
Area (sq. km)	28.53	34.85	14.41	16.75	14.12***	(0.00)
Population	1515.92	1871.39	1369.56	2460.16	146.36	(0.42)
Share male pop.	0.49	0.03	0.50	0.02	-0.00	(0.07)
Share pop. working farming	0.53	0.23	0.51	0.25	0.02	(0.40)
Share pop. working manuf.	0.23	0.12	0.25	0.16	-0.02	(0.06)
No. of Obs.	260		349		609	
Group:	**West. Occ.**		**Soviet Occ.**		**Difference**	
	Mean	SD	Mean	SD	Diff.	p-val.
PANEL B:						
Pre-War Census 1934 (Neighboring municipalities N=95)						
Area (sq. km)	54.31	67.60	47.20	51.21	7.11	(0.56)
Population	2318.60	2706.06	2439.06	1990.54	-120.46	(0.80)
Share male pop.	0.49	0.02	0.50	0.01	-0.00	(0.15)
Share pop. working farming	0.53	0.20	0.50	0.19	0.02	(0.54)
Share pop. working manuf.	0.21	0.11	0.24	0.11	-0.03	(0.21)
No. of Obs.	45		50		95	
Group:	**West. Occ.**		**Soviet Occ.**		**Difference**	
	Mean	SD	Mean	SD	Diff.	p-val.
PANEL C:						
Pre-War Election 1930 (Neighboring municipalities N=95)						
Turnout	0.88	0.04	0.85	0.08	0.03*	(0.02)
Share SPOE	0.25	0.18	0.30	0.16	-0.05	(0.16)
Share OEVP	0.48	0.21	0.27	0.29	0.21***	(0.00)
Share KPOE	0.00	0.00	0.00	0.00	0.00	(0.54)
Share FPOE	0.01	0.02	0.02	0.02	-0.00	(0.34)
No. of Obs.	45		50		95	
Group:	**No USIA**		**USIA**		**Difference**	
	Mean	SD	Mean	SD	Diff.	p-val.
PANEL D:						
Soviet municipalities with(out) Soviet businesses (N=647)						
Area (sq. km)	16.05	19.01	12.70	11.30	3.36	(0.09)
Population	1270.48	1611.78	1993.94	5261.17	-723.45**	(0.01)
Share male pop.	0.50	0.02	0.50	0.03	-0.00	(0.96)
Share pop. working farming	0.53	0.24	0.45	0.27	0.08**	(0.00)
Share pop. working manuf.	0.24	0.15	0.27	0.18	-0.04*	(0.03)
No. of Obs.	550		97		647	

Figure 1.3: Location of USIA Businesses in the Soviet Zone

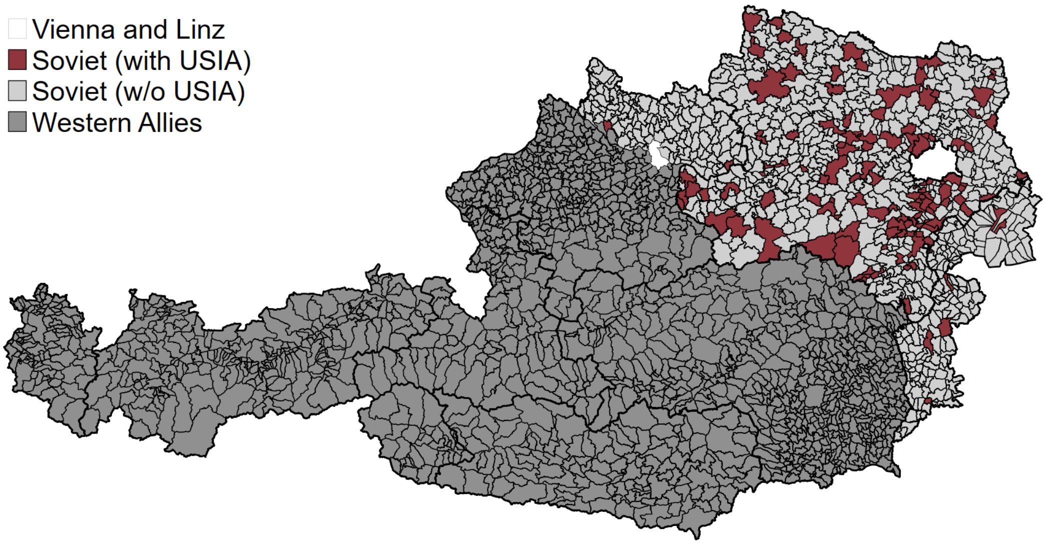
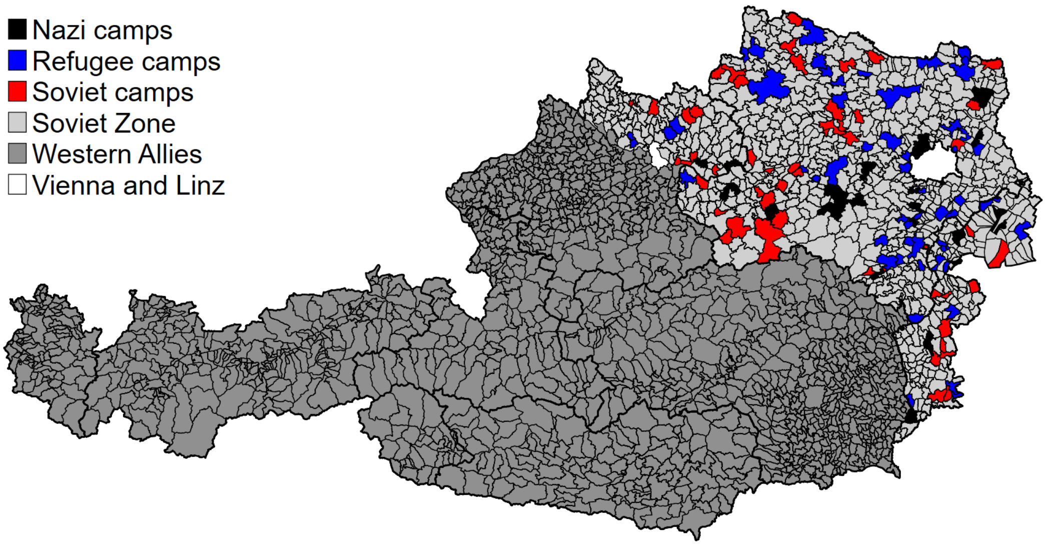


Figure 1.4: Location of Encampments in the Soviet Zone



Notes: This graph shows the locations of encampments in the Soviet Zone, in particular military camps.

on municipalities located within a fixed distance (50km in the baseline) of the occupation-zone border. The second refines identification by using municipality pairs that are adjacent to the demarcation line, allowing me to estimate the causal effect *at the border* while controlling for highly granular spatial heterogeneity.[3]

Difference-in-Differences within 50km of the Border

The first empirical specification reduces geographical confounding by restricting the sample to municipalities close to the former demarcation line. Nearby municipalities share similar pre-treatment characteristics and access to regional markets, infrastructure, and economic networks.[4] The baseline specification is:

$$Y_{i,t} = \alpha + \gamma_t \cdot Year_t + \beta_t \cdot (Soviet_i \times Year_t) + Soviet_i + X_{i,1934} + \epsilon_{i,t}. \tag{1.1}$$

Here, $Y_{i,t}$ denotes the vote share in municipality i and year t. The binary variable $Soviet_i$ equals one if the municipality is located in the Soviet occupation zone. The coefficients of interest are β_t, which measure the difference in vote-share trajectories between Soviet- and Western-occupied municipalities in year t relative to 1930. Estimates for years before 1945 serve as a test of the parallel-trend assumption, while post-1945 estimates capture the political consequences of the differential occupation regimes.

Pitfalls of Regression Discontinuity Design

At first glance, the demarcation line between occupation zones appears well suited for a spatial Regression Discontinuity Design (RDD), with distance to the border serving as the running variable. However, as emphasized in previous work, relying on distance introduces a conceptual challenge: It reduces a *two-dimensional* spatial discontinuity to a *one-dimensional* forcing variable.[5] The zig-zag shape of the occupation boundary implies that equal distances do not necessarily correspond to comparable geographic or economic conditions. This mismatch weakens continuity assumptions and limits the credibility of a traditional RDD.

Border-Pair Difference-in-Differences

To address this problem, I adapt the *"region-pairs"* empirical strategy first used by Acemoglu, García-Jimeno, and Robinson (2012) (and similar to Dell (2010)), and apply it in a way analogous to similar work on the economic effects of the Soviet occupation (Eder, Halla, and Hilmbauer-Hofmarcher 2024). The key idea is to preserve the two-dimensionality of geographic space by forming *pairs of neighboring municipalities* that lie on opposite sides of the demarcation line.

3. A third set of estimations, introduced later in this chapter, examines the mechanism of Soviet-run businesses by focusing exclusively on Soviet-occupied municipalities and exploiting the geographic allocation of Soviet firms as identifying variation.

4. This logic parallels standard "local comparison" designs in spatial DiD settings, where restricting the sample to a narrow geographic bandwidth improves comparability between treated and control units.

5. See our discussion in Eder, Halla, and Hilmbauer-Hofmarcher (2024). Because the demarcation line is highly irregular and non-linear, municipalities equidistant from the border may differ substantially in geography, terrain, and economic structure.

I restrict the sample to all municipalities directly adjacent to the former occupation border (see Figure 1.5). For every municipality on one side of the border, I identify all neighboring municipalities on the other side and construct municipality pairs.[6] For each pair j, I estimate:

$$Y_{i,j,t} = \alpha + \beta_t \cdot Soviet_{i,j} + \phi_{j,t} \cdot Area\text{-}Pair_j + X_{i,1934} + \epsilon_{i,j,t}. \tag{1.2}$$

The variable $Y_{i,j,t}$ is the vote share in municipality i belonging to pair j in year t. The binary indicator $Soviet_{i,j}$ denotes whether municipality i lies in the Soviet zone. The term $\phi_{j,t}$ is a time-varying pair fixed effect, absorbing all region-specific shocks and time-varying local characteristics affecting both members of a municipality pair. These controls account for access to markets, local political conditions, reconstruction policies, terrain, climate, and other unobserved factors shared by bordering municipalities.

As in the previous specification, the parameters of primary interest are the β_t. Coefficients estimated for years before 1945 provide a test of differential pre-trends and the plausibility of parallel trends. Post-1945 coefficients capture the effect of the contrasting occupation policies on political outcomes. The empirical framework closely parallels that used in similar research on the long-run economic legacy of the Allied occupation (see Chapter 2). In both cases, identification relies on the exogenous creation of the demarcation line, which separated regions that were otherwise geographically and economically comparable.[7] By focusing on a small geographic area on both sides of the border and by leveraging highly local cross-border pairings, the design isolates the causal impact of the Soviet occupation from confounding regional trends.

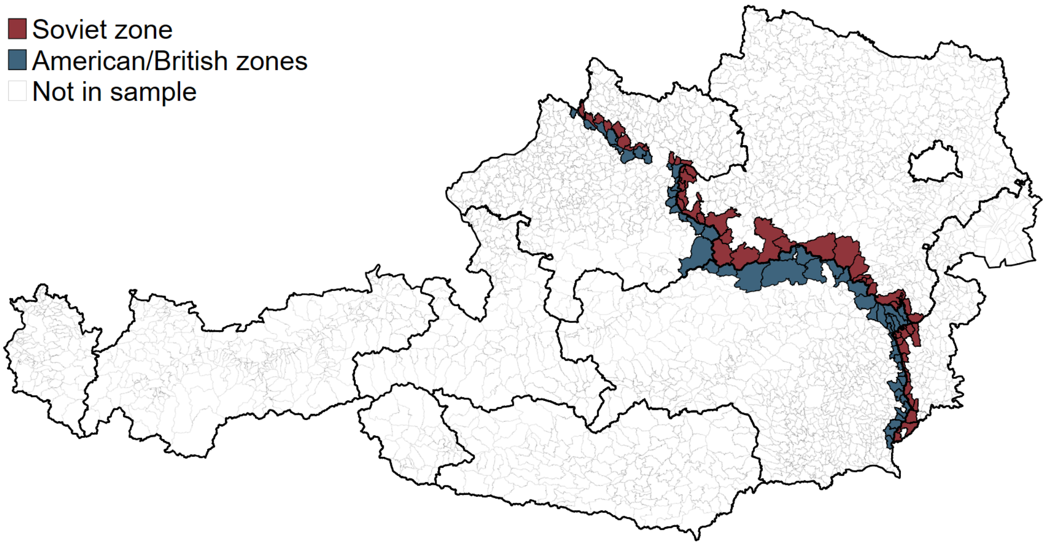
Next, to explore the mechanism of Soviet-run businesses and their impact on vote shares within the Soviet sector, I will examine the subset of Soviet-occupied municipalities and exploit the location of these businesses as identifying variation. There are roughly 260 firms in Vienna, Lower Austria, Burgenland and parts of Upper Austria in this dataset. These were taken over by the Soviets from German owners and nationalized industries of the Nazi regime and fall mainly in three types: (1) Companies already (co-)owned by Germans at the start of the war, (2) Economic aryanization of businesses after 1938 or (3) Nationalization of important industries by the Nazis. One important fact with respect to the Soviet decision to take over these specific enterprises was that *these were already at predetermined locations, some even from before the war* and thus their choice is exogenous to the Soviets through previous German ownership.

As a first step of comparison, I look at the differences of vote shares between municipalities with and without Soviet businesses *within the Soviet zone*. This translates into the

6. A municipality may enter multiple pairs if it borders multiple municipalities across the demarcation line. This is a feature of the spatial structure rather than a flaw; it ensures that all relevant cross-border comparisons are captured. See Appendix Figure B.1 in Chapter 2 for an illustrative example.

7. In previous work, the objective was to trace long-term economic divergence; here, the focus shifts to political behavior. The underlying identification logic, however, is identical.

Figure 1.5: Border Municipalities during Occupation (1945-1955)



following estimation equation:

$$Y_{i,t} = \alpha \times USIA_i + \gamma_t \times Year_t + \beta_t \times (USIA_i \times Year_t) + X_{i,1934} + \epsilon_{i,t} \qquad (1.3)$$

where $Y_{i,t}$ is the vote share in municipality i in year t. The binary variable $USIA_i$ is one if the municipality hosts at least one USIA business, and zero otherwise. Then, the interaction $USIA_i \times Year_t$ captures the dynamic differential effect between municipalities with and without those businesses. The parameter of interest is β_t. This time-varying parameter provides the average difference between the vote share of a municipality in year t relative to the baseline year of 1930. The estimates of $\beta_{t<1945}$ before World War II test for differential pre-trends before the occupation started and can be interpreted as suggestive evidence for the parallel-trend assumption. Estimates of $\beta_{t\geq 1945}$ then show the effect of the differential presence of the businesses.

Further empirical explorations focus on using matching to create a more similar comparison group. In particular, municipalities which featured USIA businesses are larger and more blue-collar. Thus, in an additional specification I match municipalities on 1934 characteristics.

1.4 Results

1.4.1 Main Specification

As a starting point, I present the results from the Difference-in-Differences specification from equation (1) which considers municipalities within 50km of the demarcation line in Figure 1.6. The results before the war are indicative that there was no differential effect between occupation zones before the Allied occupation. Then there are three elections with a significant, positive impact of the Soviet occupation on the Communist Party's vote share. After the occupation ended however, the effect dies down and is not significantly different from zero from 1962 onward. In terms of magnitude, the effect reaches its peak in 1949, where there is a one percentage point difference in vote share. This effect is large in magnitude, given that the Communist Party received a vote share of 5% nationally and roughly 2.5% in the sample municipalities.

In Appendix Table A.1 I use different distances (25, 50 and 100km) as a robustness check, which yields qualitatively similar results. However, there is one major caveat: Since this specification considers a wide band along the border, municipalities are quite heterogeneous. Panel A of Table 1.1 provides pre-WWII descriptive statistics for those: Municipalities to the west of the border are larger and more rural compared to those in the Soviet-occupied zone. Thus, I proceed with the specification which uses municipalities directly at the occupation zone border.

My preferred specification is the one using border pairs, outlined in equation (2). Panel B of Table 1.1 provides the pre-WWII descriptive statistics for the main analysis sample: Municipalities are balanced along all available 1934 characteristics. The results for the border specification is shown in Figure 1.7. The main finding is a temporary, statistically significant increase in the vote share for the Communist Party in the (former) Soviet zone, qualitatively similar to the first specification. Because of the nature of the empirical strategy used here, these municipalities along the border appear in several area-pairs. Therefore, the standard errors are clustered by municipality within a pair.

Table 1.2 summarizes the estimation results presented above: In Column (2), I present the results for the regression outlined in equation (2) which includes demographic controls. Column (1) shows the same specification without those controls. Column (3) pools all left parties (Communist Party and Social Democrats) into one. The first two columns provide two interesting insights. First, the response to the occupation is indeed immediate and only temporary. By 1962 the differential effect vanishes completely (and even becomes slightly negative, significant at a 10% level). Second, there is not a large difference in vote share for the Communist Party before World War II.[8]

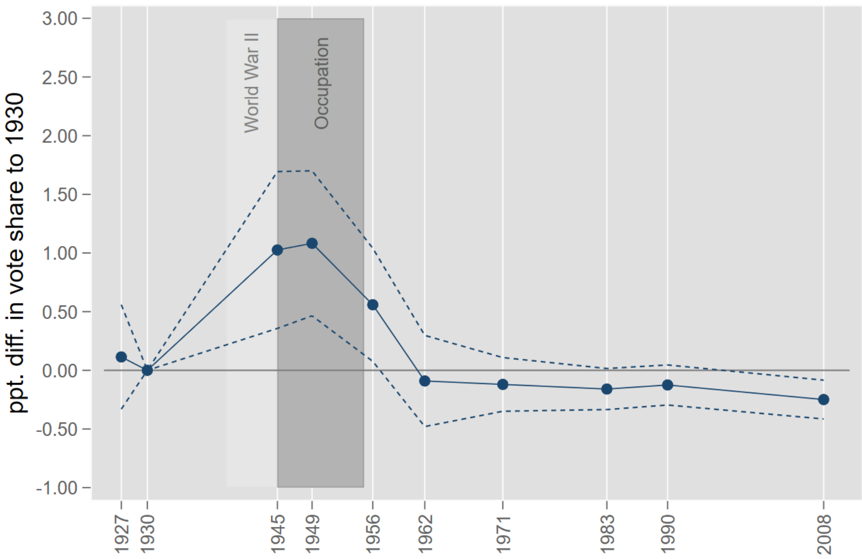
8. As a further robustness check in Appendix Table A.2 I run the border specification on a group of

Table 1.2: Communist Party vs. All left

Dep. Var. (KPOE):	**(1) No Controls**	**(2) Dem. Controls**	**(3) All Left**
Pre-WWII differences			
1927 × Soviet Zone	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.006)
Base-year (1930) differences			
Soviet Zone	-0.000*	-0.002**	0.051***
	(0.000)	(0.001)	(0.013)
Post-WWII differences			
1945 × Soviet Zone	0.005	0.005	0.005
	(0.004)	(0.004)	(0.011)
1949 × Soviet Zone	0.009***	0.009***	-0.001
	(0.003)	(0.003)	(0.009)
1956 × Soviet Zone	0.005**	0.005**	-0.007
	(0.002)	(0.002)	(0.009)
1962 × Soviet Zone	-0.003*	-0.003*	-0.018**
	(0.002)	(0.002)	(0.009)
1971 × Soviet Zone	-0.000	-0.000	-0.021*
	(0.001)	(0.001)	(0.012)
1983 × Soviet Zone	0.000	0.000	-0.032***
	(0.001)	(0.001)	(0.010)
1990 × Soviet Zone	0.001	0.001	-0.031***
	(0.000)	(0.000)	(0.011)
2008 × Soviet Zone	0.000	0.000	-0.038***
	(0.000)	(0.000)	(0.011)
Pair-year FE	Yes	Yes	Yes
No. pairs	93	93	93
No. unique municipal.	95	95	95
No. periods	23	23	23
No. observations	1,860	1,860	1,860
R-squared	0.65	0.71	0.84
Mean of dep. var.	0.01	0.01	0.39
S.d. of dep. var.	0.02	0.02	0.16

Notes: In this table the dependent variable is the Communist Party vote share (and all left parties' vote share). The city of Linz is excluded, since the demarcation line disunited the city. The *1934 controls* include the population share in agriculture, in manufacturing, and of males, municipality size (in sq. km), and the number of inhabitants. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Figure 1.6: Communist Party: DiD within 50km of the zone borders



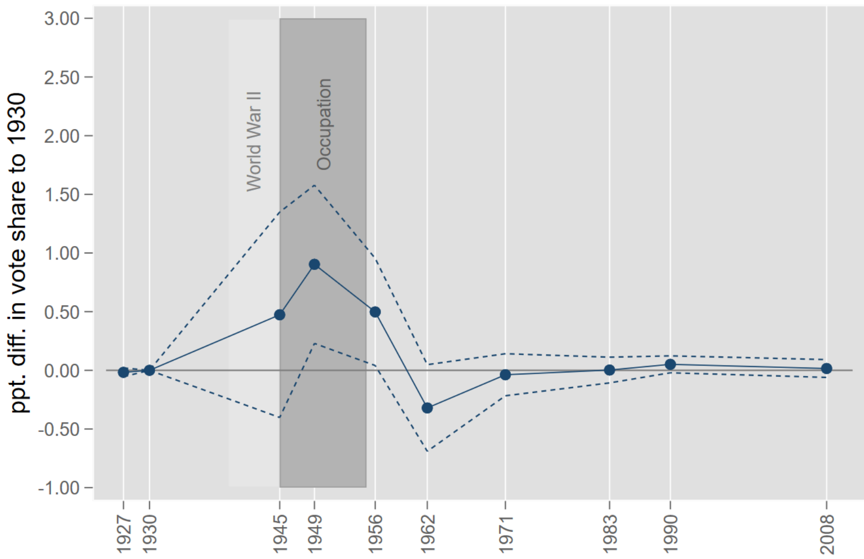
Notes: This graph shows the effect of the Soviet occupation on the Communist Party vote share in municipalities within 50km of the demarcation line. Dashed lines show the 95% confidence intervals. Table A.1 shows corresponding estimation results.

A natural extension is to look at the vote shares of other major parties. Table A.3 compares the vote shares of the Communist Party (Column 1), the Social Democrats (Column 2) and the far-right Freedom Party of Austria (Column 3), again using the regression outlined in equation (2). For the Social Democrats, there is no difference in vote share during the occupation and, compared to the mean, only a small negative effect two decades later. The results for the far-right Freedom Party are more interesting: There is a lasting negative effect after World War II which persists to this day.

This pattern aligns closely with the mechanism proposed by Ochsner and Roesel (2020), who document how postwar inflows of ideological extremists reshape the political landscape of receiving communities in the long-run. They show that municipalities in Upper Austria close to the border that witnessed substantial in-migration of fleeing Nazis exhibited a long-run increase in far-right political preferences, an effect they argue is driven

municipalities which are further apart. There, two issues are at play: Firstly, the further the distance increase the less comparable the "border pairs" get, thus controlling for this pair-year fixed-effect may introduce noise. Secondly, the more distant municipalities are less tractable over time and because of the above noted particularities of Austrian municipalities, a lot of observations are lost. This is reflected in the results: The point estimates are qualitatively similar, however confidence intervals are large.

Figure 1.7: Communist Party: DiD with border pairs



Notes: This graph shows the effect of the Soviet occupation on the Communist Party vote share in bordering municipalities along the demarcation line. Dashed lines show the 95% confidence intervals. Table 1.2 shows corresponding estimation results.

not only by direct demographic changes but also by persistent norm transmission, local network formation, and the entrenchment of extremist milieus over time. In their setting, the arrival of ideologically committed migrants shifted local social and political equilibria, thereby exerting an influence that far outlasted the original migrants themselves. Taken together, both sets of findings highlight the path dependence of local political equilibria: Whether through the infusion or disruption of extremist social networks, mid-20th-century shocks shaped Austria's contemporary political geography.

Lastly, one might wonder whether these differences are driven by a differential effect of the occupation on democratic participation. Thus, in Table A.4, I examine the turnout rates in the elections studied previously. There is a significant difference in turnout already before the war: The municipalities to the east of the border have lower democratic participation than those to the west. This is then reversed by 1945 and this persists to this day.

1.4.2 Robustness

One threat to the identification strategy of using occupation borders is that the different Allied occupation authorities impacted people's voting behavior based on some other dimension than Soviet vs. Western Allies (see Figure 1.1). To test for this, I run a placebo test along the borders between the US-UK and the US-French occupation zones.

Appendix Table A.5 presents the results of these placebo tests. Column (1) presents the results for the US-UK border and Column (2) shows the effect for the US-French occupation zone border, using the same empirical strategy as outlined in equation (2). As expected, in both specifications there is no significant or quantitatively large effect of these occupation zone borders. These findings provide supporting evidence for the validity of the estimation results presented above.

1.4.3 Mechanisms of Soviet Influence

The last part of my empirical work focuses on the mechanisms, in particular the Soviet-run businesses and their impact on the Communist Party vote share. As a first step, I approach this issue by comparing municipalities with and without Soviet businesses *within the Soviet zone*. Using the framework outlined in equation (3), I estimate the effect of the presence of at least one USIA business in a municipality. The results of this estimation are reported in Columns 1 and 2 of Table 1.3 and displayed in Figure 1.8. There is a large increase in the vote share in those municipalities where USIA businesses are present. The pattern observed here is similar to the one along the Soviet-Western Allied border, with one major difference: The effect is not present before World War II, increases sharply at the onset of the occupation but vanishes only some two decades afterwards.

The major caveat with this result is that municipalities with and without USIA businesses are different in a number of pre-WWII characteristics. In Panel C of Table 1.1 these differences are presented: Municipalities with Soviet businesses have a larger share of population working in manufacturing, are smaller and have a significantly larger residential population. Using propensity score matching I create a more similar comparison group. In particular, I use municipalities which have a similar share of workers in manufacturing compared to those which featured USIA businesses. The results of this estimation are reported in Column 3 of Table 1.3. I find that the effect remains almost the same and stays highly significant, indicating that this differential effect on vote shares is not driven by different municipality characteristics but that the presence of Soviet businesses plays an important role.

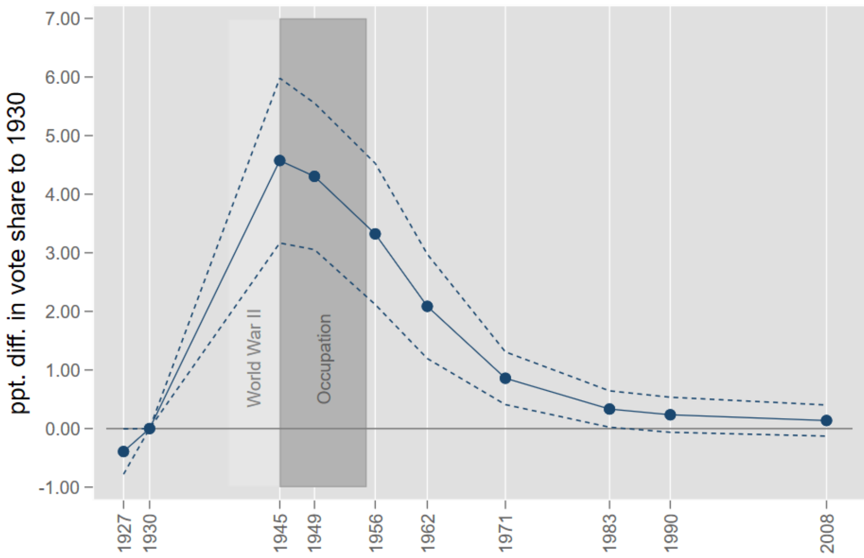
This result also adds to a larger literature on political reciprocity for economic support and opportunities, e.g. Manacorda, Miguel, and Vigorito (2011) examine the relation-

Table 1.3: Differential Effect of USIA businesses in the Soviet Zone

Specification:	**Difference-in-Differences** (1)	(2)	**Matched DiD** (3)
Pre-WWII differences			
1927 × USIA mun	-0.002	-0.004**	-0.003
	(0.002)	(0.002)	(0.004)
Base-year (1930) differences			
USIA mun	-0.000	-0.001	-0.002
	(0.001)	(0.001)	(0.003)
Post-WWII differences			
1945 × USIA mun	0.042***	0.046***	0.048***
	(0.006)	(0.007)	(0.008)
1949 × USIA mun	0.041***	0.043***	0.046***
	(0.006)	(0.006)	(0.008)
1956 × USIA mun	0.032***	0.033***	0.037***
	(0.005)	(0.006)	(0.007)
1962 × USIA mun	0.020***	0.021***	0.023***
	(0.004)	(0.005)	(0.005)
1971 × USIA mun	0.008***	0.009***	0.009***
	(0.002)	(0.002)	(0.003)
1983 × USIA mun	0.003**	0.003**	0.005*
	(0.001)	(0.002)	(0.003)
1990 × USIA mun	0.002	0.002	0.004
	(0.001)	(0.002)	(0.003)
2008 × USIA mun	0.001	0.001	0.003
	(0.001)	(0.001)	(0.003)
Dem. Controls	No	Yes	Yes
No. unique municipal.	866	646	226
No. periods	23	23	23
No. observations	17,900	13,949	4,878
R-squared	0.28	0.29	0.40
Mean of dep. var.	0.01	0.01	0.01
S.d. of dep. var.	0.02	0.03	0.03

Notes: In this table the dependent variable is the Communist Party vote share, the specification above follows equation (3). The method of estimation is least squares. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Figure 1.8: Differential Effect of USIA businesses in the Soviet Zone



Notes: This graph shows the effect of the presence of Soviet "USIA" businesses on the Communist Party vote share in municipalities in the Soviet Zone. Dashed lines show the 95% confidence intervals. Table 1.3 shows corresponding estimation results.

ship between a large anti-poverty cash transfer program and political support for the government who implement it. They find that recipients of this government spending program are 11% more likely to support the government electorally. My work contributes to this literature by looking into the effect of the USIA businesses on political support for the Communist Party. The results above point to a similar (indirect) reciprocity towards an outside political force and their domestic allies.

Other potential mechanisms may include the local effects of the Soviet occupation itself: Using data from recent research on historical encampments in the Soviet-occupied sector of Austria (LBI für Kriegsfolgenforschung 2024), I examine the effect of Soviet presence on the Communist Party vote share. I differentiate three different types: (1) internment camps for former Nazi party members, (2) refugee and POW camps and (3) camps for Soviet soldier (as part of the occupation). To analyze the impact of this different form of presence I run an analogous regression to the one above evaluating economic coercion, but rather using military encampments. The results of these estimations are reported in Table A.6. Columns 1 and 2 report the results for all camps pooled together, columns 3 and 4 report the results for Soviet military camps only (since Soviet presence is stronger at Soviet military installations vs. at other encampments). The empirical results show a small but significant effect of encampments overall. However, this sample is heavily biased towards big cities (and thus may organically be voting more left-of-center). Turning towards the sample of military encampments in particular, the pre-war controls are balanced. Here, the results become insignificant. This suggests the impact of the presence of occupation forces themselves seems less important.

Combining the two mechanisms (economic coercion and military presence) I run a regression interacting both. The results for this estimation are displayed in Table A.7. Columns 1 and 2 again restate the main results for the USIA regression an the estimation on Soviet camps. Column 3 then presents a "horse race"-type regression including both measures. Lastly, column 4 shows the interaction regression that features both measures and an interaction of the two. Generally these results confirm the notional mechanisms outlined before: The effect of the presence of USIA is both meaningful and statistically significant. These results stay robust when including the Soviet camps measure. Meanwhile the results for Soviet camps, which were on the border of significance before, lose their statistical significance and are meaningfully lower. This is in line with a mechanism through economic means, and not physical coercion.

One major additional mechanism of Soviet influence that comes to mind is *propaganda*. The recent empirical literature (e.g. Adena et al. 2015 and DellaVigna et al. 2014) shows the power of radio and newspapers in political persuasion. However, the Austrian case has a few particularities which make a differential impact along the dimensions examined above less likely: Firstly, even though radio stations were controlled by the four occupation forces respectively, political programming was very limited and broadly disliked by the local population (House of Austrian History 2023).

Secondly, the newspaper landscape was very diverse, and even the party-newspapers (circulated by all major political parties) were available nationally across the demarcation line. The Communist Party's own newspaper "Volksstimme" *(Voice of the People)* reached it's peak circulation with 120,000 papers in 1946. This was about half compared to the newspapers of the other major parties (Graber 1983). There is no detailed data available on circulation on the regional level, however topics covered touch all parts of Austria, suggesting a broad, nationwide readership.

1.5 Conclusion

This chapter sheds light on how a short-term, temporary occupation affects electoral outcomes, using the Allied Occupation of Austria as a quasi-natural experiment. Following World War II, Austria was divided in different occupation zones by the Soviet and Western Allies. After ten years with different occupation authorities and institutions in place, a sovereign nation state was restored. Identification exploits the border between the Soviet and Western Allied sectors as a discontinuity. I first show that the Soviet occupation increased vote shares for the Communist Party in municipalities close to the occupation zone border. Then I provide novel evidence that this effect might be partially driven by the presence of Soviet businesses and their impact on vote shares as a workplace for the local population (potentially through electoral reciprocity).

Further research is needed to uncover the detailed channels which drive these local differences. In particular, there is anecdotal evidence which suggests that these establishments offered more amenities and better pay to their workers compared to Austrian-owned ones (Klambauer and Bezemek 1983). However, data on these issues is scarce. Thus, further archival research and empirical work should focus on wages, hours worked and amenities at the workplace to pin down the exact mechanism.

2 The Long-Term Effects of Military Occupations: Evidence from Post-World War II Austria*

(with Christoph Eder and Martin Halla)

Abstract

What are the long-run economic consequences of a violent and extractive military occupation? We study the post–World War II occupation of Austria, where the country was divided into Soviet and Western Allied zones for ten years before reunification. Using a spatial regression discontinuity design combined with a difference-in-differences approach, we show that areas initially occupied by Soviet forces remain significantly less economically developed today. These regions are less populated, host fewer and lower-paying jobs, and exhibit substantially higher rates of out-commuting than comparable areas in the former Western zones. We provide evidence that a large population flight triggered by the Soviet advance reshaped the spatial distribution of economic activity, persistently changing local agglomeration forces and generating persistent regional differences. Our findings highlight how the violent onset of foreign occupation can shift the spatial equilibrium of economic activity and leave long-lasting economic consequences.

2.1 Introduction

In the aftermath of World War II (WW2), Austria was divided into zones governed by Allied powers with sharply different policies. While the Western zones experienced relatively inclusive governance and economic reconstruction, the Soviet zone was marked by violence at the onset of occupation and a more extractive economic regime. We study how this episode continues to shape local economic conditions today. We show that, despite the temporary nature of this division, regions exposed to Soviet occupation continue to exhibit persistently weaker economic performance, with gaps that have remained remarkably stable over time.

When Allied forces entered Austria at the end of WW2, they divided the country into four occupation zones administered by the United States, the Soviet Union, the United

* For helpful discussion and comments (also on earlier versions of this chapter), we would like to thank Miklos Koren, Christian Ochsner, Claudia Steinwender, Andrea Weber, and participants at the U Innsbruck (10th eeecon workshop and the ISW-Workshop), BeNA (in Berlin), NOeG 2015 (in Klagenfurt), Ski-Labor Economics Workshop (in Landeck-Zams), VfS 2017 (in Vienna), WU Wien (Workshop of the DFG Priority Program 1764), Masaryk University (in Brno), U Bozen (2nd Applied Microeconomics Workshop), U Vienna (Applied Economics Seminar), Central European University (Brown Bag Seminar), U Trento (School of International Studies). Alexander Artner, Philipp Henning, Michal Lapinski, and Rene Wiesinger provided excellent research assistance. This is a completely revised version of the *IZA Discussion Paper* No. 10095, entitled *"The Long-lasting Shadow of the Allied Occupation of Austria on its Spatial Equilibrium"*, published originally in July 2016.

Kingdom, and France.[9] Although Austria remained formally a single political entity, local conditions diverged sharply across occupation zones from the outset. In the Western zones, the occupying authorities restored basic civil order, avoided large-scale appropriation, and established relatively predictable administrative structures. In contrast, the Soviet advance and early occupation were marked by widespread violence, forced requisitioning, looting, and the rapid takeover of industrial assets and infrastructure. This divergence constituted an immediate institutional shock: Property rights were undermined, assets were seized, and economic activity was disrupted in ways fundamentally different from the Western zones.

We interpret the Allied occupation as a natural experiment: Previously similar Austrian regions were exposed to sharply different institutional conditions while remaining part of the same national territory and sharing a common pre-war trajectory. These differences were temporary, as Austria regained full sovereignty in 1955 and a unified institutional framework was restored.[10] This setting allows us to address two key questions. First, can a short-lived but violent institutional shock generate persistent differences in regional economic development? Second, through which mechanisms do such shocks translate into long-run spatial divergence? Our contribution is to show that even temporary institutional shocks can have lasting effects when they induce selective migration and trigger agglomeration dynamics.

Population mobility across zones was possible in the early phase of the occupation, and the violent onset of Soviet control triggered a large internal migration shock that constitutes a key mechanism in our setting. When Soviet forces entered Austria in March 1945, many residents of areas assigned to the Soviet zone sought to avoid exposure to Soviet troops. Contemporary accounts document widespread fear of violence and expropriation, reinforced by wartime propaganda and reports from Eastern Europe. Historical evidence confirms substantial violence during the early phase of the occupation, including large numbers of sexual assaults (Stelzl-Marx 2012; Johr and Sander 2002), whereas misconduct by Western Allied troops appears to have been much less frequent. Using newly compiled census data, we provide quantitative evidence on this migration response. Prior to the introduction of strict travel restrictions, a substantial share of the population in Soviet-assigned areas moved westward, generating a sudden and spatially concentrated reallocation of population and economic activity.

The subsequent occupation regimes reinforced this initial divergence. The Soviet occupation was characterized by the seizure of industrial assets and the dismantling of infras-

9. To date, there is considerable evidence on the harmful effects of war (see, for instance, Miguel and Roland 2011), civil war in particular (Ghobarah, Huth, and Russett 2003; Guidolin and La Ferrara 2007; Chamarbagwala and Morán 2011), and terrorism (Abadie and Gardeazabal 2003; Eckstein and Tsiddon 2004). In contrast, there is little research on the economic effects of military occupation. We are aware of only one cross-country study of the impact of military occupation on economic growth. Vishwasrao, Schneider, and Chiang (2019) shows that transformative occupations (i.e., those aimed at establishing institutions that promote stability) can have significant positive effects on long-term growth.

10. Acemoglu, Johnson, and Robinson (2005) discuss the case of Korea, where institutional differences persist. In contrast, Austria's division was temporary and fully reversed, allowing us to isolate the long-run effects of a short-lived institutional shock.

tructure as reparations, whereas the Western Allies pursued policies aimed at restoring economic activity and administrative stability. Although these institutional differences were temporary, their direct effects were concentrated in the immediate post-war period, when occupying powers exercised the greatest control. Over time, Austria was increasingly treated as a prospective sovereign state, with administrative responsibilities gradually transferred to domestic authorities (Stelzl-Marx 2012).

The persistence of these effects is consistent with a mechanism combining selective migration and agglomeration forces. The initial population reallocation altered the spatial distribution of economic activity, leading to lasting differences in local economic conditions. Regions that lost population and economic mass experienced weaker recovery, while regions that attracted inflows benefited from cumulative advantages. We show that even a short-lived but violent institutional shock can generate persistent regional divergence when it induces migration and triggers agglomeration dynamics. More broadly, our findings highlight how such shocks can shift the spatial distribution of economic activity in ways that persist long after formal institutions are restored.[11]

Our analysis combines newly compiled regional data from multiple censuses spanning the period before, during, and after WWII. We focus on municipalities located near the demarcation line established on July 1, 1945, which separated the Soviet and Western occupation zones (see Panel B of Figure 2.1). Our long-run data, dating back to 1900, shows that regions on both sides of this border followed parallel development paths prior to WWII, supporting the view that the placement of the demarcation line was plausibly exogenous to pre-existing economic trends. To estimate the impact of the occupation, we combine a Difference-in-Differences framework with a spatial Regression Discontinuity Design and trace regional development over time.

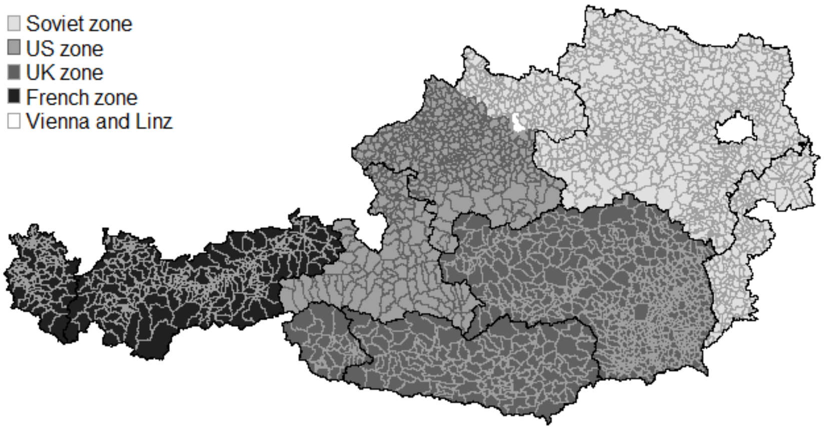
We first examine how the occupation affected the spatial distribution of population using Austrian census data from 1900 to the present, where population serves as a proxy for local economic activity. We then extend the analysis to more detailed postwar measures, including local employment, the number of firms, and commuting flows. These data allow us to distinguish between places of residence and places of work, providing a more precise picture of the spatial organization of economic activity.

Our findings are threefold. First, the onset of the Soviet occupation triggered a large internal migration shock: Between 1945 and 1946, the population in the Soviet zone declined sharply as residents fled westward. Second, this shock proved highly persistent. Even 66 years later, in 2011, the population gap between former Soviet and Western areas remains almost identical to the initial drop observed in 1945–46. Third, this divergence translated into increasingly large differences in economic activity. Local employment in the former Soviet zone was about 13 percent lower in 1961 compared to the Western zone, and the gap widened to roughly 28 percent by 2011, indicating a growing concentration of economic activity in regions formerly occupied by the Western Allies.

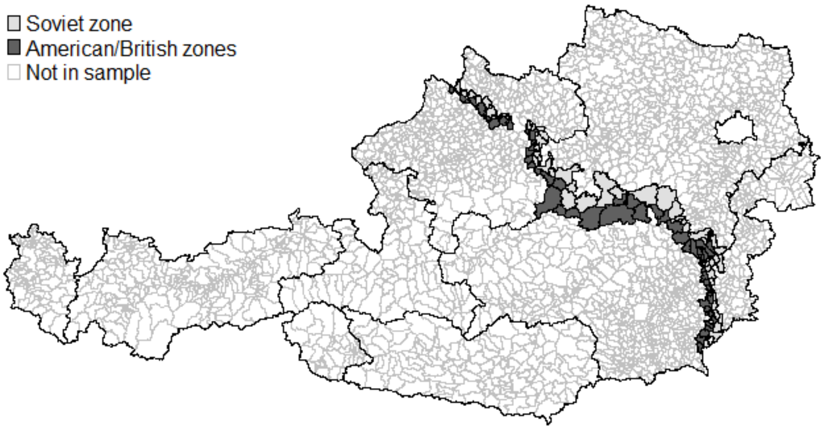
11. This has been a central issue in policy debates on military intervention and occupation in recent decades; see, for example, Bischof (2020).

Figure 2.1: Occupation Zones and Bordering Municipalities

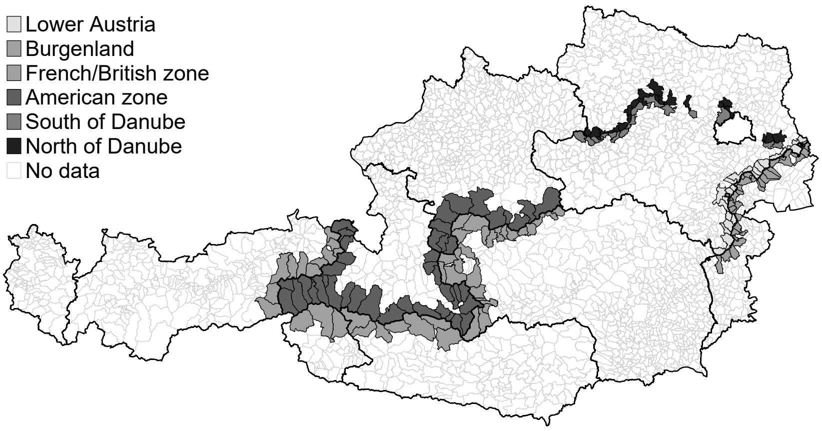
Panel A: Occupation Zones (1945–1955)



Panel B: Municipalities at the Zone Border



Panel C: Placebo Zone Border(s)



We interpret these persistent differences as the outcome of agglomeration forces. The migration shock created a lasting divergence in local population density, leading to a concentration of economic activity in the more populous Western regions. Over time, these areas benefited from cumulative advantages in employment, firm location, and productivity. Evidence from commuting patterns supports this mechanism: A substantial share of workers residing in the former Soviet zone commute to jobs located in the former Western zone, indicating that employment opportunities are disproportionately concentrated there. In this sense, the initial population shock shifted the spatial distribution of economic activity in a persistent way.Consistent with this mechanism, we also find direct evidence of productivity differences. Workers employed in the former Soviet zone earn roughly three percentage points less than comparable workers employed in the former Western zone. This wage differential remains remarkably stable over time and is consistent with sustained productivity differences between the two regions.

We contribute to several strands of the literature by linking two areas that have largely developed separately: Forced migration and spatial persistence.

First, we relate to the growing literature on the long-run effects of forced migration and population shocks on local economic development. Recent studies exploit large-scale displacement during and after World War II to show that sudden changes in population distribution can have persistent economic effects. For example, refugee inflows and expulsions have been shown to affect manufacturing employment, income, and regional development patterns in settings such as West Germany, Hungary, and Czechoslovakia (Peters 2022; Borbely and Mckenzie 2024; Testa 2021). Other work highlights persistent impacts on local fiscal policy and regional GDP arising from variation in refugee settlement across occupation zones (Chevalier et al. 2024; Ciccone and Nimczik 2022). Importantly, these studies examine settings characterized by prolonged occupation periods or sustained post-war adjustments, whereas we focus on a short-lived but intense shock associated with the onset of Soviet occupation.

Our study contributes to this literature by identifying a large internal migration shock induced by the onset of the Soviet occupation in Austria in 1945. While existing studies typically examine the effects of refugee inflows or expulsions, we document how fear-induced internal migration triggered by the violent onset of military occupation reshaped the spatial distribution of population and economic activity within a country.

Second, our results speak to the literature on agglomeration and spatial persistence. A key insight from this literature is that temporary population shocks can have lasting effects due to increasing returns and local externalities. Schumann (2014) shows that the resettlement of German expellees led to highly persistent population changes, consistent with multiple equilibria. While Schumann finds limited effects on economic activity, our results suggest that in Austria the initial population shock translated into increasing divergence in employment and productivity over time, indicating a shift in the spatial equilibrium of economic activity.

Third, we contribute to the broader literature on the long-run consequences of war, occupation, and historical shocks. A large body of work documents persistent economic and political effects of conflict, terrorism, and political violence (Miguel and Roland 2011; Ghobarah, Huth, and Russett 2003; Abadie and Gardeazabal 2003; Eckstein and Tsiddon 2004; Besley and Mueller 2012). More recent contributions show how wartime experiences shape long-run identity, trust, and political preferences (Dehdari and Gehring 2022; Munroe et al. 2023; Fontana, Nannicini, and Tabellini 2023; Cannella, Makarin, and Pique 2024).

Fourth, our chapter relates to the literature on institutions and long-run development, which emphasizes the persistent effects of extractive institutions through mechanisms such as elite capture, cultural transmission, and institutional continuity (Acemoglu, Johnson, and Robinson 2001, 2002; Dell 2010; Banerjee and Iyer 2005; Becker et al. 2016; Acemoglu et al. 2011). While this literature largely focuses on developing or colonial contexts, we study an externally imposed institutional shock within a developed European country over a long horizon.

Finally, our chapter is closely related in context to Ochsner (2023), who studies the short-lived Soviet occupation of parts of Styria and document persistent effects driven by the reallocation of skilled labor. We extend this perspective by providing the first nationwide analysis of the Allied occupation of Austria, combining long-run census data with detailed labor market information to document persistent regional divergence and identify agglomeration forces as the key mechanism.

The rest of this chapter proceeds as follows. Section 2.2 reviews the historical background of the Allied occupation of Austria after WWII and describes the nature of the impact on the economy. Section 2.3 describes our research design. We introduce our data, present descriptive statistics, and outline our estimation strategy and the underlying identifying assumptions. Section 2.4 tests whether the differential occupation had an impact on population since 1955. In Section 2.5 we move to the impact on economic activity, such as workers, firms, and commuting streams. Section 2.6 explores mechanisms of persistence. Section 2.7 offers concluding remarks.

2.2 Institutional Background

2.2.1 The Allied Occupation of Austria after WWII

Already in October 1943 the major Allies, the United Kingdom, the United States, and the Soviet Union, began coordinating their planning for the political order of post-war Europe. These early consultations reflected the growing Allied awareness that the collapse of Nazi rule would leave a power vacuum requiring immediate administrative control, as well as long-term decisions about reconstruction, political stability, and the restoration (or creation) of sovereign states. Most importantly for Austria, the foreign secretaries agreed in the so-called *Moscow Declaration* that Austria had been the first victim of the aggressive foreign policy of Nazi Germany.[12] The Allies declared *"the*

12. The validity of this so-called *victim theory* has been questioned ever since. Historians, politicians, and the public in Austria have debated whether the *Anschluss* was voluntary or forced. Today there is

annexation imposed upon Austria by Germany's penetration of March 15, 1938, as null and void" and committed themselves to the re-establishment of a free and independent Austrian state once Nazi Germany had been defeated.

Beyond this political statement, the *Moscow Declaration* also created a legal and moral framework for post-war Allied action. As emphasized by Erickson (1950), the declaration obligated the occupying powers to regard Austria not merely as defeated territory, but as a political entity whose sovereignty was to be restored rather than created. This distinction fundamentally shaped the later debates within the Allied machinery, especially regarding the degree of control, the division of responsibilities, and the timetable for political normalization. At the same time, the declaration contained an important caveat: Austria was held responsible for participation in the war on the side of the Third Reich *"to the extent of her contribution,"* reflecting the delicate balance between the victim narrative and the recognition of widespread complicity. The tensions between these goals would later resurface during the long and often obstructed negotiations of the State Treaty, as documented by Ferring (1968).

Establishment of Occupation Zones

In late 1943 and early 1944, the Allies began developing concrete principles for the future occupation of both Germany and Austria. The task of producing a detailed zoning proposal was referred to the newly established *European Advisory Commission (EAC)*, which began formal planning in January 1944. According to the final version of the *Agreement on Control Machinery in Austria*, the primary purposes of the occupation were the separation of Austria from Germany, the establishment of a central Austrian administrative authority, the preparation for free elections, and the provision of provisional governmental structures.

The major Allies submitted unilateral proposals to the EAC as early as January 1944, prompting lengthy and often contentious negotiations. The *London Protocol* of September 1944 codified that Germany would be occupied and divided into three zones, although the demarcation line between the Western Allies remained unresolved. The boundaries of the Soviet zone, however, had been defined earlier and remained relatively fixed. At the *Yalta Conference* in February 1945, the Allies agreed that France would join as a fourth occupation power; its zone in Austria was to be carved out of American and British allocations. The capitals – Vienna and Berlin – would be jointly administered by all four Allies through an Allied Control Council.

Despite these early agreements, the final months of the war produced military front lines that did not correspond to the planned zoning. Soviet forces captured Vienna and pushed further west and south than anticipated, particularly in Styria, while gaining less ground than intended in northern Upper Austria. France formally entered the Austrian zoning negotiations only in January 1945 and immediately demanded its own zone, greatly complicating the already protracted discussions.

absolute consensus that the *Anschluss* found broad support in the Austrian population at the time and that a large proportion of Austrians were collaborators and co-perpetrators.

An agreement on the Austrian occupation zones was finally reached on July 9, 1945, three months after the country fell to Allied armies. Immediately thereafter, a large-scale zone swap was carried out, substantially modifying the earlier allocations.[13] Erickson (1950) notes that these changes were driven by strategic, logistical, and administrative considerations, as well as by inter-Allied bargaining power in the immediate aftermath of the military campaign. Some regions – especially parts of Upper Austria and Styria – experienced only very brief occupation by forces that later withdrew to comply with the final settlement.[14]

A summary of the final agreement was released simultaneously by the four governments on August 8, 1945. The zoning map (Figure 2.1) shows the borders used in our analysis: The Soviet Union received the northeast, the United States the northwest, France the southwest, and the United Kingdom the southeast. Vienna was similarly subdivided, but its central district was administered jointly by the Allied Control Council. As Erickson (1950) underlines, this arrangement, unique among occupied capitals, was designed to symbolize the unity of the Four Powers in administering Austria.

Our empirical analysis relies on these final boundaries. To the extent that temporary exposure to a different occupation force generated persistent effects, using the final zones provides a conservative estimate and thus a lower bound.[15] Approximately 40% of the border municipalities (and roughly 30% of the population in these areas) experienced at least short-term occupation by forces that did not ultimately administer the region. In Panel A of Figure 2.2 we show the regions affected by this issue.

Finally, although the occupation of Austria was conceived as temporary, Ferring (1968) shows that Cold War tensions quickly permeated the negotiations for the Austrian State Treaty. Talks that began in 1947 stalled for years, were suspended from mid-1950 to 1953, and resumed only after the geopolitical thaw of 1954. The Austrian State Treaty, signed on May 15, 1955, restored full sovereignty by July 27, 1955, and Allied troops withdrew from Austrian territory on October 25, 1955 – ending one of the most protracted yet least violent occupations of the early Cold War.

Phases of the Occupation

The time period from the invasion of the Soviet troops in the end of March, 1945 until the first agreement on the occupation zones in early July, 1945 was marked by chaos. Everything depended on the military administration that had been installed. Conditions differed not only among the occupation zones to be, but also with respect to the

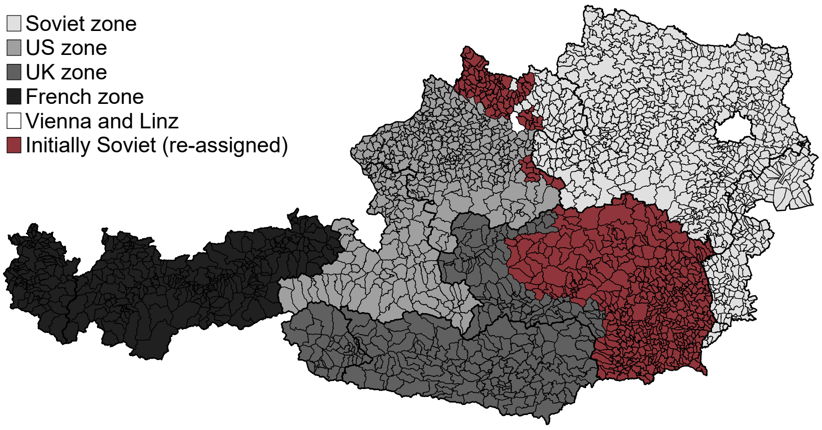
13. Among others, the UK took over most of Styria from the Soviets and the Americans; the Soviets replaced the Americans in the north of Upper Austria (the so-called Mühlviertel); and France received Tyrol, which had initially been assigned to the US. See Panel A of Figure 2.2 for detailed graphics.

14. As mentioned above, Ochsner (2023) explores the effect of this temporary Soviet occupation and subsequent retreat to the pre-determined zone borders.

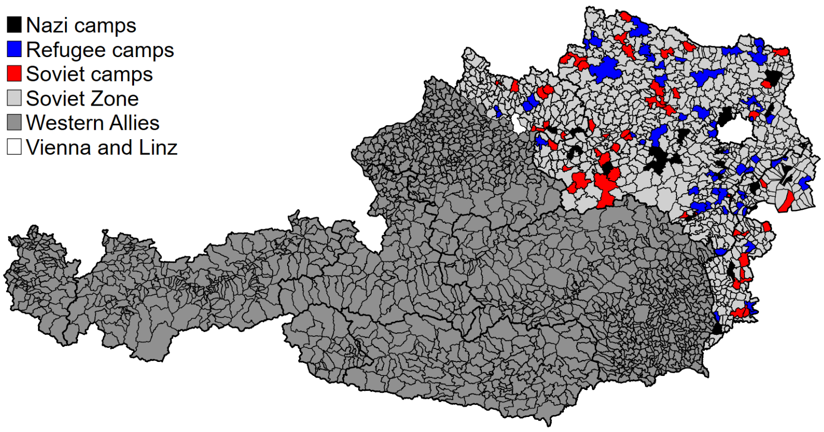
15. Ochsner (2023) shows substantial long-run differences in population and economic outcomes along the zone borders within Styria, where the Soviet were present for just 74 days. However, since both studies are using local estimation approaches, these effects are not interpretable one-to-one. See section 2.4.3 on robustness for details.

Figure 2.2: Zone Swaps and Soviet Influence

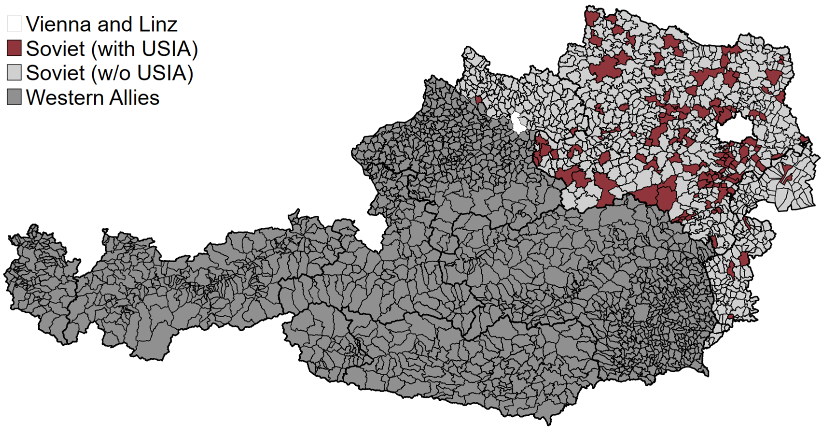
Panel A: Zone Swaps (in Red)



Panel B: Camps in the Soviet Occupation Zone



Panel C: The Soviet USIA Businesses



individual division and the particular local commander (Eisterer 2009). The different military commanders shared their interest in ensuring the security of their troops as well as in maintaining law and order. In line with this, several oral historic sources report the presence of curfews and strict travel restrictions (see Roth, Kramml, and Thomas Weidenholzer, n.d. and Hafner 2005). Thus, it seems not reconstructable at which time in which region people had the opportunity to escape the Soviet zone to be. This escape was further complicated by the unclear position of the demarcation line, and the last-minute zone swaps.

The period after July, 1945 can be characterized by two regimes defined by the so-called first and second control agreement. Under the first control agreement from July 4, 1945 the occupying power had full control and traveling across occupation zones was heavily restricted. The period after the second control agreement from June 28, 1946, was characterized as a gradual emancipation of the Austrian government, which took back more and more powers from the occupiers. Already, starting from October 22, 1945 it was possible for Austrian citizens to travel across occupation zones. A so-called inter-Allied identity card was needed. The constant checking of the movement of people and goods across lines of demarcation, however, was only ceased on the June 9, 1953.[16]

The occupation lasted much longer than initially intended, since state treaty negotiations were obstructed by the emerging Cold War (Ferring 1968). The negotiations started in 1947, were in a state of suspension from mid-1950 through 1953, were resumed in 1954, and finalized in 1955. On May 15, 1955 the *Austrian State Treaty* was signed among the allied occupying forces and re-established a free, sovereign, and democratic Austria by July 27, 1955. As a result of this treaty the Allies left Austrian territory on October 25, 1955.

2.2.2 Different Occupation Regimes

A rough description of the Soviet occupation in economic terms would be "exploitative", while the non-Soviet occupation (in particular, the one by the US) could be described as more "supportive". To describe the differences between the two occupation regimes, it is useful to distinguish two periods. First, in the period shortly after the onset of the occupation (May 1945 – 1946), the population in the Soviet occupation zone was exposed to more misconduct as compared to the Western Allies. Second, for the remainder of the occupation, the Soviet zone was economically exploited, while the rest received support.

Level of Misconduct

Due to Nazi propaganda demonizing communists, as well as factual reports on misconduct of the Soviet Army in Hungary, the Austrian population was terrified by the Soviet Army. Subsequently, the seeking of revenge and craving for booty indeed led to assaults on the local population. In particular, there is evidence for mass rapes taking place in connection with combat operations, but also during the subsequent occupation (Dack

16. Within the occupation zones of the Western Allies cross-border control were ceased already in 1947.

2008). Karner (2005) provide an overview of historical sources on Allied (in particular Soviet) misconduct. Their sources estimate 70,000 to 100,000 rapes in Vienna alone (Bischof 1999) and 200,000 to 240,000 in the whole Soviet zone (Baumgartner 1995) during the onset of the occupation.[17]

Stelzl-Marx (2012) confirms that such violence peaked during the chaotic first weeks of occupation, especially in eastern Austria, where exhausted front-line units often acted with little supervision (a detailed graphic of the Soviet footprint in Austria is provided in Panel B of Figure 2.2).[18] However, Soviet troop behavior varied considerably: While some formations engaged heavily in looting and sexual violence, others, particularly units arriving later or under stricter commanders, maintained more discipline. Soviet headquarters repeatedly issued orders to curb looting and assaults, and punishments were sometimes severe, but enforcement in spring 1945 remained inconsistent. As command structures stabilized over the following months, incidents declined, though did not disappear entirely.

The diary of a nurse from Vienna (Fraller and Langnas 2010) provides insight into the perception by the (female) population: *"Russians are in the house! In all seven years, I have never trembled with fear and despair as much as I did in those hours."* By contrast, the reputation of the troops of the Western Allies, who crossed the German border in the West about one month later, was much better. While there are also documented cases of rape, the incidence seems much lower (Stelzl-Marx 2012).

Economic Exploitation versus Support

Referring to the *Potsdam Agreement*, the Soviets claimed "German assets" within their Austrian occupation zone. Between February and July 1946, they unilaterally seized roughly 280 industrial enterprises, including the entire Austrian oil industry and the *Danube Steam Shipping Company*, along with tens of thousands of hectares of productive farmland (Bischof 2009). Central to consolidating this economic sphere was the establishment of the *Administration for Soviet Property in Austria* (USIA), whose formal mandate covered enterprises previously, at least partially—owned by German firms or the Nazi regime (see Panel C of Figure 2.2 for their locations).[19] Steiner (1993) demonstrates, however, the actual scope of USIA extended well beyond a narrow reparations regime: Through aggressive interpretations of property law, unilateral decrees, and Soviet military authority, USIA developed into a parallel economic system operating largely outside Austrian jurisdiction.

In particular, Steiner (1993) shows that USIA was tightly integrated into the Soviet command structure. Although nominally a civilian trust managing "German assets," USIA

17. These estimates are considered relatively noisy as they rely on a combination of police reports and statistics on sexually transmitted diseases. The best available evidence is not for Austria, but for Germany. Using information from hospital records in Berlin, Johr and Sander (2002) estimates that in the period between April 1945 and September 1945 about 7 percent of all women of childbearing age were raped at least once by members of the Soviet army.

18. See LBI für Kriegsfolgenforschung (2024) for the detailed data collection and methodology.

19. In a companion project Hilmbauer-Hofmarcher (2025) looks at the electoral consequences of these firm-takeovers and finds short-term effects on voting for the far-left.

functioned in practice as an extension of Soviet ministries, especially those responsible for heavy industry and foreign trade. Its enterprises were grouped into "trusts" and "associations" modeled on Soviet industrial combines, with a highly centralized chain of command and a management culture oriented toward plan fulfillment rather than market efficiency. This organizational system reinforced USIA's autonomy from Austrian institutions: The Soviets claimed exclusive control over administration, production, and profits, and repeatedly rejected Austrian attempts to inspect accounts, verify ownership claims, or subject USIA to national regulatory standards.

USIA's enterprises were systematically geared toward resource extraction and income transfer to the Soviet Union, rather than toward the reconstruction of the Austrian economy. Industrial output was frequently exported eastward via Soviet-controlled channels, and profit rates in USIA companies, despite chronic under-investment, were often pushed to their maximum. Steiner (1993) highlights the strict labor regime imposed in many USIA plants: Workers reported intensified monitoring, politically motivated dismissals, and management practices that resembled Soviet factory discipline, more than Austrian labor norms. Combined with USIA's exploitation of a legal loophole to avoid Austrian taxation, resulting in an estimated 1.1 billion Schillings in foregone state revenue (Williams 2007), the Soviet zone developed into a "state within the state," economically powerful yet largely unaccountable (Steiner 1993).

In addition to its industrial operations, USIA conducted extensive commercial activities, including procurement networks, export operations, and retail sales through their "USIA shops," which stocked goods unavailable in the Austrian market. These commercial ventures further strengthened the Soviet economic enclave and served political purposes: USIA cultivated ties with Austrian politicians and business elites, disseminated propaganda, and positioned itself as a long-term actor in Austria's economic landscape (Klambauer 1978; Klambauer and Bezemek 1983; Bischof, Pelinka, and Stiefel 2000). In practice, the Soviet objective of winning Austrian "hearts and minds" remained decidedly secondary – profit extraction and political leverage dominated.

The Western Allies, by contrast, shifted toward substantial assistance in 1946, responding to a severe food crisis that overwhelmed Austria's diminished agricultural capacity. With support from the United Nations, the United States launched extensive food aid programs (Dornik 2005). One year later came the *European Recovery Program* (ERP). Austria, uniquely among Soviet-occupied states, participated fully, receiving roughly USD 1.1 billion in non-repayable aid from 1947 to 1953.[20] About 81% of ERP funds were spent in Western zones and 19% in the Soviet zone (Haas 2007).[21] Compared to the limited, extractive, and politically instrumental Soviet economic policy, Western aid represented a comprehensive reconstruction strategy.

20. 41% financed immediate needs such as food, power infrastructure, and currency reform; 59% funded medium- and long-term investments.

21. See also Chapter 6.4 in Stelzl-Marx (2012).

2.3 Research Design

We aim to estimate the effect of the differential occupation on the economic development for the period after 1955, when the demarcation line became completely obsolete. We start our empirical analysis with a RDD around this demarcation line where we use the distance as the forcing variable. We rule out pre-WWII differences by using the difference in log population as the dependent variable. However, this simple approach has the disadvantage that the two-dimensional border between occupation zones is reduced to the one-dimensional distance variable. Although this approach produces an unbiased estimate of the causal effect of the Soviet occupation, an estimation strategy that does not aggregate information of the forcing variable would be more efficient.

In our preferred estimation strategy, we thus form pairs of geographic areas along the demarcation line that share a common border, but are located in different former occupation zones. This research design is based on the idea that the difference in post-treatment outcomes can be identified by focusing on a small area around the former demarcation line. We have to overcome two challenges to identify the causal effect at this discontinuity in space. First, we should allow for the possibility of unobserved differences between areas in the two occupation zones that were already in place before the demarcation line was decided. To do so, we have collected a long data series starting in 1900. These data allow us to compare population levels and trends across regions in a period before separation. It turns out that the regions east and west to the demarcation line had been following parallel trends in population development prior to WWII. This suggests that the exact position of the demarcation line was exogenous. Motivated by these parallel trends in the pre-occupation period, we assume in our analysis that the population trends would have been parallel in the absence of the separation later on.[22]

Second, we have to be careful to rule out other time-varying confounding factors, such as differences in the proximity to Western markets in the post-WWII period. To address this, we exploit the demarcation line as a discontinuity in space. Small geographic units bordering the demarcation line have the same geographic features and equal access to markets. More generally speaking, we assume that there are no confounding factors, which change discontinuously at the demarcation line.

2.3.1 Data

Population Censuses

We use *municipality-level data* on the size of the population, different indicators for economic activity and commuting streams. These data are drawn from different sources

22. We refrain from referring to our estimation procedure as a DiD approach, since a standard DiD approach assumes that only one group was affected by the treatment. We recognize that both the East and West have been affected by the events after WWII and we aim to estimate the relative difference in population (and other outcomes).

published by *Statistik Austria* (the Austrian statistical agency) and its predecessor agencies. The vast majority of these data originates from the decennial census. These have been conducted since 1869 with irregular intervals in the inter-war period.[23] A detailed overview over these sources can be found in Appendix C in Table B.12. For the earlier years we have to resort to printed publications. For later years (1971 and onward) we have access to electronic individual-level data, which we aggregate at the municipality level ourselves. Population data is available for the full sample period from 1869 through 2011. These long panel data set allows us to check for any pre-WWII differences across regions. Other variables are only available for the post-WWII period. Information on economic activity and commuting streams is available from 1961 through 2011 and 2001, respectively.

Municipality borders have changed significantly since the beginning of our sample period. For instance, since 1934 the number of municipalities has dropped from 4,397 to 2,354 in the year 2011. In the case of population data, *Statistik Austria* provides the adjusted figures after any revision of municipality borders. Thus, we have consistent time-series based on the current municipality borders. For all other variables, we generate our own time series for the smallest geographic unit we can cleanly trace over time.[24] In our main estimation sample, which covers the area along the demarcation line (our RDD-sample, to be defined below), we end up with 95 mutually exclusive geographic units that comprise 128 municipalities according to the current borders.The detailed descriptive statistics for this sample are shown in the second panel of Table 2.1. For simplicity, we will refer to these larger geography units also as municipalities below. Of these municipalities there are 50 in the former Soviet zone and 45 in the non-Soviet zone.

Direct Economic Measures

To complement our usage of population data, we turn towards more direct economic outcomes. For these we mainly use data from the Austrian Firm Census. These data are available *at the district level* between 1902 and 2011 (see Appendix Table B.13 for details). To complement our results based on population and employment as measured by the Austrian firm census, we add further evidence based on more recent population censuses that include information on commuting. Since 1961, we have information on employment at the municipality level and since 1971 we have access to a 5 percent sample of individual-level census data. These data include information on individuals' place of residence, employment status, type of employment, place of employment, and commuting behavior. In addition we use data from the *Austrian Social Security Database* (henceforth ASSD). The ASSD includes administrative records to verify pension claims and is structured as a matched employer–employee data set. Starting from 1972, we

23. For the years 1946 (Österreichisches Statistisches Zentralamt 1948) and 1948 (Österreichisches Statistisches Zentralamt 1949) we obtain information on the population from two non-census sources. In 1946, population estimates are based on the number of food stamps. In 1948, population figures are based on an administrative inquiry.

24. If municipalities have merged, we simply aggregate pre-merger data across the merging municipalities. If one municipality has been divided and it parts merged with other municipalities, we aggregate the pre-merger data across all affected municipalities.

Table 2.1: Balance Table

	Non-Soviet Zone		**Soviet Zone**		**Difference**	
	Mean	Std. Dev.	Mean	Std. Dev.	Diff.	p-value
PANEL A: Geographic Features						
Slope (mean)	12.37	9.15	12.55	8.07	-0.18	(0.92)
Elevation (mean)	577.72	327.77	542.86	276.72	34.87	(0.58)
Aspect (mean)	165.90	22.59	175.41	12.24	-9.51*	(0.01)
Ruggedness (mean)	7.59	8.02	7.02	4.98	0.56	(0.68)
Distance City	9.48	7.22	10.19	6.92	-0.71	(0.62)
Distance Capital City	44.70	34.07	40.43	30.78	4.27	(0.52)
PANEL B: Controls from Census (1934)						
Area	54.31	67.60	47.20	51.21	7.11	(0.56)
Population	2318.60	2706.06	2439.06	1990.54	-120.46	(0.80)
Share Male Pop.	0.49	0.02	0.50	0.01	-0.00	(0.15)
Share Agriculture	0.53	0.20	0.50	0.19	0.02	(0.54)
Share Industry	0.21	0.11	0.24	0.11	-0.03	(0.21)
PANEL C: Pre-War Election Outcomes						
Share SPOE	0.25	0.18	0.30	0.16	-0.05	(0.16)
Share OEVP	0.48	0.21	0.27	0.29	0.21***	(0.00)
Share Hitlerbewegung	0.01	0.02	0.02	0.02	-0.00	(0.34)
PANEL D: Neighboring municipalities (N=95) – Pre-WWII						
Population in year [...]						
1900	2106.36	2109.35	2317.74	1817.55	-211.38	(0.60)
1910	2259.60	2427.81	2428.42	1958.25	-168.82	(0.71)
1923	2290.62	2734.20	2395.40	1906.17	-104.78	(0.83)
1939	2257.31	2598.09	2381.20	2033.00	-123.89	(0.80)
PANEL E: Neighboring municipalities (N=95) – Post-WWII						
Population in year [...]						
1951	2494.18	2958.07	2368.90	2106.37	125.28	(0.81)
1961	2618.60	3228.32	2413.88	2194.99	204.72	(0.72)
1971	2800.36	3508.68	2579.66	2338.14	220.70	(0.72)
1981	2897.40	3822.03	2635.90	2388.53	261.50	(0.69)
1991	2998.93	3991.35	2721.84	2478.57	277.09	(0.68)
2001	3054.07	4070.48	2808.66	2666.65	245.41	(0.73)
2011	3079.31	4423.05	2811.68	2725.43	267.63	(0.72)
Observations	45		50		95	

observe for each worker basic socio-economic information and on a daily basis employment along with her occupation. Information on earnings is provided per year and per employer.[25]

Additional Outcomes

In a later analysis on the mechanisms, we also make use of other administrative records, in particular Habsburg-era education records from the year 1900 on the district level. In addition we use survey results on trust towards others on the district level from the *Austrian Gender & Generations Survey (GGS)*.

2.3.2 Estimation Strategy

The core idea of our main estimation strategy is to exploit the demarcation line (i. e., the later inner Austrian border between 1945-1955) as a discontinuity in space. This lends itself to an RDD, in which the distance to the demarcation line serves as the running variable. We implement the regression discontinuity approach in two different ways. The first one employs the distance to the demarcation line as the running variable, which translates into the following estimation equation:

$$\begin{aligned} Y_{i,t} &= \alpha + \beta_t \cdot Soviet_i + \gamma_t \cdot Distance_i \\ &+ \delta_t \cdot (Distance_i \times Soviet_i) + \phi_t + \varepsilon_{i,t}, \end{aligned} \tag{2.1}$$

where $Y_{i,t}$ is the outcome variable of interest in geographic area i measured in year t. The binary variable $Soviet_i$ is equal to 1 if the area belongs to the Soviet zone, and 0 otherwise. $Distance_i$ measures the distance of the centroid of a geographic area to the demarcation line in kilometers. ϕ_t is a time fixed effect.

Note that we allow the estimate of the effect of the Soviet occupation β_t to vary by year. The estimates of β_t give therefore the average difference between a geographic area in the non-Soviet to one in the Soviet occupation zone in year t relative to the baseline year of 1939. The estimates of β_t for years before WWII act as a test of the parallel trend assumption. We interpret β_t for year after WWII as the effect of the Soviet occupation.[26]

An obvious drawback of a RDD with distance as the running variable is the mismatch between a one-dimensional running variable in a two-dimensional plane. Our preferred approach accounts for the two-dimensionality of space in a simple but effective way. We focus on the sample of municipalities that border the demarcation line highlighted in the middle Panel of Figure 2.1. Among these, we form pairs of areas that share a

25. The limitations of the data are top-coded wages and the lack of information on (contracted) working hours (Zweimüller et al. 2009).

26. The estimate β_{1939} is the average difference in the outcome variable between areas in the two former occupation zones in 1939.

common border (which is the demarcation line).[27] For each of these pairs we calculate the difference in the population level for each year and compare the mean of the differences over time. We build on the empirical strategy pioneered by Acemoglu, García-Jimeno, and Robinson (2012) – and farther Dell (2010), who exploit local–municipality pairings to isolate regional heterogeneity in exposure to key shocks. Adapting this approach to our setting allows us to compare otherwise similar local units that differ only in their treatment status. This approach translates into the following estimation model:

$$O_{i,j,t} \quad = \quad \alpha + \beta_t \cdot Soviet_{i,j} + \phi_{j,t} \cdot Area\text{-}Pair_j + \varepsilon_{i,j,t}, \qquad (2.2)$$

where $O_{i,j,t}$ is the log population in municipality i, belonging to pair j, measured in year t. The binary variable $Soviet_{i,j}$ is equal to one if the municipality is in the Soviet zone, and zero otherwise. The estimate of $\phi_{j,t}$ denotes a time-varying fixed-effect for municipality-pair j in year t. These are quite powerful controls, since they account for all time-varying factors that affect the population levels of bordering municipalities on both sides of the former demarcation line. By construction, many municipalities along the demarcation line appear in several area-pairs. Therefore, we cluster standard errors by municipality within a pair. We have six data points before WWII (1900, 1910, 1923, 1934, 1939), three during WWII (1943, 1944, 1945), three during the occupation period (1946, 1948, 1951) and six after the establishment of the new state (1961, 1971, 1981, 1991, 2001, 2011). Data for 1943 through 1945 are only available at the district level. The year 1939 serves as the base year in all our estimations.

The parameters of primary interest are the β_t. These parameters provide the average difference between the population of a municipality in the Western-occupied zone to one in the Soviet zone in a given year t relative to the baseline year of 1939. Estimates of β_t for years before WWII test for differential pre-occupation trends and provide suggestive evidence for the parallel-trend assumption. Estimates of β_t post WWII show at what point in time the population gap arises and how it has developed over time. Any difference reflects the differential impact between the extractive occupation by the Soviets and the more supportive occupation by the Western Allies. The estimate β_{1939} is the average difference in the outcome variable between municipalities in the Soviet zone and the West in 1939.

In summary, we aim to estimate the effect of differential occupation on economic development for the period after the Allied withdrawal from Austria. Our research design is based on the idea that the difference in post-treatment outcomes can be identified by focusing on a small area around the former demarcation line. First, we allow for the possibility of unobserved differences between areas in the two occupation zones that existed before the demarcation line was decided. Second, we must be careful to rule

27. Since a municipality can have several neighboring municipalities in the other occupation zone, a municipality may enter the sample multiple times as a member of a different pair. Appendix Figure B.1 depicts this example. Municipality A borders two municipalities (B and C) located in the other occupation zone. Thus, municipality A enters the sample twice: once in a pair with municipality B and another time paired with municipality C.

out other time-varying confounding factors, such as differences in proximity to Western markets in the post-World War II period. To do this, we use the demarcation line as a spatial discontinuity. Small geographic units bordering the demarcation line have the same geographic characteristics and the same access to markets.[28]

2.4 Main Results

2.4.1 The Effect of the Soviet Occupation on Population

Stylized Facts and Regression Discontinuity Design

Figure 2.3 shows indicative graphs of the effect of the Soviet occupation in a RDD with the distance to the demarcation line as the running variable. The dependent variable is the difference in log population between three different points in time and 1939. We restrict the sample to municipalities in Austria with a distance of at most 40 kilometers to the demarcation line.

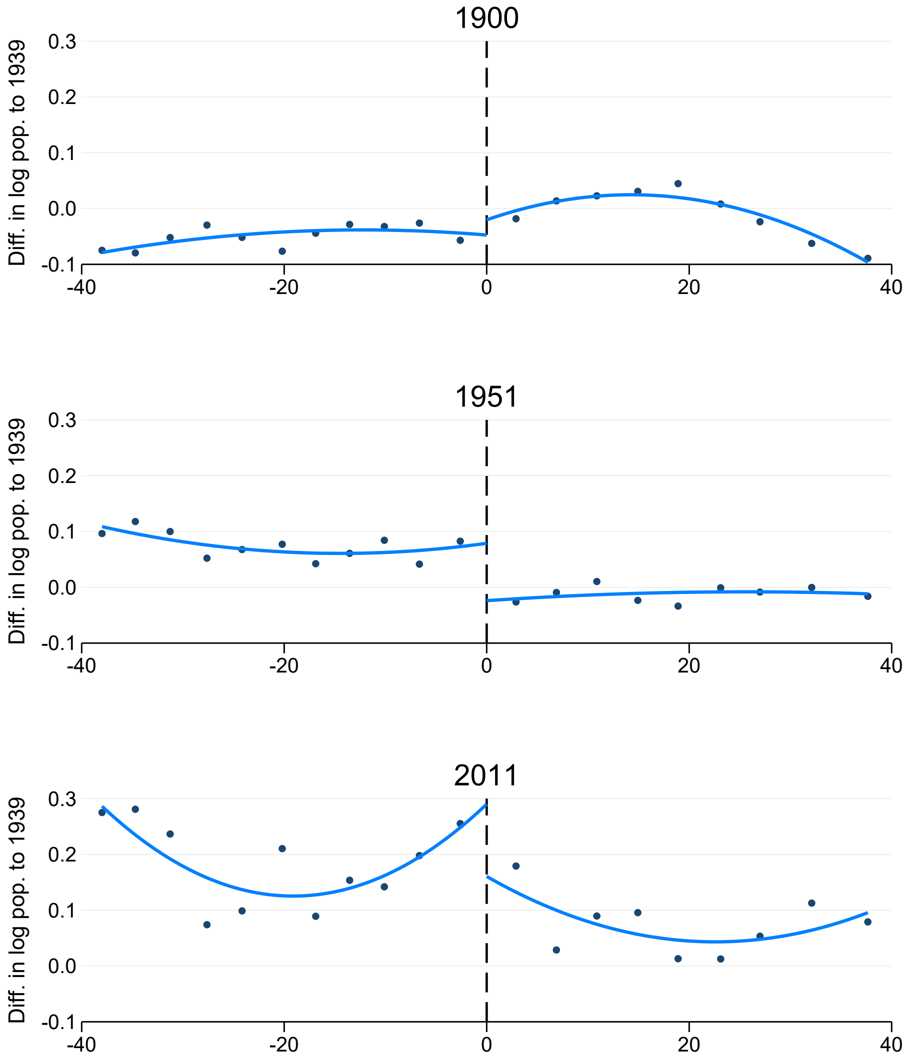
The upper figure shows the difference in log population between 1900 and 1939. Population data come from the censuses of 1900 and the combined census in 1939. We show the pre-WWII population growth as support for the identifying assumption of parallel trends. There is no obvious differences between the two sides of the demarcation line. This finding backs the interpretation of any post-WWII findings as a causal effect. The middle figure shows the short-run effect of the Soviet occupation on population growth when the Soviet occupation was still in place. There is a clear difference between the Soviet and non-Soviet zones. The population growth in the non-Soviet zones is higher by about 10 percent. The population growth is also fairly stable further away from the demarcation line. We show the The long-run effect in the lower figure in 2011. The figure shows more variation (a difference of about 14 percent) in the population growth rates in both zones, especially around the cutoff.[29]

The results from the RDD framework with a one-dimensional forcing variable show that the Soviet occupation had a negative effect on population growth in the short-run. The long-run effects point in the same direction, but are often imprecisely estimated and depend on the specification of the functional form. For that reason, we employ an estimation framework in the next section that uses the discontinuity in space in a more efficient way and non-parametric way.

28. One approach could be to use a "difference in discontinuity" (DiDisc) design which combines features of regression discontinuity design (RDD) and difference-in-differences (DiD), see (Grembi, Nannicini, and Troiano 2016; Butts 2023). It compares changes in outcomes at a cutoff point across two groups (e.g., treatment and control) or time periods, helping to isolate causal effects by controlling for confounding trends. However, it does not address local variation in e.g. market access.

29. These figures only suggest an effect of the Soviet occupation, we estimate the effect of the soviet occupation in a RDD setting with quadratic distance measures at the shown points in time. These estimates are reported in Figure 2.3.

Figure 2.3: RDD Estimation of the Effect of the Soviet Occupation on Population



Notes: The Soviet occupation zone is depicted on the right side of the graphs. The respective RD estimates are 0.040 (std. error: 0.037, p-value: 0.284) for 1900, -0.105 (std. error: 0.023, p-value: 0.000) for 1951 and -0.139 (std. error: 0.082, p-value: 0.093) for 2011.

Neighboring Pairs

Table 2.2 summarizes the estimation results for the population size based on equation (2.2). Panel A of Figure 2.4 provides a graphical representation of our preferred specification. The main result is a large and statistically significant decrease in the population of the former Soviet zone between 1939 and 1946, which persists over time.

Columns (I) to (V) are based on our main estimation sample, which comprises the municipalities along the demarcation line. In column (I), we present a parsimonious specification using only pair-year fixed-effects. Column (II) adds economic and political controls measured in the year 1930, column (III) adds geographic controls, and column (IV) adds both. Lastly, in column (V) pairs along the Danube river are omitted. We find robust results across specifications. First, the included leads are neither individually nor jointly statistically significant. This strengthens our confidence that the parallel trend assumption, which is at the core of our identification strategy, holds. Second, the population dropped in the East (as compared to the West) between 1939 and 1946 by approximately 9 percent. Over the next six decades, this gap remained fairly constant or increased, depending on the specification.[30]

This can best be seen by examining Panel A of Figure 2.4, which shows our preferred specification from column (I). We conclude that the population size in the Soviet zone is on average about 11 percent smaller (as compared to the non-Soviet zone) in the post-WWII period. In the next step, we are going to narrow down the period during which this decline occurred and discuss what caused this to happen.

Onset and Cause of Population Decline

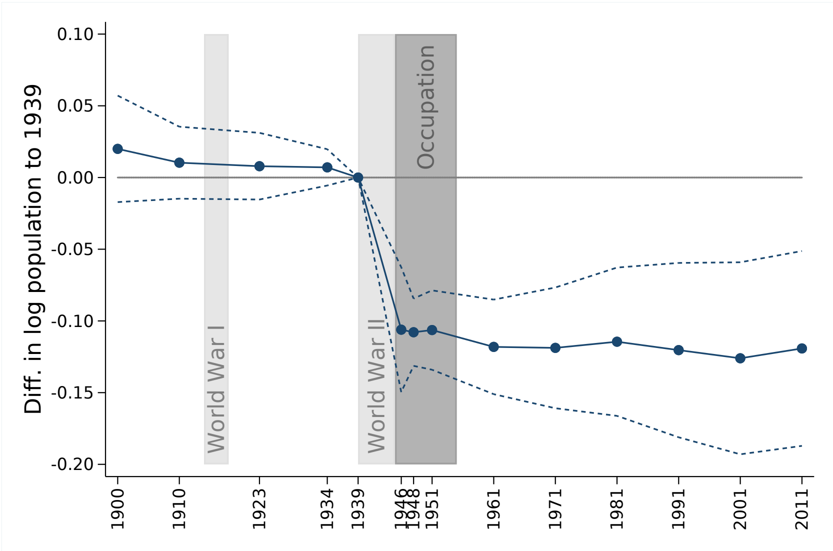
For a correct interpretation of the population decline in the former Soviet zone, it is crucial to explain the timing and nature of this event. To provide evidence on the exact timing, more observations in the period between 1939 and 1946 would be needed. Unfortunately, to the best of our knowledge, there are no other municipal-level data available for the WWII period. However, we have found district-level data for the years 1943, 1944, and 1945. Based on these additional data, we reran our specification at the district-level along the demarcation line. Panel B of Figure 2.4 shows the results. We see that the population decline has already occurred, between the first quarter of 1945 and the fall of 1946. The most plausible explanation for such as sharp drop is an migration stream from East to West.

This interpretation is consistent with what we know from oral history about the final period of WWII. Several sources document that the Austrian population was terrified of the Soviet army and wanted to avoid any encounter with it. This fear was caused by Nazi propaganda demonizing communists, as well as factual reports on misconduct of

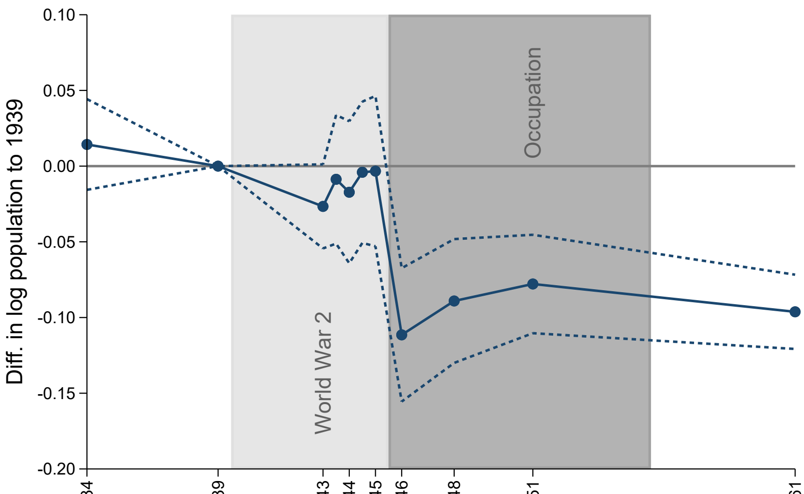
30. As noted above, a lot of municipalities along the demarcation line appear in several area-pairs. Therefore, we cluster standard errors by municipality within a pair. In Appendix Table B.1, we demonstrate the robustness of our estimation results with respect to different approaches of inference using different levels of clustering and synthetic controls.

Figure 2.4: Estimation of the Effect of the Soviet Occupation on Population

Panel A: Soviet Zone Border (Municipality level)



Panel B: Soviet Zone Border (WWII – District level)



Notes: The graph in Panel A shows the effect of the Soviet occupation on population in bordering municipalities along the demarcation line. Column (I) of Table 2.2 summarizes the corresponding estimation results for the population size based on equation (2.2). The graph in Panel B shows the effect of the Soviet occupation on population in bordering districts during and after WWII. The specification is estimated analogously to the one above, only using district pairs (shown in Column (VI) of Table 2.2). Dashed lines show the 95% confidence intervals.

Table 2.2: Estimation of the Effect of the Soviet Occupation on Population

	Log Population				
Sample definition:	Bordering municipalities				Bordering
	Baseline	Dem. Controls	Geo. Controls	All Controls	Districts
	(I)	(II)	(III)	(IV)	(V)
Pre-WW2 differences					
1900 × Soviet zone	0.020	-0.018	0.014	-0.024	
	(0.021)	(0.029)	(0.028)	(0.029)	
1910 × Soviet zone	0.010	0.005	0.015	-0.000	
	(0.016)	(0.024)	(0.019)	(0.020)	
1923 × Soviet zone	0.008	-0.006	0.015	-0.007	
	(0.014)	(0.016)	(0.015)	(0.016)	
1934 × Soviet zone	0.007	0.002	0.008	0.003	0.014
	(0.008)	(0.009)	(0.009)	(0.010)	(0.015)
Base-year (1939) differences					
Soviet zone	0.109	-0.040	0.089	-0.040	0.094
	(0.165)	(0.160)	(0.187)	(0.153)	(0.132)
Within-WW2 differences					
1943 × Soviet zone					-0.009
					(0.025)
1944 × Soviet zone					-0.004
					(0.025)
1945 × Soviet zone					-0.003
					(0.025)
Post-WW2 differences					
1946 × Soviet zone	-0.106***	-0.080*	-0.094**	-0.085*	-0.111***
	(0.030)	(0.043)	(0.039)	(0.043)	(0.024)
1948 × Soviet zone	-0.108***	-0.083***	-0.102***	-0.093***	-0.089***
	(0.015)	(0.014)	(0.017)	(0.015)	(0.023)
1951 × Soviet zone	-0.106***	-0.091***	-0.100***	-0.089***	-0.078***
	(0.017)	(0.015)	(0.018)	(0.016)	(0.018)
1961 × Soviet zone	-0.118***	-0.112***	-0.121***	-0.105***	-0.096***
	(0.020)	(0.020)	(0.022)	(0.018)	(0.010)
1971 × Soviet zone	-0.119***	-0.112***	-0.132***	-0.110***	
	(0.026)	(0.032)	(0.026)	(0.024)	
1981 × Soviet zone	-0.114***	-0.117***	-0.136***	-0.110***	
	(0.031)	(0.042)	(0.030)	(0.031)	
1991 × Soviet zone	-0.120***	-0.116**	-0.143***	-0.110***	
	(0.036)	(0.049)	(0.035)	(0.039)	
2001 × Soviet zone	-0.126***	-0.135**	-0.149***	-0.117***	
	(0.041)	(0.056)	(0.041)	(0.044)	
2011 × Soviet zone	-0.119***	-0.137**	-0.141***	-0.114**	
	(0.041)	(0.057)	(0.040)	(0.045)	
Pair-Year FE	Yes	Yes	Yes	Yes	Yes
1930s econ. and pol. controls		Yes		Yes	
Geography controls (slope, aspect)			Yes	Yes	
Drop pairs along Danube river					
No. observations	2,604	2,604	2,604	2,604	216
No. pairs	93	93	93	93	12
No. unique municipal./districts	95	95	95	95	14
No. periods	14	14	14	14	9
R-squared	0.48	0.72	0.57	0.77	0.55
Mean of dep. var.	7.66	7.66	7.66	7.66	10.98

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of population. The control variables in each specification are interacted with year dummies. The *1930s controls* include the share in agriculture, in manufacturing, and of males all in 1934, the market status in 1939, and the vote share for Social Democrats and Conservatives in 1930. *Geography controls* include the mean slope and five equal-sized groups of mean aspect of the municipalities topography. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (allowing for clustering by municipality within a pair and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

the Soviet Army in Hungary.[31] The Soviet army successfully entered Austria in March 1945 on the Eastern Front. We attribute the sharp population drop in the East between 1945 and 1946 to a large migration stream from East to West as the population fled the advancing Soviet army. To the best of our knowledge, we are the first to provide a quantitative assessment of this internal migration shock.

At the end of WWII, there was also large external migration events. In 1945 there was a large influx of ethnic German refugees (so-called *Volksdeutsche*) and other displaced persons who had left their homes either voluntarily or by force. The majority of ethnic German refugees came from neighboring countries such as Yugoslavia, Czechoslovakia, Romania, and Hungary. Of these approximately 540,000 refugees, only 340,000 remained permanently in Austria (Radspieler 1955). For the purposes of our analysis, it is interesting to know what proportion of these refugees settled, or were allowed to settle, in the Soviet and non-Soviet zones. We found district-level data on the resident population by their place of birth for the years 1934 and 1951. We distinguish between the population born in Austria and the population born elsewhere. Between 1934 and 1951 the share of non-Austrians has increased from 4 to 6 percent. We define the dependent variable to be the ratio of each group to the total population in 1934. We use our estimation model from equation (2.2), and estimate the model for the overall population, the Austrians, and the non-Austrians. Estimations results are summarized in Table 2.3. Column (I) shows that the effect on the overall population decline of about minus 10 percent is very comparable to the estimate we observe in municipality-level data (see the coefficients on “1951 $\times$ Soviet zone” in Table 2.2). Columns (II) and (III) inform us that both groups were less likely to remain in (or move to) the Soviet zone. Two thirds (0.059/0.095) of the overall population decline of around 10 percent is attributable to Austrians and around one third (0.036/0.095) to non-Austrians.

Generalizability

The focus on the municipalities along the demarcation line bears the risk of missing out on the larger picture. Our estimated effect in columns (I) to (V) of Table 2.2 may only be a local phenomenon that is specific to the geographic area along the demarcation line. For instance, people might have left their homes to escape the approaching Soviet army, but did not go far away from their previous homes. We now examine the generalizability of our findings to larger/different geographic areas.

Before turning towards the results on generalizability themselves, we provide a first look at data in a balance test: Panel A of Table 2.1 displays geographic features of our sample region. In Panel B and C we analyze the demographics from 1934, the last census before the war and pre-war election outcomes. In Panel D and E we show the raw averages for population in the years before and after the war, respectively. In particular, in Panel B it can be observed that there were no significant differences between villages on either side of the later demarcation line.

31. The seeking of revenge and craving for booty among Soviet soldiers indeed led to assaults on the local population. In particular, there is evidence for mass rapes taking place in connection with combat operations, but also during the onset occupation (Stelzl-Marx 2012).

Table 2.3: Estimation of the Effect of the Soviet Occupation on Population: Decomposition by Location of Birth

	Ratio of pop. of group g in t to total pop. in 1934		
	Born in Austria	Not born in Austria	Overall
	(I)	(II)	(III)
Base-year (1934) differences			
Soviet zone	-0.009	0.009	0.000
	(0.009)	(0.009)	(0.000)
Post-WW2 differences			
1951 × Soviet zone	-0.059***	-0.036**	-0.095***
	(0.014)	(0.013)	(0.017)
Pair-Year FE	Yes	Yes	Yes
No. observations	48	48	48
No. pairs	12	12	12
No. districts	14	14	14
No. periods	2	2	2
R-squared	0.80	0.68	0.87
Mean of dep. var.	0.96	0.05	1.01
in 1934	0.96	0.04	1.00
in 1951	0.94	0.06	1.00
ratio 1951 to 1934	1.00	1.88	1.02

Notes: This table summarizes estimation results based on district-level data according to borders in 1939. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the ratio of the respective variable, i.e. sub-population to the total population in 1934. The control variables in each specification are interacted with year dummies and include a Soviet zone dummy, the log population in 1934 and 1939, the log population in agriculture and in manufacturing, and the share of males in the population, all measured in 1934. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

In Table B.2 in the Appendix we run a standard difference-in-differences specification to test the robustness of our local results: In column (I) we use the sample of bordering municipalities, columns (II) and (III) shows the bordering court districts and administrative districts respectively, column (IV) displays municipalities of bordering states and column (V) all Austrian municipalities. Overall, the effects slightly increase as we expand the geographical reach of our estimation sample, but they are very comparable with our main results, ranging from about minus 8 to minus 30 percent. This also highlights the need for a bigger focus on local confounders, such as market access, which we control for in our main specification.

Moving Away from the Demarcation Line

Additionally, we include areas further away from the demarcation line in our estimation sample. In particular, we use estimation samples based on pairs of municipalities that have the same absolute distance to the demarcation line. We distinguish four groups: Municipalities with a distance between 0 and 10km, between 10 and 20km, between 20 and 30km, and between 30 and 40km. In this way we can observe the population drop as we move away from the demarcation line. Estimation results are summarized in Appendix Figure B.2 and Appendix Table B.3. It turns out that the population response is quantitatively very comparable, as we move further away from the demarcation line. The effects slightly increase as we expand the geographical coverage of our estimation sample, but are very comparable with a range from about minus 8 to minus 20 percent. We have also estimated the effect based on a so-called "doughnut sample". This contains municipality pairs along the demarcation line that are not further apart than 10km, but do not share a common border. Based on this sample we estimate a population drop of 11.8 percent, which is statistically indistinguishable from the main result.

In column (VI) of Table 2.2 we include *all* municipalities in the bordering districts in our sample. In this estimation, we do not form municipality-pairs and, thus, do not control for pair-year fixed effects. Based on this district-level sample we estimate a population drop of around 12 percent, which remains highly persistent. This set of results is convincing evidence that the estimated effect in column (I) is *not* just a local phenomenon. The population drop (rise) in the Soviet (non-Soviet) zone is present throughout. This also means that migrants' points of departure (their initial residence) and the points of arrival (their new residence) were equally distributed in space in the respective occupation zone, and this initial distribution was highly persistent over the following seven decades.

Urban Areas

The demarcation line between the Soviet and non-Soviet zones often runs through rural areas. When we estimate the effect of the Soviet occupation in our baseline sample with bordering municipalities, we therefore estimate the effect in mostly rural areas. It is also interesting if the Soviet occupation has the same effect in urban areas, where economic activity is more concentrated. Previous literature has shown persistent effects of population shocks to local labor markets (Braun et al. 2020). Appendix Figure B.3

shows the population development of Austrian cities relative to 1939 by occupation zone. Interestingly, even in the sample of all municipalities over 10,000 inhabitants in 2011, the cities in the Soviet zone developed with precisely the same trend before WWII as the cities in the non-Soviet zone. The difference in population levels after WWII, however, is remarkable. There is a wide gap between cities in the two zones. There is an immediate effect of minus 30 percent in the Soviet zone that remains more or less stable over time.

2.4.2 Robustness & Sensitivity Analysis

Placebo Tests

One threat to our identification strategy is that the occupation zone borders followed the Danube through Upper Austria, which is a natural or pre-existing line of division. Additionally, the Soviet zone comprised the state borders between Lower Austria and Burgenland. This offers the opportunity to conduct multiple placebo tests. In Panel C of Figure 2.4 we visualize the the border placebos along the Danube and along these state borders.

We now address the threat to identification from occupation zone borders that followed these pre-existing divisions. If the area north of the Danube or certain federal states had different population growth paths after WWII, then the estimated effects might be a spurious correlation that we capture. To rule out this possibility, we conduct placebo tests that implement hypothetical occupation zone borders. First, we define a placebo demarcation line along the border of the federal states of Lower Austria and Burgenland, both of which were part of the Soviet occupation zone. Second, we define a placebo demarcation line along the Danube River in the state of Lower Austria, which was within the former Soviet zone.

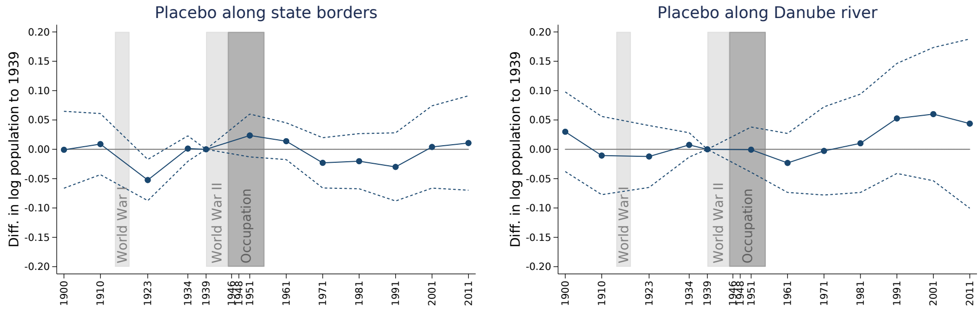
Panel A of Figure 2.5 summarizes the results of these placebo specifications. These estimates include fixed effects for each pair and year of bordering municipalities. In none of these specifications is there a significant or quantitatively important effect of these placebo occupation zones. Appendix Table B.4 provides detailed estimation output (see columns I and II).

Robustness to Geographic Anomalies

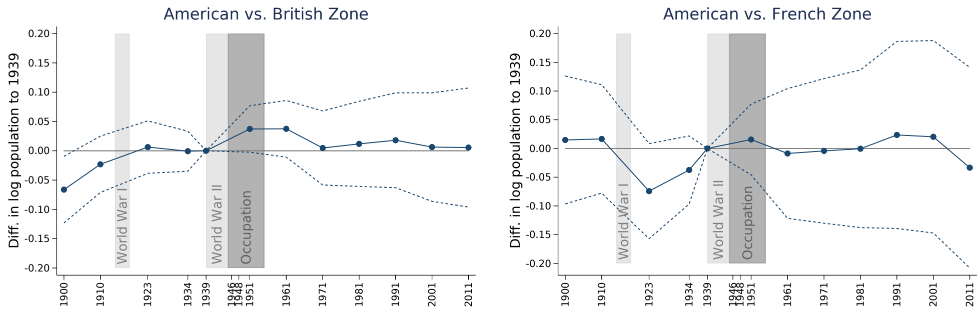
To assess the robustness of our results further, we provide robustness checks dropping all municipalities in our main specification (1) that were part of the ”zone swap” in Upper Austria and (2) that were part of the ”zone swap” agreement that left Styria in the hand of the Western Allies. First in Table B.5 we provide the results for this estimation based on our municipality-pairs specification. Column (II) presents the results for the specification without all pairs that feature Upper Austrian municipalities that were swapped, while Column (III) drops all Styrian municipalities. This attenuates the results only very slightly in the case of Upper Austria. In the case of Styria we lose quite a bit of power, thus complicating the interpretation. Second, to further assess robustness, we run the aforementioned standard Difference-in-Differences estimation using the same

Figure 2.5: Estimation of the Effect of the Placebo Occupations on Population

Panel A: Geographic Placebo Zone Borders



Panel B: Non-Soviet Placebo Zone Borders



Notes: The graphs show the effect of the Soviet occupation on population in bordering municipalities along the Placebo demarcation line or along American and British/French zone borders. Dashed lines show the 95% confidence intervals. Table B.4 shows corresponding estimation results.

sample definitions on Upper Austria and Styria (results can be found in Table B.6). There we can see robust results in both areas (if anything, dropping these only increases the point estimates).

Another issue related to the geography of our main specification concerns the issue of agglomeration (see the discussion of Rosenthal and Strange (2020) in the next section). It is thus an appealing question whether the effect is driven by small or big municipalities in our context. To test the robustness to this argument, we re-run our main results dropping municipalities smaller and bigger then the median respectively: In Table B.5 we show the results for this estimation. Column (IV) presents the results for smaller municipalities, Column (V) for bigger ones. It can be seen that bigger municipalities show stronger, more persistent effects – thus reinforcing our hypothesis on the importance of agglomeration. Standard Difference-in-Differences results, reported in Table B.6 confirm this dynamic.

Homogeneity among Western Allies

In addition, there could also be differences between the occupation zones of the Western Allies. This could threaten our identification, since the main assumption is that only the Soviet occupation was exploitative and all others were supportive.[32] Therefore, we conduct a placebo tests along the borders between the U.S.-U.K. and U.S.-French occupation zones, respectively. The corresponding estimation results are summarized in Panel B of Figure 2.5. (Appendix Table B.4 provides detailed estimation output, see columns III and VI). As expected, there is no significant or quantitatively important effect of these placebo occupation zones in any of these specifications.

In summary, these placebo tests support the causal interpretation of our estimation results presented above.

2.5 Extension: Direct Measures of Economic Activity

So far, we have used population as a proxy for economic activity. In this section, we provide evidence for additional and more direct outcome variables derived from more recent censuses.

2.5.1 Workers and Firms

First, we use data from the Austrian Firm Census. These data are available at the district level between 1902 and 2011 (see Appendix Table B.13). We use these data to estimate the effect of differential military occupation on the number of workers and firms (by size). All of these outcome variables exclude agriculture, in accordance with the scope of this census. Methodologically, we follow our previous approach and form pairs of neighboring districts along the demarcation line. Based on this sample, we estimate

32. See, for example, Ciccone and Nimczik (2022) for an examination of the impact of differences in refugee settlement patterns between the French and U.S. occupation zones in Germany.

a model equivalent to the equation (2.2). The estimation results are summarized in Table 2.4. Columns (II) to (V) use data from the period between 1930 and 2011. For this period we have a panel of 21 districts. Columns (VI) to (VII) use data with longer series starting in 1902. Here we lose six districts that we cannot follow between 1902 and 1930. For comparison, columns (I) show equivalent results for population.

Table 2.4: Estimation of the Effect of the Soviet Occupation on Firms and Workers

						Robustness: long series	
	Population	Workers	Firms all	Firms ≥ 20 workers	Firms ≥ 100 workers	Workers	Firms all
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Pre-WWII differences							
1902 × Soviet zone	0.050*					0.036	-0.027
	(0.028)					(0.171)	(0.182)
Base-year (1930) differences							
Soviet zone	-0.243	-0.201	-0.214	-0.245	0.186	-0.022	-0.132
	(0.195)	(0.247)	(0.197)	(0.241)	(0.296)	(0.317)	(0.206)
Post-WWII differences							
1954 × Soviet zone	-0.154***		-0.083***	-0.364***	-0.536***		-0.073**
	(0.026)		(0.024)	(0.099)	(0.140)		(0.030)
1964 × Soviet zone	-0.173***	-0.264***	-0.150***	-0.392***	-0.488***	-0.283**	-0.163***
	(0.040)	(0.080)	(0.040)	(0.121)	(0.166)	(0.097)	(0.048)
1973 × Soviet zone	-0.201***	-0.298***	-0.188***	-0.364***	-0.706***	-0.348**	-0.224***
	(0.044)	(0.090)	(0.056)	(0.126)	(0.179)	(0.128)	(0.067)
1981 × Soviet zone	-0.206***	-0.357***	-0.212***	-0.430***	-0.736***	-0.410***	-0.243***
	(0.051)	(0.089)	(0.061)	(0.135)	(0.208)	(0.135)	(0.069)
1991 × Soviet zone	-0.210***	-0.365***	-0.270***	-0.445***	-0.842***	-0.403**	-0.305***
	(0.054)	(0.113)	(0.074)	(0.142)	(0.168)	(0.176)	(0.083)
2001 × Soviet zone	-0.208***	-0.372***	-0.298***	-0.341**	-0.942***	-0.427*	-0.329***
	(0.057)	(0.128)	(0.069)	(0.143)	(0.191)	(0.201)	(0.079)
2011 × Soviet zone	-0.204***	-0.370**	-0.270***	-0.411**	-0.838***	-0.417	-0.249**
	(0.062)	(0.155)	(0.093)	(0.159)	(0.203)	(0.243)	(0.116)
Pair-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	360	280	320	320	320	208	234
No. pairs	20	20	20	20	20	13	13
No. unique districts	21	21	21	21	21	15	15
No. periods	9	7	8	8	8	8	9
R-squared	0.58	0.73	0.70	0.80	0.76	0.73	0.73
Mean of dep. var.	10.97	9.59	7.72	4.51	2.57	9.71	7.84

Notes: This table summarizes estimation results based on district-level data. The cities Linz is excluded, since the demarcation disunited the city. The 1954 data does not include information on workers. The dependent variable is equal to the log of population/workers. The specification includes the variables listed and pair-wise year fixed effects (where pairs are given by neighboring districts along the demarcation line). The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Column (II) shows that the number of workers in non-agricultural firms is already 26 percent lower in the districts of the Soviet zone in 1964. By 1981, the difference increases to 36 percent and remains fairly constant thereafter. A comparison with the results on population in column (I), shows that the effect on the number of workers is quantitatively more important. This suggests that using population as a proxy for economic activity provides a lower bound. The estimated effect on the number of firms (of any size) is smaller as compared to the number of workers, but as column (III) shows, it follows the same pattern. Columns (IV) and (V) use the number of firms with more than 20 and 100 workers, respectively, as the dependent variable. These results reveal heterogeneity and suggest, that the Soviet occupation had a larger negative effect on the development of large firms.[33]

To probe the robustness of these results, we look on the estimates based on the longer series in columns (VI) and (VII). These specifications have the advantage of allowing us to test whether districts along the demarcation line followed the same pre-WWII trend in the number of workers and firms. As the results show, there is no evidence of differential pre-WWII differences across occupation zones. The estimated coefficients on the post-WWII period are very comparable to the baseline estimates.

2.5.2 Commuting Behavior and Frontier Workers

In the previous section, we saw that the estimated effect of differential occupation is quantitatively more important for the number of workers than for the population. One possible explanation for this difference is systematic commuting behavior that drives a wedge between local population and local employment. If systematic commuting behavior is significant, then using local population as a proxy for economic activity may underestimate the true effect. It is also important to note that commuting generally became more important over time.

To complement our results based on population and employment as measured by the Austrian firm census, we add further evidence based on more recent population censuses that include information on commuting. Since 1961, we have information on employment at the municipality level and since 1971 we have access to a 5 percent sample of individual-level census data. These data include information on individuals' place of residence, employment status, type of employment, place of employment, and commuting behavior. We use these data to examine employment, local employment, and commuting flows, particularly across the former demarcation line. We construct the following municipality-level outcome variables:

- $Workers_{i,j,t}$: number of residents of municipality i (belonging to pair j) who are employed in year t
- $Local\ workers_{i,j,t}$: number of workers who are employed in municipality i (irrespective of their municipality of residence)

33. To validate our results, we estimate a Poisson model for the count data (not tabulated). It produces very similar effects for the number of firms by size.

- *Frontier-workers*$_{i,j,t}$: number of residents of municipality i who commute across the former demarcation line.

Since we do not observe these outcome variables in pre-WWII data, we have to adjust our estimation strategy. We estimate the following model,

$$\begin{aligned} Y_{i,j,t} = \alpha'' + \beta_t'' \cdot Soviet_{i,j} + \gamma_t'' \times X_{i,j,pre-WWII} \\ + \phi_{j,t}'' \times Area\text{-}Pair_j + \varepsilon_{i,j,t}'', \end{aligned} \tag{2.3}$$

where $Y_{i,j,t}$ represents one of the three outcomes outlined above. We assume that municipalities belonging to different occupation zones are, conditional on observable pre-WWII municipality characteristics ($X_{i,j,pre-WWII}$) interacted with year fixed effects (γ_t'') and year-specific pair fixed effects $\phi_{j,t} \times Area\text{-}Pair_j$, comparable within in our RDD sample. While this assumption is clearly more restrictive, we can show that we obtain equivalent results for the population outcome based on equation (2.2) and (2.3). Column (I) of Table 2.5 summarizes the estimates for the population response based on equation (2.3). These estimates are very comparable to those obtained based on the model specified in equation (2.2), which requires less restrictive assumptions. We assume the same holds for labor-market data.

The remaining columns of Table 2.5 summarize the estimation results for our new labor-market outcomes. Column (II) shows that the employed population (*workers*) dropped by a similar magnitude compared to the resident population (column (I)). Thus, the share of the residential population that is economically active is comparable in the former Soviet and non-Soviet zones. The main result of this section is the large difference in *local workers*, as shown in column (III). We estimate a reduction in local employment in the Soviet zone of 13 percent in 1961. This difference is increasing over time in absolute terms, and amounts to minus 28 percent in 2011. Thus, the examination of labor-market outcomes reveals that the economic activity is substantially more concentrated in the former non-Soviet occupation zone compared to the resident population. Put differently, in our case, population data are an invalid proxy for economic activity, since commuting behavior is not uniformly distributed in space.

In the final column, we look at frontier workers and ask whether more people commute from the former Soviet zone to the former non-Soviet zone than vice versa. This estimate is based on a 5 percent random sample of the decennial censuses from 1971 to 2001. The dependent variable is the share of workers crossing the former demarcation line on their way to work. As expected, we find that significantly more people commute from the former Soviet zone to the former non-Soviet zone than vice versa. The estimated effect is between 4 and 8 percentage points.

We conclude that the distribution of economic activity in space is substantially more concentrated as the distribution of the resident population. A further important difference between these two distributions is their dynamic development over time. The drop in the relative population size was persistent, but stayed more or less constant over time (see Appendix Figure B.4). By contrast, the difference in economic activity, as captured by the local workers, increased over time and almost tripled over a period of about five decades. Figure B.5 illustrates these differing trends graphically, contrasting resident population with local working population and local employment. Put differently, if population data were used exclusively as a proxy for economic activity, the degree to which economic activity is unevenly distributed across space would be underestimated.

Table 2.5: Estimation of the Effect of the Soviet Occupation on Labor Market Outcomes

	Resident Population	Workers	Local Workers	Share of frontier workers
	(I)	(II)	(III)	(IV)
1961 × Soviet zone	-0.119***	-0.086**	-0.130**	0.047***
	(0.022)	(0.034)	(0.051)	(0.009)
1971 × Soviet zone	-0.122***	-0.108***	-0.222***	0.034***
	(0.031)	(0.029)	(0.056)	(0.010)
1981 × Soviet zone	-0.120***	-0.114***	-0.174***	0.030**
	(0.040)	(0.040)	(0.056)	(0.014)
1991 × Soviet zone	-0.128***	-0.126***	-0.244***	0.036**
	(0.047)	(0.047)	(0.066)	(0.015)
2001 × Soviet zone	-0.141***	-0.140***	-0.316***	0.044**
	(0.051)	(0.051)	(0.083)	(0.017)
2011 × Soviet zone	-0.133**	-0.119**	-0.282***	0.063***
	(0.051)	(0.050)	(0.079)	(0.018)
Pair-year FE	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes
No. observations	1,116	1,116	1,116	1,110
No. pairs	93	93	93	93
No. unique municipal.	95	95	95	95
No. periods	6	6	6	6
R-squared	0.97	0.97	0.95	0.59
Mean of dep. var.	7.71	6.89	6.45	0.09

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of population/workers. The control variables in each specification are interacted with year dummies and include a Soviet zone dummy, the log population in 1934 and 1939, the log population in agriculture and in manufacturing, and the share of males in the population, all measured in 1934. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

As in the case of population outcomes, we also estimate the effect of the Soviet occupation for different labor-market outcomes in the four different samples, comprising municipal pairs that are further away from the demarcation line.[34] We run this estimations separately for local workers (see Appendix Table B.7) and commuters (see Appendix Table B.8). In our estimation for local workers, we find a very similar pattern across the four samples, with quite comparable effects. The only notable difference is that the extent of commuting across the former demarcation line decreases somewhat in municipalities that are 30–40km away. We also investigate the impact on commuting in urban areas (i.e., municipalities over 10,000 inhabitants in 2011). We find that even among cities, those in the non-Soviet zone experience more local workers and inbound commuters.

2.6 Mechanism of Persistence

The results so far show that the differential occupation of Austria had long-lasting effects on the population size and economic activity in the former occupation zones. In this section, we discuss potential mechanisms that could account for this persistence.[35] We begin with our preferred explanation, *highly local* agglomeration effects. The migration shock in 1945/46 clearly increased (decreased) population density in the West (East). According to so-called *scale economy models* which allow for multiple equilibria, a sufficiently large shock can switch the economy from one spatial equilibrium to another. The driving force behind this switch are agglomeration effects, which increase productivity in more densely populated areas. Using matched employer-employee data, we provide direct evidence of higher productivity/wages in the former non-Soviet zone (compared to otherwise similar units in the former Soviet zone). In a second step, we discuss other prominent channels (such as government institutions and market access) that can be easily ruled out in our setting without further empirical testing. In a third step, we assess channels that require further empirical investigation. These are selective migration, culture, human capital accumulation, and sectoral composition.

34. Note, in this table we cannot control for the full set of covariates, as in Table 2.5. We do not have information on the sectoral employment and sex ratio in 1934 for municipalities further away form the demarcation line, since we did not track the development of their municipality borders since 1934. Fortunately, the estimation results in Table 2.5 change only marginally if we exclude these covariates.

35. Voth (2021) summarizes the recent literature on the determinants of historical persistence: A large literature shows that institutions are the most important determinant of economic development (see, e.g., Acemoglu, Johnson, and Robinson 2001, 2002). Acemoglu et al. (2011) uses the French Revolution and its radical reforms in Germany to assess its impact on long-run growth. The seminal paper by Becker et al. (2016) on the persistent effects of the Habsburg Empire also discusses other possible mechanisms for persistence, such as: geography, education, and culture. Ochsner and Roesel (forthcoming) provide evidence that local events can still be made a salient issue centuries later using the Turkish Sieges of Vienna as a laboratory. More recent contributions to the literature argue that multiple equilibria should be added to this list, arguing that exogenous shocks can have long-term impacts if they move the society from one equilibrium to another (see, for example, Nunn 2014; Fuchs-Schündeln and Hassan 2016).

2.6.1 Agglomeration Effects

Scholars in economic geography offer two - possibly not mutually exclusive - explanations for the uneven distribution of economic activity across space. The *locational fundamentals theory* considers geographic features (*first-nature characteristics*) as the driving force behind the spatial distribution (see, e.g., Head and Mayer 2004). Accordingly, economic activity is concentrated in areas with attractive natural endowments, such as access to a river or large plain. On the other hand, *scale economy models* stress the importance of the local interaction of economic agents (*second-nature characteristics*). The seminal papers of Henderson (1974) and Krugman (1991), and the subsequent literature demonstrate how agglomeration and dispersion forces (e.g., internal increasing returns to scale and trade costs) determine the spatial distribution of economic activity. Put differently, individuals may cluster in areas that are innately more productive, or density itself may enhance productivity because of agglomeration economies.[36]

These two theories provide different predictions for the adjustment of the spatial distribution in Austria for the period after 1955 (i.e., the year when the former demarcation line became obsolete). The locational fundamentals theory predicts a reversion to the initial spatial distribution of economic activities, since natural advantages of particular locations reassert themselves, and push the economy back to the unique spatial equilibrium. By contrast, scale economy models generally allow for multiple equilibria. If the shock is sufficiently large, it switches the economy from one equilibrium to another. Thus, the internal migration shock we have observed may have permanent effects and shifts the spatial equilibrium. This intuition is presented more formally in B.4, where we develop our own simple spatial equilibrium model.

The empirical evidence we have presented so far suggests that the differential occupation of Austria indeed led to a sufficiently large enough internal migration shock, which has permanently shifted the spatial equilibrium with more economic activity in the former non-Soviet zone. We now directly test for agglomeration effects, which constitute another sufficient condition for the scaling models to hold. We check for productivity differentials in workers employed in firms in the former Soviet versus non-Soviet zone. We measure productivity with wages.

In this context, a relevant consideration is on what level of granularity these agglomeration effects actually occur. Rosenthal and Strange (2020) provide some valuable insight into this question: The authors explore the idea that agglomeration economies diminish

36. Existing empirical papers obtain exogenous variations in scale, while holding natural advantages constant, from two types of natural experiments. First, a number of papers use wartime bombing (see, e.g., Davis and Weinstein 2002; Brakman, Garretsen, and Schramm 2004; Bosker et al. 2007, 2008; Miguel and Roland 2011) and, second, one study exploits spatial restrictions in migration (Schumann 2014). Broadly speaking, the majority of these papers find evidence in line with the locational fundamentals theory. Notably, these studies rely heavily on population data as a proxy for economic activity. Bleakley and Lin (2012) examines exogenous variation in a natural advantage, while holding scale constant. They show that US cities with a historical natural advantage due to a geomorphological feature grew at an even higher rate after changes in technology made this feature obsolete, compared to cities whose first-nature characteristics have not changed. Their finding supports a scale economy model.

with distance. Their paper provides evidence that these economies have effects at different spatial scales, such as regions, cities, and neighborhoods. Interestingly, these effects extend down to smaller scales such as buildings and organizations.

To test this hypothesis we use data from the *Austrian Social Security Database* (henceforth ASSD). The ASSD includes administrative records to verify pension claims and is structured as a matched employer–employee data set. Starting from 1972, we observe for each worker basic socio-economic information and on a daily basis employment along with her occupation. Information on earnings is provided per year and per employer.[37] In a first step, we run log wage regression on individual-level wage determinants (sex, age, citizenship, occupation) for each year of data. We then calculate the mean of residuals from each of these regressions by municipality and year. In a second step, we use these average wage residuals to estimate the effect of the differential occupation. Therefore, we follow the specification outlined in equation (2.3).

Table 2.6 summarizes the estimation results. Column (I) lists the results from our baseline specification. The regression is weighted (frequency weights) with the number of workers per municipality (from the wage regression in the first step). We find that *comparable* workers earn roughly 3 percentage points less if the are employed in the former soviet zone. This estimated wage differential is quite constant over time. In Column (II), we control in addition (in the first step of our estimation procedure) for workers industry affiliation (where we distinguish between 85 different industry types, NACE two-digit classification). Column (III) removes immigrants from the dataset as they could contaminate the results. Lastly, in Column (IV), we combine both adaptions. We can see that results on productivity are robust to the inclusion of these controls and remain highly persistent over time.

One concern with our specification is that there might be differential selection into employment in the former Soviet vs. non-Soviet occupation zones. Thus, we test the effect of the Soviet occupation on labor force participation using our municipality level data. In Table 2.7 we present the results for the overall labor force participation in Column (I), for men in Column (II), for women in Column (III) and the gap between (II) and (III) in Column (IV). We do not find any significant difference between the occupation zones, suggesting no differences in selection into employment. In summary, we find robust evidence for a persistent response to the Soviet occupation, in particular as measured by productivity differentials.

To further test the robustness of our estimates we include areas further away from the demarcation line in our estimation sample. In particular, we use estimation samples based on pairs of municipalities that have the same absolute distance to the demarcation line, analogous to Appendix Table B.3. We consider four groups: Municipalities with a distance between 0 and 10km, between 10 and 20km, between 20 and 30km, and between 30 and 40km. Estimation results are summarized in Appendix Figure B.7 and Appendix Table B.9. It turns out that the effect on productivity is quantitatively very comparable,

37. The limitations of the data are top-coded wages and the lack of information on (contracted) working hours (Zweimüller et al. 2009).

Table 2.6: Estimation of the Effect of the Soviet Occupation on Productivity (in Terms of Wage Residuals)

	Baseline	Industry controls	No immigrants	Industry controls + no immigrants
	(I)	(II)	(III)	(IV)
1972 × Soviet zone	-0.036**	-0.036**	-0.037**	-0.036**
	(0.015)	(0.015)	(0.015)	(0.015)
1981 × Soviet zone	-0.029**	-0.037***	-0.031**	-0.038***
	(0.013)	(0.010)	(0.012)	(0.010)
1991 × Soviet zone	-0.027***	-0.028***	-0.028***	-0.029***
	(0.010)	(0.008)	(0.010)	(0.009)
2001 × Soviet zone	-0.019*	-0.021**	-0.021*	-0.024**
	(0.011)	(0.010)	(0.011)	(0.010)
2011 × Soviet zone	-0.030*	-0.036***	-0.032**	-0.035***
	(0.016)	(0.012)	(0.016)	(0.013)
Pair-year FE	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes
No. pairs	106	106	106	106
No. unique municipal.	104	104	104	104
No. periods	5	5	5	5
No. observations	435,948	435,948	404,235	404,235
R-squared	0.82	0.79	0.82	0.78
Mean of dep. var.	-0.01	-0.02	-0.00	-0.02

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is the wage residuals for bordering municipalities. The control variables in each specification are interacted with year dummies and include a Soviet zone dummy, the log population in 1934 and 1939, the log population in agriculture and in manufacturing, and the share of males in the population, all measured in 1934. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 2.7: Estimation of the Effect of the Soviet Occupation on Labor Force Participation

	Labor force participation			
	Overall	Male	Female	Gender gap
	(I)	(II)	(III)	(IV)
Post-WW2 differences				
1951 × Soviet zone	0.023***			
	(0.008)			
1961 × Soviet zone	0.010*	0.015	0.026	-0.010
	(0.006)	(0.027)	(0.050)	(0.067)
1971 × Soviet zone	0.002	-0.008	0.007	-0.015
	(0.005)	(0.006)	(0.009)	(0.010)
1981 × Soviet zone	0.000	0.001	-0.000	0.001
	(0.004)	(0.004)	(0.007)	(0.008)
1991 × Soviet zone	-0.005	-0.004	-0.006	0.002
	(0.005)	(0.005)	(0.008)	(0.007)
2001 × Soviet zone	0.002	0.006	-0.002	0.008**
	(0.003)	(0.004)	(0.004)	(0.004)
2011 × Soviet zone	0.007	0.009*	0.005	0.004
	(0.004)	(0.004)	(0.005)	(0.005)
Pair-Year FE	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes
No. observations	1,302	1,116	1,116	1,116
No. pairs	93	93	93	93
No. unique municipal.	95	95	95	95
No. periods	7	6	6	6
R-squared	0.87	0.95	0.85	0.92
Mean of dep. var.	0.46	0.47	0.44	0.03

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is labor force participation of each group. The control variables in each specification are interacted with year dummies and include a Soviet zone dummy, the log population in 1934 and 1939, the log population in agriculture and in manufacturing, and the share of males in the population, all measured in 1934. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

as we move further away from the demarcation line. The effects slightly increase as we increase the distance to the demarcation line even further for our estimation sample.

These more direct measures provides credible evidence on the existence of agglomeration effects. One particular insight that the Austrian setting provides is the persistence of these effects even in the absence of a coordination device. Previous literature (e.g., Becker et al. 2020) found persistent effects on education in the presence of a coordinating force in a similar context, specifically, in the case above the resettlement of millions of Poles after WWII. In our case, the effect is due to an uncoordinated flight from east to west in anticipation of the occupation that would follow.

2.6.2 Governmental Institutions and Public Services

The previous literature provides clear evidence for the importance of institutions on long-run growth performance across countries and regions (Acemoglu, Johnson, and Robinson 2001, 2002). In our context, it is important to stress that despite Austria's partition during the occupation, it was treated as one political unit by the occupying authorities. Crucially, while the behavior of the occupying forces was dramatically different, by the end of 1945 the newly established Austrian government was already providing public services. In particular, national and state elections were held on November 25th. Voter turnout was over 93%, and less than a month later the newly elected government was sworn in. Thus, important determinants of long-term change such as schooling (unlike in Germany, see Fuchs-Schündeln and Hassan (2016)) and health care were not under the control of the occupying authorities. Thus, institutions are unlikely to be the channel for the persistence observed here.

2.6.3 Market Access

Redding and Sturm (2008) use the division and subsequent reunification of Germany as a natural experiment to test the importance of market access for economic development. Their central finding is that cities in West Germany located close to the former inner-German border experienced significantly lower population growth than other western cities. They interpret this pattern as evidence that the sudden loss - and later, restoration - of access to markets across the border had persistent effects on local economic outcomes. In other words, proximity to the border in their setting serves as a proxy for differential exposure to market access shocks, with locations near the frontier disproportionately suffering from the collapse of cross-border economic interactions after division.

This insight highlights the importance of carefully ruling out time-varying confounding factors related to market access in any research design that exploits the border as a source of quasi-experimental variation. In particular, if areas close to the former demarcation line systematically differed in their access to major western markets in the post-WWII period, as shown in Redding and Sturm (2008) for the case of East Germany's border zone, then simple comparisons across space might conflate the effect of our treatment variable with long-run differences in economic potential driven by changing market accessibility.

Our empirical strategy addresses precisely this concern. By exploiting the demarcation line as a spatial discontinuity and employing the area-pairs specification (2.2), we compare highly localized geographic units that lie directly adjacent to each other across the boundary. These neighboring units share essentially identical geographic characteristics and, crucially, the same access to regional and national markets on either side of the border. The extremely fine spatial scale of our comparison ensures that any broader market-access gradients are differenced out.

Thus, while Redding and Sturm (2008) underscore how large-scale border-induced market-access shocks can differentially affect more and less remote regions, our design eliminates this channel by construction. Because the units we compare are equally situated with respect to postwar western market access, we are confident that the above mechanism does not threaten the validity of our estimates. Our results therefore capture the causal effect of the treatment of interest, uncontaminated by the market-access dynamics emphasized in other contexts.

2.6.4 Selective Migration

Selective migration in 1945/46 may account for the different long-run outcomes in the two regions. For the economic outcomes we have examined, the most pressing question is whether high potentials left the Soviet zone and never returned.[38] We examine migrants in terms of their human capital. This is difficult to test because there is little consistent data on human capital. We try to address the issue of selective migration in the following way: We look at differences in educational attainment over time. A major limitation is the lack of a long time series of education data. We proceed in two steps. First, we test for differences before WWII. To do so, we use district-level data on the share of individuals attending school in 1900, on the eve of the Habsburg Empire. These data allow us to look for differential trends in schooling long before the turmoil of war and occupation. The results summarized in Panel A of Table 2.8 show that there is no significant difference between later occupation zones. This result is robust to splitting the sample by gender (columns II and III) and to focusing only on adjacent municipalities (columns IV to VI). As our main point of comparison, in a second step we examine the educational attainment of individuals born before 1920 and residing in the former Soviet and non-Soviet zones in 1971. We observe this information in the census. The choice of birth cohorts ensures that we are comparing individuals who completed their schooling before the occupation began (and thus are not affected in their educational choices by the potential shock of foreign occupation). The estimation results are summarized in Panel B of Table 2.8. If anything, there is a small (sometimes insignificant) positive effect on the educational attainment of people living in the former Soviet zone. Thus,

38. There is evidence that members of the Nazi elite were more likely to flee westward to zones occupied by the Western Allies (Ochsner and Roesel 2020).

the assumption that positively selected people left the Soviet zone for the West is not supported by this empirical analysis.

Table 2.8: Educational Attainment Distribution in the Soviet and in the non-Soviet Zone

Panel A: Schooling in 1900 by Occupation Zone

	Share of individuals visiting school by district					
	full sample	male	female	full sample	male	female
	(I)	(II)	(III)	(IV)	(V)	(VI)
Soviet zone	-0.003	0.006	-0.014	-0.008	-0.001	-0.015
	(0.008)	(0.009)	(0.012)	(0.009)	(0.009)	(0.017)
Border Sample	No	No	No	Yes	Yes	Yes
No. observations	51	51	51	18	18	18
R-squared	0.00	0.01	0.03	0.04	0.00	0.05
Mean of dep. var.	1.00	1.00	1.01	1.00	1.00	1.01
S.d. of dep. var.	0.03	0.03	0.04	0.02	0.02	0.03

Panel B: Educational Attainment in Individuals Born Before 1920

	Share with educational attainment of				
	mandatory educ. only	apprent- iceship	middle school or more	high school or more	tertiary education
	(I)	(II)	(III)	(IV)	(V)
1971 × Soviet zone	0.013*	-0.017***	0.005	0.004	0.001
	(0.007)	(0.005)	(0.004)	(0.003)	(0.001)
Pair-year FE	Yes	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes	Yes
No. observations	186	186	186	186	186
No. pairs	93	93	93	93	93
No. unique municipal.	95	95	95	95	95
R-squared	0.85	0.81	0.83	0.79	0.71
Mean of dep. var.	0.84	0.12	0.04	0.02	0.01

Notes (I): **Panel A** of this table summarizes estimation results based on district-level data from 1900. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the ratio of children receiving any type of schooling in 1900 to the total number of children in mandatory schooling age in 1900. Rounding errors in the measurement of each subgroup results in means greater than 1. Standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.
Notes (II): **Panel B** of this table summarizes estimation results based on municipality-level data from 1971. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the share of the respective variable. The control variables in each specification are interacted with year dummies and include the log population in 1934 and 1939, the log of the population in agriculture and in manufacturing and the share of males in the population. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

2.6.5 Human Capital Accumulation

Becker et al. (2020) show that people affected by forced migration were significantly more educated decades after resettlement. This trend is driven by a shift in preferences away from material possessions towards investment in human capital. We now examine whether the differential occupation in Austria had an impact on subsequent investment in human capital. Given that the Soviet occupation was "extractive", including the dismantling of industrial estates and the removal of almost all mobile equipment, and that the Western Allies were more supportive, one could imagine a lasting impact on human capital decisions. For instance, one could imagine a substitution effect between physical capital and human capital. To test this hypothesis directly, we return to the municipal-level data from the decennial censuses. Beginning in 1971, detailed information on educational attainment is available.[39] We examine whether we observe different average levels of educational attainment in neighboring municipalities along the former demarcation line.

In Table 2.9, we have summarized the regression results for the share of the adult population with at least middle school (column I), at least high school (column II), and individuals with tertiary education (column III). We find a statistically significant positive effect of the Soviet occupation on educational attainment, which increases over time. The effect remains strong for higher levels of education. The share of students with a high school diploma or higher is not significantly different initially, but is 2.7 percentage points higher by 2011. Similarly, for high school, the difference is half a percentage point in 1971 and increases to 2.6 percentage points. For those completing tertiary education, this effect increases to 1.3 percentage points. While this is consistent with the previous literature on migration and the hypothesis of physical capital substitution by human capital, it does not help explain the poorer economic development in the former non-Soviet zone.

2.6.6 Culture

Another important aspect of persistence after an exogenous shock like the one Austria experienced is people's preferences. Alesina and Fuchs-Schündeln (2007) show that, after the German reunification, East Germans favor state intervention more than West Germans. This is mainly a function of age and converges as time goes on. Lichter, Löffler, and Siegloch (2021) discuss a different effect related to the division of Germany. They investigate the long-run effects of government surveillance in Eastern Germany. The find a persistent negative effect on interpersonal and institutional trust. Given that trust is important for economic relationships it is not surprising that they also find a negative impact (of surveillance) on income and (self-) employment. We test for this effect with survey results on trust towards others on the district level from the *Austrian*

39. The pre-WWII data presented in Table 2.8 provide suggestive evidence that there were no different trends in schooling between the Soviet and non-Soviet zones before WWII.

Table 2.9: Estimation of the Effect of the Soviet Occupation on Educational Outcomes

	Middle school or more	High school or more	Tertiary education
	(I)	(II)	(III)
1971 × Soviet zone	0.007	0.005*	0.001
	(0.004)	(0.003)	(0.001)
1981 × Soviet zone	0.009	0.012***	0.003**
	(0.007)	(0.004)	(0.002)
1991 × Soviet zone	0.006	0.013***	0.007***
	(0.007)	(0.005)	(0.003)
2001 × Soviet zone	0.015**	0.019***	0.008***
	(0.006)	(0.005)	(0.003)
2011 × Soviet zone	0.027***	0.026***	0.013***
	(0.006)	(0.005)	(0.003)
Pair-year FE	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes
No. observations	930	930	930
No. pairs	93	93	93
No. unique municipal.	95	95	95
No. periods	5	5	5
R-squared	0.95	0.93	0.91
Mean of dep. var.	0.18	0.09	0.03

Notes: This table summarizes estimation results based on municipality-level data from 1971, 1981, 1991, 2001, and 2011. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the share of population that reached at least the indicated education level. The control variables in each specification are interacted with year dummies and include the log population in 1934 and 1939, the log of the population in agriculture and in manufacturing and the share of males in the population. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Gender & Generations Survey (GGS) (here no pre-WWII data is available). Appendix Table B.10 shows the results from this estimation. The reported estimates are small and insignificant. This is suggestive that the differential occupation had no effect on trust.

2.6.7 Sectoral Composition

In a last step, we analyze whether the Allied occupation affected the spatial distribution of economic activities across sectors. Census data provide us with the number of people working in agriculture, manufacturing, and the service sector from 1934 through 2011.[40] As in most industrialized countries, the importance of the agriculture sector decreased sharply over this period in Austria (from 45% to 4%), while the service sector expanded (from 21% to 69%). The relative size of the manufacturing sector follows an inverted U-shaped pattern, with a peak in 1971 at 44% of total employment.[41] Since we have pre-WWII data, we can employ the estimation strategy described in equation (2.2). The respective estimation results for the log share of people working in agriculture, manufacturing, and the service sector are summarized in Table 2.10.[42] The mostly insignificant coefficients on the interaction term between the binary variables capturing the data from the year 1934 and the one for the Soviet zone (see first row), indicate that the municipalities along the demarcation line did not differ in their sectoral development before the occupation in agriculture and manufacturing. The significant effect of the service sector trend is economically small, since the service sector accounted for only 12% at that time.

The treatment effects are given by the interaction terms using the post-WWII years. We find that it took the Soviet zone initially longer to shift from agriculture to manufacturing and services. The smaller manufacturing sector remains fairly constant over time, but the coefficients are insignificant from 1971 onwards. Between the 1970s and the 1990s there are no differences in the sectoral compositions between the zones discernible. Interestingly, we see that the Soviet zone is more service-focused in more recent decades.[43] In sum, results on sector shares are not as pronounced as compared to the other outcomes studied above and are mostly temporary in our sample.

40. The sector shares are calculated based on the employees' municipality of residence. Given that the we find stronger employment effects for *local workers* as compared to *workers* in general (see Table 2.5) and a strong effect for productivity, it would be interesting to define the sector shares also based on the location of the employer. However, this information is not available in the census data before 1971.

41. These numbers do no include the city of Vienna.

42. Log shares are appropriate in a DiD framework if the common trend of the treatment and control groups changes the sector share by a factor, not a constant. Since sector shares changed considerably between the base year (1939) and some of the post-war periods, we do not assume that a potential effect on the sector share in 1951 remained constant over time, but assume instead a constant percentage change in the sector shares. We therefore assume a data generating process of the form $s_{i,t} = \alpha^t \bar{s}_i \gamma^{Soviet_i}$, where α is a trend parameter and γ is the effect of the Soviet occupation, so that the log share is $\log s_{i,t} = t \log \alpha + \log \bar{s}_i + Soviet_i \log \gamma$.

43. When the sector shares are used as outcome variables, the qualitative picture is very similar, but less pronounced. See Appendix Table B.11 in the Appendix for additional results on firms and workers.

Table 2.10: Estimation of the Effect of the Soviet Occupation on Sector Employment Shares

	Employment share		
	Agriculture	Manufacturing	Services
	(I)	(II)	(III)
Pre-WW2 differences			
1934 × Soviet zone	0.013	-0.000	
	(0.011)	(0.012)	
Base-year (1939) differences			
Soviet zone	-0.006	0.022	-0.017
	(0.028)	(0.025)	(0.011)
Post-WW2 differences			
1951 × Soviet zone	0.027***	-0.021**	-0.007
	(0.009)	(0.010)	(0.009)
1961 × Soviet zone	0.022*	-0.022*	0.000
	(0.011)	(0.012)	(0.011)
1971 × Soviet zone	0.006	-0.003	-0.003
	(0.018)	(0.022)	(0.013)
1981 × Soviet zone	-0.007	0.010	-0.003
	(0.023)	(0.026)	(0.014)
1991 × Soviet zone	0.001	-0.003	0.002
	(0.026)	(0.028)	(0.016)
2001 × Soviet zone	-0.002	-0.021	0.022
	(0.027)	(0.028)	(0.014)
2011 × Soviet zone	0.000	-0.023	0.023*
	(0.026)	(0.026)	(0.013)
Pair-Year FE	Yes	Yes	Yes
No. observations	1,674	1,674	1,488
No. pairs	93	93	93
No. unique municipal.	95	95	95
No. periods	9	9	8
R-squared	0.90	0.76	0.94
Mean of dep. var.	0.31	0.36	0.34

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the share of population working in the respective sector in each municipality. The control variables in each specification are interacted with year dummies and include a Soviet zone dummy, the log population in 1934 and 1939, the log population in agriculture and in manufacturing, and the share of males in the population, all measured in 1934. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Our findings relate to two empirical facts established in other, previous work: Ochsner (2023) shows that the dismantling of infrastructure and outflow of skilled workers caused by the temporary Soviet occupation of Styria resulted in differential development of investment in industries. We corroborate the results on this mechanism with detailed employment data. Eder (2022) documents a faster shift from agriculture to manufacturing and later from manufacturing to services in Austrian regions which suffered more casualties during WWII.

2.7 Conclusions

This chapter studies the long-run economic consequences of a short-lived but violent episode of military occupation. When Soviet forces entered eastern Austria in 1945, their advance was accompanied by widespread violence, looting, and expropriation that sharply contrasted with the more orderly occupation regimes implemented by the Western Allies. Although the Allied occupation lasted only ten years and Austria was subsequently reunified under a common institutional framework, the abrupt divergence in the conditions of occupation created a powerful local shock across otherwise comparable regions.

Using a spatial regression discontinuity design combined with a difference-in-differences approach, we show that areas first controlled by Soviet forces remain significantly less economically developed today. These regions are less populated, host fewer and lower-paying jobs, and exhibit substantially higher rates of out-commuting than otherwise similar areas secured by Western Allied forces. Importantly, these disparities have not converged over time; in several dimensions, they have widened.

Our evidence points to a large and sudden migration response as the key initial mechanism. Before travel restrictions were imposed, roughly 11 percent of the pre-war population fled from Soviet-entered areas into the Western zones. This asymmetric population shock reshaped the spatial distribution of labor and economic activity across Austria. Over time, agglomeration forces amplified these initial differences, concentrating employment and productivity in the more densely populated Western regions. As a result, the onset of the occupation appears to have shifted the spatial equilibrium of economic activity across Austrian regions in a way that has persisted for nearly eight decades.

Taken together, our findings show that the conditions prevailing at the onset of military occupation, rather than the duration of occupation itself, can have lasting economic consequences. Even a relatively brief episode of violent and extractive foreign rule can trigger migration and agglomeration dynamics that lock in persistent spatial inequalities. By highlighting these mechanisms, our results contribute to a broader understanding of how external military interventions can leave durable economic legacies.

Several avenues for future research remain. First, more evidence is needed on how different forms of occupation, from violent seizure to administrative oversight, affect local economic trajectories. Second, the interaction between conflict-induced migration, agglomeration forces, and long-run spatial inequality remains understudied. Finally, comparative work across historical and contemporary contexts could help identify the conditions under which occupation shocks generate persistent divergence or, alternatively, eventually converge.

3 From Economics of Density to Economics of Destiny? Bombs, Housing and Inequality in Vienna*

Abstract

Cities are reshaped by shocks such as wars and natural disasters, which disrupt established spatial and social structures. Understanding how urban areas recover from these events is central to explaining long-term patterns of economic geography and inequality. This chapter examines how wartime destruction affects the evolution of city neighborhoods, using the Allied bombing of Vienna during World War II as a natural experiment. I construct a newly digitized, high-resolution dataset linking archival records of building-level bomb damage to modern property values, census data, and firm information. The results show that heavily bombed areas experienced extensive post-war reconstruction and expansion, with substantial increases in residential buildings, firm counts, and population density. At the same time, there is no decrease in long-run real estate prices, incomes, or educational attainment. Overall, the evidence points to a recovery process driven by rebuilding and spatial expansion, rather than major socioeconomic divergence, highlighting the resilience and context-specific nature of urban reconstruction after large shocks.

3.1 Introduction

Cities develop through both gradual adjustment and discrete shocks that disrupt established spatial equilibria. Wars, natural disasters, or technological transformations can inflict severe destruction on the built environment, yet they also create the possibility of large-scale reorganization. Because cities serve as enduring hubs of production and consumption, such episodes often mark critical points in their long-run trajectory. Destruction can simultaneously impose costs and alleviate structural constraints, such as land-use regulations or property rights, that typically impede change. Understanding how reconstruction unfolds after major shocks is therefore central to explaining the persistence and transformation of urban structure over time.

In this chapter, I study how cities rebuild after large shocks by examining wartime destruction in Vienna during World War II. Specifically, I use the pattern of Allied bombing

* I am indebted to my advisor Ádám Szeidl for his guidance and support. I am grateful to Gábor Békés, Alice Kügler, Miklós Koren, Michael Landesmann, Tomy Lee, Sergey Lychagin, Ulrich Morawetz, Dávid Nagy, Matthias Schnetzer, Claudia Steinwender and the participants of the WU Development Economics Workshop, the Trade Economics Workshop in honor of Elhanan Helpman and the 16th Geoffrey Hewings Regional Economics Workshop for their helpful comments and suggestions. I thank Kevin Stadler and the Austrian Academy of Sciences for giving me access to digitized maps of Vienna's WW2 bombing damage. I am grateful to Markus Lampe and Kilian Rieder for sharing their historic real estate data for Vienna with me. I thank Georg Fellner, Jiannis Kaucic and Susanne Reppe for their help in better understanding the historical context.
I am grateful for funding from the Dissertation Support Grant from the Austrian Chamber of Labor and the CEU Research Support Grant.

as a quasi-natural experiment to trace how localized destruction affected neighborhood outcomes over the following decades. Previous research shows that such shocks can have both negative and positive long-term effects, from persistent scars (Redding and Sturm 2024) to creative reconstruction (Hornbeck and Keniston 2017; Takeda and Yamagishi 2024). Yet we know less about the mechanisms through which rebuilding unfolds and how it reshapes the urban fabric. In particular, we know relatively little about whether post-shock development operates through upgrading (higher quality), intensification (higher density), or functional change (residential or commercial use), even though these margins determine how cities absorb shocks and how land values adjust. More broadly, it remains unclear whether reconstruction fundamentally reshapes urban productivity through agglomeration forces or primarily restores the pre-existing spatial equilibrium of the city.

I show that in Vienna, neighborhoods that suffered greater wartime destruction underwent more intensive rebuilding, resulting in higher residential density and an expansion of amenities without any loss of housing quality. Destruction thus set in motion a process of denser, more vibrant neighborhood development rather than long-term decline. Crucially, this reconstruction restored density without generating additional agglomeration effects. Although bombed neighborhoods today host more residents, firms, and amenities, these increases occur roughly in proportion to population growth, leaving firms and amenities per-capita largely unchanged. Consistent with this pattern, real-estate prices do not increase in more heavily destroyed areas.

These findings indicate that post-war reconstruction primarily relaxed built-environment constraints and enabled denser residential development rather than creating new productivity advantages or urban clustering. In this sense, destruction activated an expansion margin—allowing more housing units on the same land—while leaving housing quality broadly unchanged and generating little functional transformation. The main contribution of the chapter is to identify this density-led reconstruction mechanism and to show how cities can absorb large shocks by restoring density without producing new agglomeration externalities.

This pattern also contrasts with evidence from other historical reconstruction episodes. For example, Hornbeck and Keniston (2017) show that the Great Boston Fire triggered heterogeneous rebuilding that reorganized land use and enabled productivity-enhancing redevelopment. In Vienna, by contrast, reconstruction largely preserved the city's prewar institutional and spatial structure. Rather than enabling extensive land consolidation or functional transformation, rebuilding primarily restored housing capacity within existing planning and ownership frameworks, resulting in higher density but little change in per-capita economic activity or land values.

For my empirical analysis, I create a newly digitized, highly detailed dataset on wartime destruction and connect it to property values, and socioeconomic composition in Vienna before and after WWII. I utilize and map bomb damage data from the Viennese Municipal Archives to assess pre-war housing, built-up areas and the extent of wartime destruction at the individual building level (see Figure 3.1). This data is combined with information on historical property values. These are then merged with highly detailed (registry-based) census data from 2011-2013 on socioeconomic and firm outcomes. For the main part of the analysis, I end up with 4223 grid cells of 250 x 250 meters for the whole city of Vienna (Troger 2018).

In my empirical analysis, I follow the recent literature in urban economics on neighborhood effects: I use a fixed-effects specification to examine the causal effect of wartime destruction on post-war (and as placebo test, pre-war) outcomes (Redding and Sturm 2024). The core identification assumption is that, after controlling for fine-grained (1–2 km) grid-cell fixed-effects, any remaining variation reflects the causal impact of bombing on the measured outcomes. In Vienna, this assumption can be directly tested only for real estate prices due to limited interwar socioeconomic data. The sparse existing evidence suggests that wartime destruction was quasi-random at the neighborhood level, supporting the validity of this approach. Finally, to test the robustness, I vary the spatial clustering level (1 vs. 2 km), redefine wartime destruction (from severely to all bombed buildings), and shift the grid layout following Békés and Harasztosi (2018) to test sensitivity to neighborhood definitions.

The first set of results shows that wartime destruction led to a substantial and highly localized expansion of Vienna's housing stock, which constitutes one of this chapter's central contributions. Local areas experiencing greater destruction saw a notable increase in post-war housing development, particularly in the number of buildings and flats, even after controlling for 1 km grid-cell fixed-effects. These increases are both statistically and economically significant, with destroyed areas seeing more residential construction and especially more flats, each additional bombed plot implying roughly a 10% increase in the number of flats, though changes in flat size were modest. The robustness of these findings is supported by consistent results across different model specifications. Additional evidence from the age of buildings in 2013 indicates that affected areas retained more pre-1919 buildings, yet experienced a concentrated expansion of new construction in the immediate post-war decades. Although bombed areas also saw a rise in the number of public housing estates built right after the war, this did not translate into a comparable rise in public housing flats, suggesting that the expansion was driven primarily by private (or non-public) developments. Importantly, this substantial increase in housing supply does not translate into higher real-estate prices in more heavily destroyed areas, reinforcing the interpretation that reconstruction expanded housing capacity rather than generating new location-specific productivity advantages. Overall, the analysis on housing reveals a clear pattern of intense rebuilding and durable expansion in the housing stock of bombed neighborhoods, largely outside the public sector.

The second set of results demonstrates that wartime destruction also produced a long-run expansion in population density and modest shifts in demographic composition, extending the core argument beyond housing to socioeconomic conditions. Areas more heavily damaged during the war now have significantly higher total population levels, indicating sustained densification. While population composition shows only modest changes, there are slight but statistically significant shifts in relationship status, gender distribution, and especially citizenship, with a relative increase in EU (particularly Eastern European) citizens and a decline in Austrian nationals. Bombed areas also tend to have a somewhat younger population and a higher share of university-educated residents, largely replacing those with vocational training. Despite this demographic expansion, income outcomes are mixed: Raw income levels are largely unaffected, truncated income is marginally lower, and there is no meaningful effect on income inequality. Together with the housing results, these patterns indicate that destruction led to higher residential density without generating strong socioeconomic divergence across neighborhoods.

The final set of results examines economic and social infrastructure, showing that destruction triggered a broad expansion in firms and amenities, although these increases mirror population growth rather than stronger agglomeration forces. Areas that experienced heavier bombing now host significantly more firms, particularly small and medium-sized enterprises, and exhibit higher numbers of workers. This increase is driven entirely by the expansion of smaller firms, with no significant effect on large firms. When scaled by population, however, the effect on firms per-capita becomes statistically insignificant, indicating that the rise in firm counts reflects a response to local population expansion rather than denser economic activity. A similar pattern emerges for amenities: Bombed areas today feature more kindergartens, schools, pharmacies, and medical facilities, largely driven by private initiatives such as private schools and kindergartens. Yet once population is accounted for, these relationships disappear, with amenities per 1,000 inhabitants showing no significant link to destruction. Taken together, these results show that post-war reconstruction increased the scale of neighborhood activity but did not generate additional clustering or agglomeration effects.

The robustness checks further reinforce the main results by showing that the observed post-war expansion is not driven by measurement choices or spatial definitions. Varying the grid-cell size (1 km and 2 km), shifting the geographic grid, and altering the operational definition of wartime damage all consistently confirm the main findings. These tests demonstrate that the results are not sensitive to specific measurement choices or neighborhood definitions, reinforcing the credibility and stability of the observed post-war spatial patterns. Overall, the robustness analysis provides strong confirmation that the expansionary responses documented are genuine features of Vienna's reconstruction rather than artifacts of empirical design.

Analyzing the mechanisms shows that this expansion was driven primarily by physical reconstruction processes rather than institutional change, thereby clarifying how destruction generated the long-run growth documented in the main results. The evidence indicates that bombing led to denser built environments, taller residential structures, and more fragmented property ownership, yet it did not substantially alter the proportion of public or council housing. In contrast to findings from other postwar contexts (Redding and Sturm 2024), these patterns reflect the persistence of Vienna's pre-existing institutional framework and its reconstruction policies, which prioritized the restoration of damaged buildings over the creation of new public housing. Long-term, interest-free reconstruction loans further reinforced the continuity of the city's architectural and socioeconomic fabric, allowing redevelopment to proceed largely within the established housing system. Crucially, the analysis finds little evidence that reconstruction generated new agglomeration forces. Increases in population, firms, and amenities occur largely in proportion to residential growth, and real estate prices do not rise in more heavily destroyed areas. This suggests that post-war rebuilding restored urban density without producing additional clustering or productivity externalities. Taken together, these patterns indicate that Vienna's post-war expansion was achieved through physical rebuilding under institutional continuity: A recovery process that transformed the built environment while largely preserving the city's pre-war spatial equilibrium.

This chapter is related to several strands of the literature: First, this study contributes to research on urban economics, particularly focusing on exogenous shocks like wartime destruction and (natural) disasters: In particular fire (Hornbeck and Keniston 2017), pandemic (Glaeser 2022), wartime destruction (Redding and Sturm 2024; Takeda and Yamagishi 2024; Davis and Weinstein 2002), and occupation/reunification (Ahlfeldt et al. 2015) have been studied extensively. Other settings, such as expansion of transportation (Heblich, Redding, and Sturm 2020), neighborhood revitalization (Rossi-Hansberg, Sarte, and Owens III 2010; Almagro, Chyn, and Stuart 2024), location sorting because of tourism (Almagro and Domínguez-Iino 2025) and economic transformation (Heblich et al. 2023) are also highly relevant to this context.[44] Lin and Rauch (2022) provide a detailed overview of the recent literature.

In particular, Redding and Sturm (2024), found large negative effects from wartime destruction on housing quality and tenant income through council housing in London. They highlight sorting, spillover- and neighborhood-composition effects, whereas my results emphasize physical expansion of housing and rebuilding, but *without* significant spillovers or new agglomeration dynamics. On the contrary, Takeda and Yamagishi (2024) document that after the atomic bombing of Hiroshima, the urban structure recovered fully within about five years, restoring the city layout despite the destruction. This is largely driven by strong agglomeration forces. By contrast, my results show more flats, population, firms, and amenities, without evidence for a strong return to pre-war spatial structure or agglomeration-driven recovery. The long-term effect of wartime destruction seems to be diffuse expansion and densification, not a rapid restoration of a pre-existing urban core. Lastly, the Hornbeck and Keniston (2017) show that the Boston Fire of 1872 cleared a large swath of outdated buildings and triggered a wave of reconstruction that enabled growth and economic gains (capitalized in land values). Instead Vienna saw long-run expansion of housing stock, population, firms, and amenities in bombed areas, but there is little evidence of (spillover-driven) agglomeration effects. Growth seems mechanically driven by physical rebuilding and densification, not by changes in neighborhood externalities or coordination failures being resolved.

Second, this chapter contributes to a large literature on the importance of neighborhoods for inequality and (inter-generational) upward mobility. Previous studies use quasi-

44. Ahlfeldt, Albers, and Behrens (forthcoming) analyze "prime locations" in global cities around the world based on local economic activity.

experimental variation, the *"Moving to Opportunity"* (MTO) program or neighborhood revitalization programs: Chetty et al. (2014) show the importance of neighborhood characteristics for social upward mobility. Chetty, Hendren, and Katz (2016) also show that this effect is meaningful for children's education more directly. In contrast, I exploit a city-wide shock (wartime destruction in Vienna) as a source of exogenous variation. I add to the existing evidence by showing that instead of relying of expensive programs to move lower SES families, regional planning- and rebuilding policies can have large impacts in the long-run.

Lastly, the chapter contributes to a sprawling literature on Vienna's (social) housing policies. Kadi and Matznetter (2022) and Banabak, Kadi, and Schneider (2024) show the long history of gentrification in Vienna and how housing policies (past and present) aided and counteracted this development. Morawetz and Klaiber (2022) use spatially disaggregated data for Vienna to assess the impact of housing policies on the magnitude of spatial sorting by different income groups near urban amenities. Musil and Kaucic (2024) take an in-depth look at two inner-city districts to assess the role of housing market segmentation as driver of urban micro-segregation. Premrov and Schnetzer (2023) describe the impact of council housing on neighborhood income inequality. The closest study to my project is Zhu et al. (2025): They explore how the bombings of World War II influenced the long-term redevelopment of Vienna. Drawing on detailed parcel-level data, they analyze building heights, patterns of property ownership, and population density. My chapter adds evidence on the long-run persistence of housing policies, the stickiness of spatial equilibria and (the absence of) spatial segregation.

The rest of the chapter is structured as follows: Section 3.2 describes the institutional background and historical context, describes the data and outlines the empirical strategy. Section 3.3 presents the main analysis and empirical results. Section 3.4 discusses the results. Section 3.5 summarizes the scope for additional work and concludes.

3.2 Context, Data, and Empirical Strategy

3.2.1 Institutional Context

Housing in Vienna

At the start of 2025, Vienna had around 2 million inhabitants,[45] distributed across 23 districts. The city's urban structure reflects a clear inner–outer contrast: The central districts are dense, with limited green space but extensive metro coverage and numerous amenities, while the outer districts feature more dispersed housing (e.g., single-family homes with gardens), larger parks, and fewer transport links (Morawetz and Klaiber 2022).

45. Vienna's population has fluctuated considerably over the past century, declining from 2 million in 1910 to 1.5 million in 1971 due to war, economic hardship, and sub-urbanization, before recovering from the 1980s onward through immigration.

Vienna comprises about 900,000 dwellings, mainly apartments and detached homes. Roughly 65,000 public housing units were built between the world wars, financed through progressive real estate taxes, and 96,000 more followed after World War II. Today, around 220,000 municipality-owned apartments accommodate over a quarter of the population. Home ownership remains low (under 20%), and rental regulations vary by construction period (Morawetz and Klaiber 2022). Public and subsidized housing, municipal, limited-profit, and capped-rent apartments are spread throughout the city, producing a diverse and spatially mixed housing landscape.

The city's urban development since the 19th century has reflected recurring cycles of displacement, regulation, and reinvestment. In the late 1800s, rising land values in the inner city displaced working-class residents to peripheral districts, while large developers, backed by mortgage banks, replaced older housing with upscale buildings (Kadi and Matznetter 2022). By the early 20th century, overcrowding and tenant unrest prompted state intervention.

After World War I, rent freezes and tenant protections curbed private investment but eased affordability pressures. The Social Democratic administration launched a large-scale municipal housing program, constructing thousands of flats annually, yet rent control limited mobility and social upgrading. Economic stagnation and depopulation led to physical and social decline in much of the private rental sector (Banabak, Kadi, and Schneider 2024; Kadi and Matznetter 2022). Following the Nazi annexation in 1938, Jewish tenants were forcibly evicted and their homes redistributed to non-Jewish occupants - a case of "reverse gentrification," where elite residents were replaced by lower-status tenants (Kadi and Matznetter 2022).

Post-1945 reconstruction prioritized rent control and stability over market liberalization. Tenants improved dwellings themselves, and new housing was increasingly supplied by non-profit cooperatives. Urban change was gradual and tenant-driven rather than elite-led (Kadi and Matznetter 2022). From the late 1960s, rent liberalization and construction subsidies coincided with population decline and low demand, allowing artists and students to repopulate decaying inner-city areas - an early wave of gentrification. Public policy shifted toward "soft renewal," combining subsidized renovation with anti-displacement measures (Morawetz and Klaiber 2022).

Since the late 1980s, geopolitical integration and economic growth have intensified housing demand and speculation. Rent deregulation, temporary contracts, and condominium conversions attracted investors, transforming Vienna's private rental market into a profit-oriented sector (Musil et al. 2022). While direct displacement remains rare, exclusionary pressures have grown, partly offset by Vienna's robust public and non-profit housing sector, which together still covers over 40% of the city's dwellings (Kadi and Matznetter 2022).

Also in terms of institutions, Vienna has a storied past: After World War I, the city's Social Democratic government launched a major municipal housing program to combat shortages and improve workers' living conditions. Financed by a dedicated housing tax, it produced around 65,000 affordable apartments between 1923 and 1934, exemplified by the *Karl-Marx-Hof*, combining low rents, standardized amenities, and employment stimulus during economic crisis (Holzner and Huberman 2022).

Post-1945, Vienna resumed large-scale social housing with projects such as the Per-Albin-Hansson-Siedlung (6,000 flats, in the 10th district). Through the late 20th century, it expanded and diversified provision via cooperatives and owner-occupied units. While new construction slowed after the 1980s, the city prioritized "soft renewal" of older housing. By 1991, over 220,000 municipal units existed, and a new initiative, *"Gemeindewohnungen NEU"* (2017), aims to add 3,700 more by 2027 (Wiener Wohnen 2025).

Today, roughly 42% of Viennese households live in public or limited-profit housing, 22% in pre-1945 regulated units, and 20% in ownership housing. Since the mid-2000s, rising property values have reshaped historic stock, deepening spatial and social segmentation (Musil and Kaucic 2024). Despite growing inequality and uneven gentrification, the extensive public and rent-regulated sector continues to buffer exclusion and sustains a social mix (Musil et al. 2022).

Wartime Destruction

From the summer of 1943, present-day Austria became a target of Allied strategic bombing. Initial attacks focused on the *Wiener Neustädter Flugzeugwerke*, an aircraft factory before shifting to oil facilities near Vienna and, by 1945, to major rail hubs. The campaign, based on pre-war Anglo-American planning, aimed to cripple German logistics and secure air superiority for a ground invasion (Reisner 2015).

Following Austria's annexation, Wiener Neustadt, producing nearly half of the Reich's single-engine fighters, became a primary target. U.S. bombers began attacks in 1943, sharply reducing aircraft output and weakening the Luftwaffe. Improved escort fighters in 1944 enabled deeper, more effective raids, which later extended to fuel refineries in and around Vienna.

By early 1945, bombing focused on Austria's transport infrastructure, using radar-guided strikes that caused widespread destruction and civilian losses. In total, 2.7 million tons of bombs were dropped across Europe, with Austria sustaining heavy damage and casualties (Reisner 2015).

Vienna itself was struck relatively late, with 53 air raids recorded between March 1944 and March 1945, as front-lines neared the city. The 1946 *"Kriegssachschadensplan"* ("war damage map") documented 46,862 damaged buildings, 41% of the total - of which 6,214 were destroyed or beyond repair, resulting in the loss of 86,875 apartments. Key industrial sites, bridges, and railway stations were also devastated (Wiener Stadt- und Landesarchiv 2025c). The map, however, served urban reconstruction rather than ordnance identification (further methodological details follow in the next section).

3.2.2 Data Sources

I compile a new, detailed dataset on Vienna's property values, demographic characteristics and wartime destruction, after and, in the case of property values before, World War II. My dataset focuses on the area governed by the city (state) of Vienna from 1945 to today. This area spans just over 415 square kilometers and had a population of 2.03 million inhabitants in 2025.

For my analysis, I use 250 x 250 meter grid-cells provided by the Austrian Statistics Agency (*Statistik Austria*). These are the smallest geographic units where data is consistently provided by the authorities for analysis. Within the city boundaries, there are 7840 grid-cells, of which 4223 are inhabited, which will be the main analysis sample. These units can be grouped into larger areas: To create consistent geographic groupings, I overlay the 250 x 250 meter grids with 1 x 1 km (2 x 2 km) grid-cells across the Vienna metropolitan region, which are also provided by *Statistik Austria*.[46]

Wartime Destruction

I assess the damage caused by the war using bomb damage maps created by the City of Vienna in 1946. These maps were digitized and geo-referenced using the 48 map sheets from the archives (Wiener Stadt- und Landesarchiv 2025c), tracing the built-up areas from 1946 and recording the damage levels using the color-coding system of the maps (see Appendix Figure C.1 for an example). The damage includes a scale which ranges from light damage, heavy damage, burnt out to completely destroyed.[47]

The main measure of destruction is the number of each grid-cell's housing estate, that suffered significant structural damage (classified as 'heavy damage' or worse). More superficial damage, such as light damage, was excluded. To avoid bias, I do not distinguish between repairable and irreparable damage. As a robustness check, I run a specification with all types of damage. I also create a relative damage measure, however the data on the contemporary number of buildings is very unreliable - as can be observed in the comparison of bombing outcomes in Appendix Table C.1. Thus I use the more robust (but somewhat less interpretable) raw measure of number of housing estates damaged or destroyed.

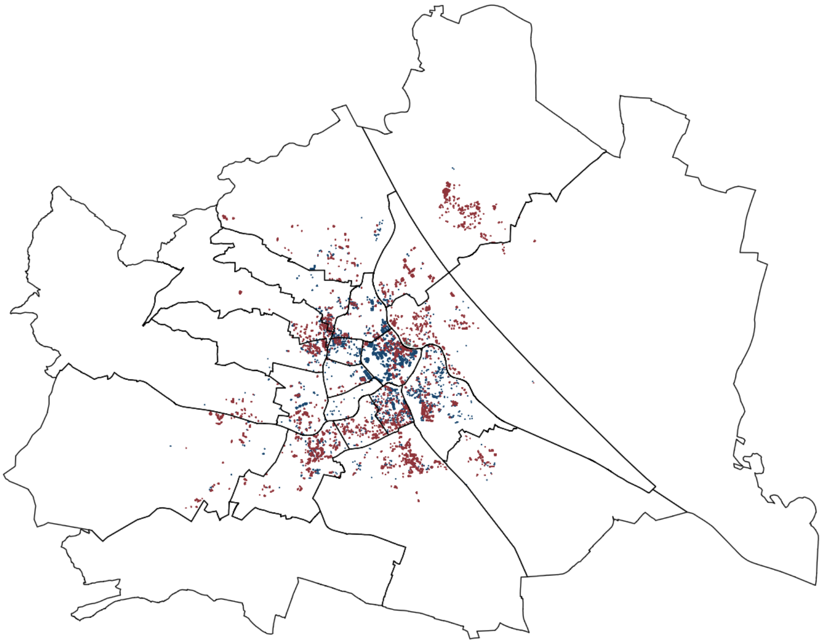
Panel A of Figure 3.1 visualizes damage levels for each building using the original map's color codes. Over 40% of grid-cells shows at least some damage, with over 20% experiencing serious structural harm. Damage was more prevalent in the inner city, though significant variation occurred even within small neighborhoods, supporting my assumption that bomb damage was largely random at a local level. Additionally, some clusters

46. In a later robustness check, I vary the area of the 1 x 1 km grid-cells, following a recent methodological contribution by Békés and Harasztosi (2018).

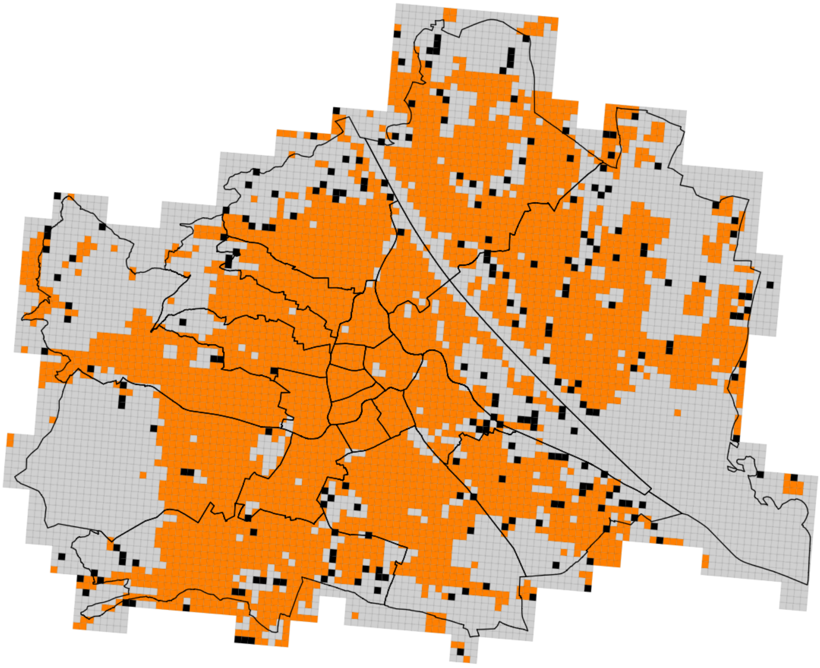
47. Many thanks to Kevin Stadler from the Austrian Academy of Science for providing the digitization output - the detailed methodology and code on extracting the data from the maps can be found at https://github.com/kevinstadler/bombdamage. A discussion on the pros and cons of using machine learning versus hand-collecting data on bombing can be found in the data section of Redding and Sturm (2024). To address these potential criticisms, I random-sampled from the converted data to verify the accuracy and achieved a > 95% level of accuracy.

Figure 3.1: Bombing Data and Grid-Cell Data from Statistik Austria

Panel A: Bombing Data



Panel B: Grid-Cell Data from Statistik Austria



in the suburbs can be observed, reflecting the prevalence of (military) industrial estates. I then aggregate up the data for bomb damage to 250 x 250 m grid-cells, the unit of observation in my further analysis.

Property Values

To measure property values before and after World War II, I use historical data on sales - essentially the court files for property transfers, a system dating back to the monarchy. Specifically, I draw on the 1934, 1939, 1951, 1961 and 1991 contract data for the city of Vienna: This contains a large number of hard-copy archival data, comprising all districts of Vienna. Each entry lists an address, property type (e.g. house, factory or shop) and its sale price. These data from court filings are complemented by data from the land registry. Those are periodically compiled building/land cadastres, in Austria commonly referred to as "Salzberg", named after J. Wolfgang Salzberg.[48]

The original dataset has been collected by compiling each entry from historical records manually. For roughly 50% of the data exact addresses are known and thus can be matched to the main dataset using grid-cells from the national statistics office. Thus, the main estimates on the direct effect of wartime destruction pool all observations, as the bomb damage for the own building is known. For the indirect effect, only datapoints are used that can be address-matched.[49] Thus, I aggregate the residential property price values at the 250 x 250 m grid-cell level. Each sale is assigned to a grid-cell based on location. For flats/houses prices are measures by square meter of livable space, for land prices are measured by square meter of total area. All prices are normalized to the same reference year.

Socioeconomic Outcomes

I estimate the contemporary housing and socioeconomic variables using data from Statistik Austria. The smallest units consistently available in that dataset are the 250 x 250 m grid-cells. An overview of this grid can be found in Panel B of Figure 3.1. Light grey areas are uninhabited, orange are inhabited areas and black signifies an inhabited area, where the statistics office withholds data for privacy reasons.[50]

The contemporary housing and socioeconomic data has been selected from the closest time-frames around the census reform in 2011, to ensure compatibility between all datasets. As such the main housing dataset is from 2014, the demographic, education and income data are from 2013 and the data on businesses is from 2011. From the housing dataset I obtain data on the number of buildings per grid-cell, their type, number

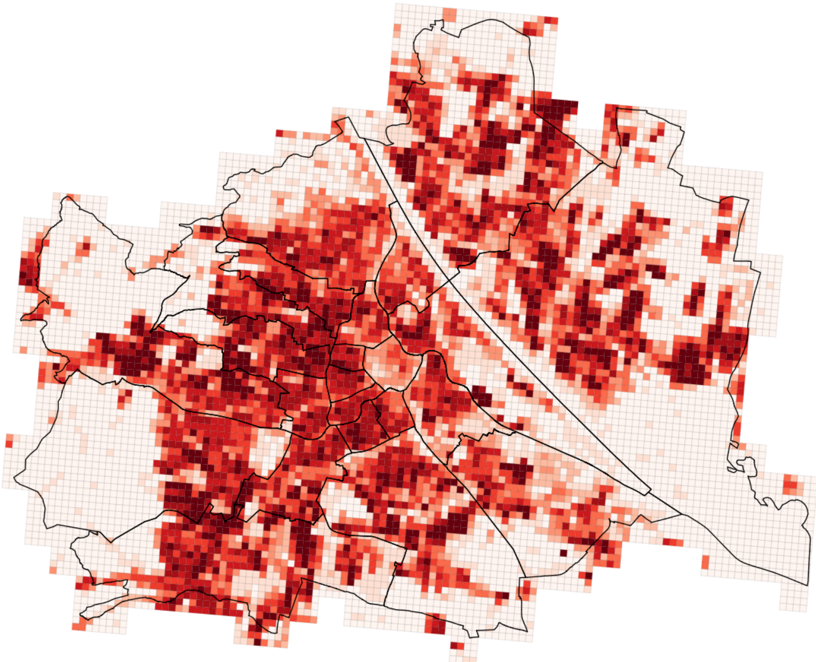
48. This dataset was compiled by a team of researchers of the Austrian National Bank and the Vienna University of Economics and Business. Original data are collected both from district court filings and from historical cadastres (Wiener Stadt- und Landesarchiv 2025b). For further information, see Haider, Lampe, and Rieder (2021). I thank the project team, in particular Markus Lampe, for sharing their data.

49. For the main analysis using my fixed-effects specification, the remaining 50% are dropped. They are used in later robustness checks.

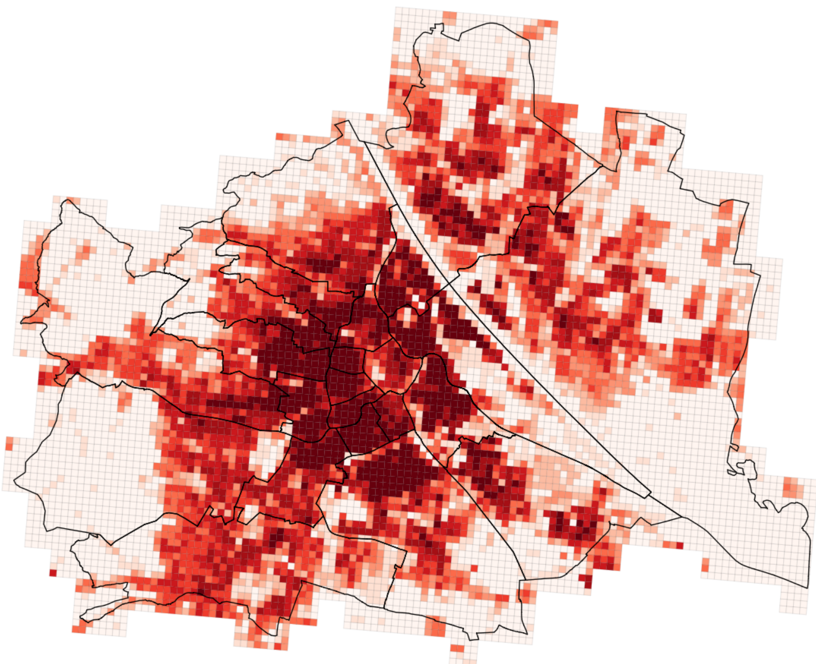
50. Around 200 of the grid-cells are withheld for privacy reasons as there are less than 5 inhabitants living in this cell, and thus could be identified from aggregates.

Figure 3.2: Housing Outcomes across Vienna

Panel A: Stylized Facts - Number of Housing per Grid-Cell



Panel B: Stylized Facts - Number of Flats per Grid-Cell



of flats, average room number per flat and similar information. Panel A of Figure 3.2 displays a heat map of number of buildings per grid-cell, darker colors indicating more densely built-up areas. Panel B then displays the same for the number of flats per grid-cell.

From the dataset on the resident population I obtain data on most demographic characteristics, e.g. age, education, employment, and - crucially - income. In Panel A of Figure 3.3 a heat map of population density is displayed. Panel B of the same Figure shows the income distribution in Vienna by grid-cell. Taken together the data shows clear patterns throughout the city, e.g. a less densely populated but more affluent central business district, surrounded by a more dense area with lower average income which in turn is again surrounded by suburban developments - there, one can clearly spot the less densely populated and most affluent areas of the city. However, it is clearly visible that there are quite strong heterogeneities within most neighborhoods - a fact that plays an important role for our identification strategy.

Lastly, from a special inquiry to *Statistik Austria* I acquire data on income inequality as measured by the Gini Index and from the dataset on businesses I obtain data on the number of firms, number of employees, firm size and sector. In Panel A of Appendix Figure C.2 a heat map of the Gini Index for Vienna is displayed, with darker colors being more unequal. Panel B of the same Figure shows the number of firms in Vienna by grid-cell. The data on inequality shows similar hotspots as the data on income, displaying that the most affluent regions are also the most unequal. The firm-level data details that Vienna has a monocentric structure, with one central business district and less activity in the outer districts.

Pre-WWII socioeconomic conditions are hard to obtain for Vienna. The inter-war data is scarce and of insufficient quality. While there have been population censuses in 1934 and 1939 as well as frequent housing censuses, the overwhelming majority of those are on the district level at best, offering only around 20 data points - insufficient for granular analysis like the one undertaken here. One caveat thus on my main results (other than real estate prices) is that there are no pre-trends available. Another issue common to persistence studies such as this is that the intermediate period after World War II is treated as a black box (Redding and Sturm 2024; Rossi-Hansberg, Sarte, and Owens III 2010). I combat this issue by presenting a long data series for real estate prices, and discussing municipal policies in this period in great detail.

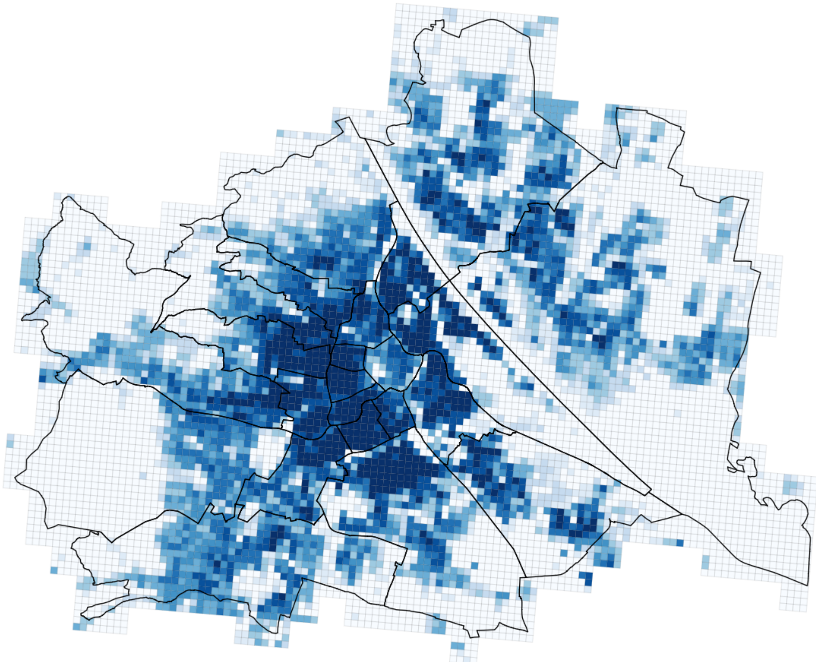
Additional (GIS) Data

I also incorporated other variables, including data on (public) amenities, landmarks and also data on public housing estates. These are available from the City of Vienna's Open Government Data (OGD) program and come already with detailed GIS links.[51]

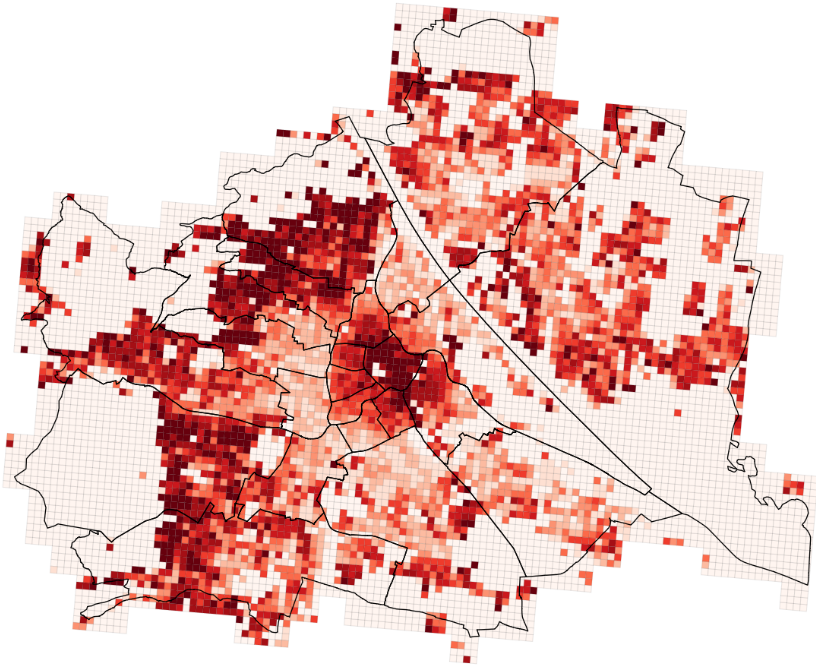
51. An overview of the OGD program for Vienna - including the complete data catalogue - can be found here: https://www.wien.gv.at/viennagis/datenquellen/.

Figure 3.3: Demographic Outcomes across Vienna

Panel A: Stylized Facts - Population Density per Grid-Cell



Panel B: Stylized Facts - Income Distribution per Grid-Cell



3.2.3 Empirical Framework

I use the Allied bombing of Vienna in World War II as a quasi-natural experiment to explore how changes in neighborhood dynamics impact socioeconomic outcomes (following Redding and Sturm (2024); and Ahlfeldt et al. (2015)). First, I estimate the following regression specification for the causal effect of wartime destruction on post-war (and as placebo test, pre-war) outcomes:

$$Y_i = \beta D_{i,war} + \delta_k + u_i, \tag{3.1}$$

where i indexes the 250 x 250 m grid-cells and k indexes 1 x 1 km grid-cells (or 2 x 2 km in my robustness specifications); Y_i is post-war (pre-war, as placebo test) housing, socioeconomic outcomes or property values; $D_{i,war}$ is wartime destruction; δ_k are fixed effects for 1 x 1 (2 x 2) km grid-cells; and u_i is a stochastic error.

In my baseline specification, and most others, above I report standard errors clustered by 1 km grid-cells (or 2 km in the robustness specifications). In the baseline regression, I also include a variable capturing the distance to the city center (as measured my km distance to St. Stephan's cathedral), in line with recent literature (Morawetz and Klaiber 2022; Musil et al. 2022).[52]

Lastly, I measure the spillover effect of wartime destruction using the built-up area of any adjacent grid-cells. These areas exclude the "treated" areas themselves, such that they form a circle around them. I estimate the following regression between a location's own post-war outcomes, its own wartime destruction, and the wartime destruction in these surrounding areas:

$$Y_i = \beta D_{i,war} + \sum_{g=1}^{G} \gamma D_{ig,war} + \delta_k + u_i, \tag{3.2}$$

where I index surrounding areas by $g \in 1, \ldots, G$; $D_{ig,war}$ is the fraction of the built-up area seriously damaged in the area g in surrounding location i; the other variables are defined above.

The identifying assumption in both frameworks above is that, once controlling for 1 km (2 km) grid-cell fixed-effects, the only difference driving the results is the effect of bombing on outcomes measured above. In the Viennese setting it is only possible to test this assumption for the outcome real estate prices, since there is only scarce data from the inter-war period on socioeconomic outcomes. As an initial balance test, I thus use the pre-WW2 real estate prices to check for pre-trends in my estimating framework: I estimate the regression from equation (1) using real estate prices from 1934 and 1939

52. As a robustness check I also run these regressions without this variable - the results are qualitatively similar.

as outcomes. In Table 3.1, I report the results of this analysis. There are no pre-trends between bombed and unbombed grid-cells, at least in terms of real estate prices. These results are also robust to varying the level of grid-cell fixed-effects (1 sq. km vs. 4 sq. km). Additionally, in Section 3.2.2 and Appendix C.3, I provide supporting evidence that wartime destruction can be viewed as quasi-random on the neighborhood level. Controlling for highly local, *"neighborhood"* fixed-effects should thus provide a framework which allows this identification to succeed.

To further test the robustness of my results, I vary the level of clustering (1 vs. 2 km), I vary the definition of the variable on wartime destruction from only strongly affected buildings to all bombed buildings and I move the area of the 1 x 1 km grid-cells, following Békés and Harasztosi (2018) to assess the importance of my definition of a neighborhood to the robustness of my results (the results to these are reported toward the end of the next section).

Table 3.1: Balance Test on Real Estate Prices

	Effect on Real Estate Prices (pre WW2)			
	1 sq. km FE		**4 sq. km FE**	
	1934	**1939**	**1934**	**1939**
	(I)	(II)	(III)	(IV)
Bomb Count	0.003	-0.017	0.015	0.008
	(0.021)	(0.011)	(0.017)	(0.009)
Distance (Center)	-0.251	0.011	-0.193	-0.109
	(0.262)	(0.159)	(0.148)	(0.081)
Construction	-0.158	-0.059	0.942	-0.177
	(0.191)	(0.121)	(0.966)	(0.140)
Constant	7.957***	6.762***	6.381***	7.825***
	(1.177)	(1.549)	(0.874)	(0.823)
FE	1 sq. km	1 sq. km	4 sq. km	4 sq. km
No. of Obs.	138	211	167	234
R-squared	0.61	0.51	0.40	0.40
Mean (dep. var.)	6.61	6.73	6.38	6.64

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the log price of real estate by sqm (normalized to 1991 ATS). The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

3.3 Empirical Results

In the following section, I present results for the three main outcomes of this chapter: First I show the effect of wartime destruction on housing, then I present results on demographics and lastly I examine the effect on firms and other local amenities.

3.3.1 Effect on Housing

The first step in understanding how Vienna rebuilt is to examine what happened to its housing stock. The regressions in the following are based on the main fixed-effects specification proposed in equation (1). Table 3.2 presents the effect of bomb damage on post-war housing outcomes using my main specification with 1 km (2 km) grid-cell fixed effects. The columns distinguish different outcomes Y_i. Even after accounting for location fixed-effects, grid cells that experienced greater wartime destruction ended up with more buildings, especially residential ones (Columns 1–2). They also contain more flats (Column 3), though only small changes appear in the composition of flat sizes (Columns 4–5). Taken together, the rebuilding effort did not simply restore what had been lost, it generated an increase in available housing.

To interpret these results, it is helpful to consider what would have happened had reconstruction simply replaced destruction one-for-one. In that case, the estimated coefficients would hover around zero. Instead, the positive effects imply that bombed areas became more densely built than they had been before and more dense than comparable undamaged areas. This increase in density translates primarily into additional flats, suggesting a shift toward more multi-unit buildings and high-rise housing forms. The data therefore indicate that bombing triggered not just recovery but a rethinking of urban planners toward denser, more modern residential architecture.

The magnitude of these changes is substantial. Comparing undamaged and destroyed grid cells (defined as having at least one fully destroyed building), the coefficients in Panel A imply a 3 percent increase in total buildings, a 4 percent increase in residential buildings, and a 10 percent increase in flats. While some of this may reflect the subdivision of larger units, this is not the dominant pattern. The broader story is one of within-building densification, likely driven by a combination of public policy and private redevelopment that aimed to alleviate acute housing shortages in the post-war years.

These core results hold up robustly. Panel B of Table 3.2, using 2 km fixed-effects, produces almost identical effect sizes and significance levels. To explore these patterns further, Appendix Table C.2 examines flat size directly. The results reveal a small increase in three-room flats and a small decline in very large flats (five rooms or more), consistent with a mild shift toward smaller units within an overall expansion of the housing stock. This reinforces the idea that reconstruction did not simply rebuild the old city, it subtly reshaped it.

A complementary perspective comes from studying the age of buildings in the city today. Table 3.3 provides a "survival"-type analysis of contemporary building ages (in 2013).

Table 3.2: Estimation of the Effect of WW2 Bombing on Housing

	Effect on Housing Outcomes				
	All Buildings	**Residential Buildings**	**Number of Flats**	**Share with 1-3 Rooms**	**Share with 5+ Rooms**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	1.446***	1.380***	23.908***	0.219**	-0.198**
	(0.253)	(0.189)	(3.573)	(0.105)	(0.081)
Distance (Center)	-3.396	-2.958	-55.735***	-2.989*	3.046**
	(2.529)	(2.410)	(18.297)	(1.619)	(1.395)
Constant	66.683***	57.192***	609.283***	66.651***	7.731
	(17.780)	(16.938)	(128.317)	(11.205)	(9.660)
No. of Obs.	4,187	4,187	4,187	3,937	3,937
R-squared	0.32	0.31	0.65	0.42	0.46
Mean (dep. var.)	44.03	37.56	237.70	46.18	28.61
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	1.331***	1.088***	21.158***	0.190*	-0.134
	(0.314)	(0.267)	(5.202)	(0.108)	(0.086)
Distance (Center)	-1.954	-0.606	-47.124***	-2.609**	2.452**
	(1.331)	(1.367)	(16.459)	(1.186)	(1.060)
Constant	56.475***	40.737***	551.027***	64.072***	11.793
	(9.405)	(9.646)	(115.144)	(8.268)	(7.389)
No. of Obs.	4,219	4,219	4,219	3,969	3,969
R-squared	0.19	0.17	0.54	0.30	0.34
Mean (dep. var.)	43.79	37.36	236.04	46.12	28.71

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of dwellings/the share of rooms. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Although the data do not allow me to trace each structure across time, they do show which periods are over-represented in areas that suffered more damage. With 1 km (2 km) fixed effects, bombed grid cells contain more pre-1919 buildings (Column 1), no difference in interwar buildings (Column 2), and more buildings from the immediate post-war era (Columns 3–4), but no differences for more recent construction (Columns 5–6). The estimates imply a 12 percent increase in pre-1919 survivors, a 5.5 percent increase in 1945–1960 buildings, and a 3.3 percent increase in 1961–1980 buildings. This mix of old and new reflects how bombing selectively removed some structures while sparing others, but also subsequent policy choices. In particular, the high number of pre-1919 housing might seem counter-intuitive. However, it reflects the very particular rebuilding policies in Vienna after the war (i.e. subsidizing the reconstruction of housing of the 1870-1910 period, see section 3.4.1).

Taken together, these results suggest that bombed areas did not languish or remain empty but instead underwent a rapid and lasting reconstruction process. The coexistence of more historic buildings and additional post-war buildings points to a pattern of selective destruction and equally selective rebuilding. Many historic buildings survived or were restored, while new ones filled surrounding gaps. The resulting urban landscape of dense, mixed-age buildings is a hallmark of Vienna's post-war development and helps explain why these neighborhoods remain economically and demographically diverse today.

To understand which actors drove this transformation, I next examine the role of public housing. Table 3.4 shows the effect of wartime destruction on the presence of public housing estates. Both with 1 km and 2 km fixed effects, bombed areas saw a significant rise in public housing built between 1945 and 1960 (Column 3), but no increase in pre-war or late post-war periods (Columns 2, 4–5). The coefficients imply an 18 percent increase in post-war public housing estates in bombed areas. This indicates that municipal authorities played a central role in the initial reconstruction push, deploying public housing as a stabilizing force in heavily damaged districts.

However, when turning from those housing estates to public-housing flats, a different picture appears. Table 3.5 shows two central facts: (1) around one quarter of all flats in the city are public housing units, with roughly one third built pre-war and two thirds post-war; (2) although wartime destruction significantly increased the number of public housing estates, it did not significantly increase the number of public-housing flats (in bombed locations). This means that the large post-war rise in total flats (Table 3.2) must have been driven primarily by private or cooperative construction, not municipal building programs.

This distinction is central to the broader narrative of this chapter. Post-war reconstruction in bombed areas was not solely a public-sector story. The data show that private rebuilding, often encouraged by zoning reforms and densification incentives (Section 3.4.1), played a crucial role in reshaping these neighborhoods. While this surge in private construction boosted housing supply, it also produced a stock that was denser and, in many cases, more standardized than the pre-war one. The result is a trade-off that still shapes these areas today: Bombed neighborhoods became more densely populated and architecturally heterogeneous, but their housing stock also reflects the pressures and compromises of rapid reconstruction. These changes persist into the present, influencing who lives in these areas, how local services developed, and how the urban economy adjusted around them.

Table 3.3: Estimation of the Effect of WW2 Bombing on Buildings (by date)

	Effect on the age of buildings					
	pre-1919	**1919-1945**	**1945-1960**	**1961-1980**	**1981-2000**	**2001-2015**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	0.955***	-0.001	0.285***	0.309***	0.009	-0.050
	(0.159)	(0.063)	(0.048)	(0.056)	(0.035)	(0.031)
Distance (Center)	-2.502***	-1.349	0.343	-0.699	0.008	1.111*
	(0.771)	(1.308)	(0.554)	(0.764)	(0.720)	(0.578)
Constant	24.361***	15.739*	2.555	13.845***	6.027	-3.036
	(5.337)	(9.102)	(3.854)	(5.321)	(5.010)	(4.023)
No. of Obs.	4,044	4,044	4,044	4,044	4,044	4,044
R-squared	0.68	0.24	0.25	0.27	0.25	0.23
Mean (dep. var.)	7.80	6.36	5.19	9.25	6.09	4.64
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	0.955***	-0.005	0.241***	0.248***	-0.029	-0.092**
	(0.208)	(0.070)	(0.048)	(0.062)	(0.048)	(0.037)
Distance (Center)	-2.513***	-0.460	0.819*	0.307	0.188	0.078
	(0.615)	(0.676)	(0.444)	(0.527)	(0.438)	(0.279)
Constant	24.465***	9.540**	-0.753	6.857*	4.793	4.167**
	(4.274)	(4.746)	(3.116)	(3.698)	(3.072)	(1.956)
No. of Obs.	4,073	4,073	4,073	4,073	4,073	4,073
R-squared	0.60	0.11	0.12	0.14	0.13	0.13
Mean (dep. var.)	7.75	6.32	5.17	9.22	6.08	4.63

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of buildings (by build date). The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

3.3.2 Effect on Real-Estate Prices

A central question to follow the results on housing would be that of the quality of those flats. Unfortunately, data on quality is hard to come by in the historical time-frame of this study. There are aggregate statistics showing an upgrade in quality immediately after World War II and in the 1980s following a change in housing policy. A more crude measure of the desirability of housing are it's price.

Table 3.6 examines whether the reconstruction dynamics documented above translated into meaningful changes in the price of land, buildings, or flats. This allows us to assess not only how neighborhoods rebuilt, but whether the market capitalized or penalized

Table 3.4: Estimation of the Effect of WW2 Bombing on Public Housing

	Effect on Public Housing Estates				
	All P.H.	**pre-WW2**	**1945-1960**	**1961-1980**	**1981-2010**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	0.043***	0.002	0.022***	0.009*	0.010*
	(0.014)	(0.006)	(0.007)	(0.005)	(0.006)
Distance (Center)	-0.164**	-0.067	-0.033	-0.017	-0.047**
	(0.072)	(0.048)	(0.027)	(0.025)	(0.019)
Constant	1.540***	0.588*	0.336*	0.220	0.396***
	(0.502)	(0.335)	(0.189)	(0.175)	(0.131)
No. of Obs.	4,188	4,188	4,188	4,188	4,188
R-squared	0.29	0.20	0.14	0.19	0.21
Mean (dep. var.)	0.42	0.12	0.12	0.11	0.07
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	0.030*	0.001	0.015***	0.007	0.006
	(0.018)	(0.006)	(0.005)	(0.006)	(0.007)
Distance (Center)	-0.077	-0.065	0.013	-0.006	-0.020
	(0.068)	(0.042)	(0.022)	(0.018)	(0.013)
Constant	0.942*	0.575*	0.017	0.143	0.207**
	(0.478)	(0.295)	(0.152)	(0.126)	(0.092)
No. of Obs.	4,220	4,220	4,220	4,220	4,220
R-squared	0.19	0.11	0.08	0.10	0.11
Mean (dep. var.)	0.42	0.12	0.12	0.11	0.07

*Notes:*Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of public housing estates. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

areas that were more heavily damaged. Using my main specification with 1 km (and later 2 km) grid-cell fixed effects, I compare real estate prices for both the pre-war years (1934, 1939) and the post-war decades (1951, 1961, 1991). Across all periods, the pattern is remarkably stable: Grid cells that experienced greater wartime destruction show no discernible increase or decrease in prices. Columns (1)–(5) all yield statistically and economically insignificant estimates. When contrasting undamaged grid cells with those that had at least one completely destroyed building, the implied effects remain small: Less than a two percent increase, and not significant at conventional levels. In other words, despite dramatic changes in built density, reconstruction did not translate into systematic price differences between bombed and unbombed areas. This suggests that

Table 3.5: Estimation of the Effect of WW2 Bombing on Public Housing

	Effect on Public Housing Flats				
	All P.H.	**pre-WW2**	**1945-1960**	**1961-1980**	**1981-2010**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	-0.569	-0.804	0.082	-0.020	0.173
	(1.444)	(1.174)	(0.767)	(0.339)	(0.196)
Distance (Center)	-10.644	-25.110	5.776	9.480	-0.790
	(35.579)	(29.352)	(10.510)	(15.268)	(2.149)
Constant	147.196	200.522	-21.118	-42.830	10.622
	(250.026)	(206.459)	(73.503)	(107.383)	(15.083)
No. of Obs.	4,188	4,188	4,188	4,188	4,188
R-squared	0.15	0.16	0.07	0.12	0.08
Mean (dep. var.)	71.94	23.44	19.53	23.75	5.22
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	-1.768	-0.899	-0.647	-0.100	-0.122
	(1.799)	(1.373)	(0.663)	(0.586)	(0.245)
Distance (Center)	-1.382	-17.638	12.052	6.961	-2.757**
	(35.435)	(24.110)	(14.532)	(12.077)	(1.280)
Constant	82.618	148.475	-65.130	-25.462	24.734***
	(250.544)	(170.884)	(102.278)	(85.595)	(9.061)
No. of Obs.	4,220	4,220	4,220	4,220	4,220
R-squared	0.05	0.05	0.02	0.06	0.03
Mean (dep. var.)	71.40	23.26	19.38	23.57	5.18

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of public housing flats. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

the expansion of housing supply in affected neighborhoods occurred without reducing overall quality or making these areas less desirable in the long run.

The results above capture the indirect effect of destruction on prices, i.e., how neighborhoods fared after the rebuilding process had unfolded. To complement this, I follow Haider, Lampe, and Rieder (2021) and turn to hedonic regressions that isolate the direct relationship between wartime destruction and prices, controlling explicitly for distance to the center, amenities, and wartime-specific factors such as aryanization, restitution, and bomb damage. Table C.3 splits the analysis by contract type (land, buildings, flats). Here the picture becomes more nuanced: While the effect of bombing on building and flat prices remains insignificant (Columns (1) and (2)), land prices respond quite differ-

Table 3.6: Estimation of the Effect of WW2 Bombing on Real Estate Prices

	Effect on Real Estate Prices (around WW2)				
	1934	**1939**	**1951**	**1961**	**1991**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	0.003	-0.017	0.006	0.020	-0.005
	(0.021)	(0.011)	(0.015)	(0.013)	(0.013)
Distance (Center)	-0.251	0.011	-0.139*	0.013	0.057*
	(0.262)	(0.159)	(0.076)	(0.127)	(0.032)
Construction	-0.158	-0.059	-0.201	-0.349	-0.641***
	(0.191)	(0.121)	(0.237)	(0.435)	(0.156)
Constant	7.957***	6.762***	8.197***	5.958	5.318***
	(1.177)	(1.549)	(1.693)	(4.017)	(2.001)
No. of Obs.	138	211	302	320	595
R-squared	0.61	0.51	0.36	0.31	0.31
Mean (dep. var.)	6.61	6.73	5.07	6.16	8.24
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	0.015	0.008	0.024	0.018*	0.014
	(0.017)	(0.009)	(0.015)	(0.010)	(0.010)
Distance (Center)	-0.193	-0.109	-0.110**	0.037	0.032
	(0.148)	(0.081)	(0.049)	(0.103)	(0.040)
Construction	0.942	-0.177	-0.179	-0.462	-0.942***
	(0.966)	(0.140)	(0.225)	(0.308)	(0.152)
Constant	6.381***	7.825***	7.425***	5.309	7.021***
	(0.874)	(0.823)	(1.145)	(3.197)	(2.506)
No. of Obs.	167	234	321	347	629
R-squared	0.40	0.40	0.21	0.22	0.21
Mean (dep. var.)	6.38	6.64	5.03	6.15	8.18

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the log price of real estate by sqm (normalized to 1991 ATS). The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

ently. Columns (3) and (4) show a large and statistically significant positive effect of bomb damage on land prices, indicating that destruction created opportunities for redevelopment that the market for land recognized, even if the prices of individual structures did not immediately reflect these changes.

A similar pattern emerges when I split the estimation by contract year in Table C.4.

Breaking the sample into 1951, 1961, and 1991 contracts, and again controlling for distance, wartime issues, and contract type, I find a strong and persistent positive effect of bomb damage on real estate prices across all three decades. These results imply that the market priced in the redevelopment potential of bombed land well beyond the immediate post-war years.[53]

Taken together, these findings paint a coherent picture: Despite large increases in density, bombed neighborhoods did not suffer a price penalty, if anything, land in these areas became more valuable, reflecting the flexibility and redevelopment opportunities created by destruction. In the broader narrative of Vienna's reconstruction, this reinforces the idea of an expansion-led growth: Density rose without diminishing quality, and the land market responded not by discounting damaged areas, but by capitalizing on the new scope for rebuilding.

3.3.3 Effect on Socioeconomic Outcomes

To understand how physical reconstruction translated into social change, I next examine the long-run demographic footprint of wartime destruction.

First, Table 3.7 summarizes the effects on key population outcomes using my main specification with 1 km (and later 2 km) grid-cell fixed effects. The first striking pattern is that bombed areas today host more people: Column (1) shows a significant increase in total population in grid cells that experienced heavier destruction. The remaining columns document only small, though statistically significant, differences in relationship status (Columns (2)–(3)), gender (Column (4)), and citizenship (Columns (5)–(6)), suggesting that the large population rebound did not coincide with a major change in these basic demographic characteristics.

The magnitude of the population effect is economically important. Comparing undamaged and (partially) destroyed grid cells, the coefficient in Panel A implies a 9 percent increase in total residents. The smaller adjustments in relationship status, gender, and citizenship, aside from a modest decline in Austrian citizens offset by a rise in EU citizens, indicate that the demographic recovery was broad-based. These results reinforce the narrative emerging from the analysis of housing: Bombed neighborhoods were not left behind but rebuilt and repopulated, ultimately becoming denser and still-attractive parts of the city. The slight tilt toward more non-Austrian EU citizens hints at a subtle change in the social mix, perhaps reflecting differential migration patterns into affordable, reconstructed housing in the post-war decades.

To add to this picture, I turn to additional demographic characteristics in Appendix Tables C.5–C.8. These show that bombed areas are today populated by a somewhat younger population, with a small but significant rise in residents aged 15–34, and a slightly more educated population due to a modest increase in university graduates

53. I do not split the sample in this specification by contract type, for reasons of sample size. However, the results in Table C.3 suggest that this is in large part driven by the effect on land prices.

Table 3.7: Estimation of the Effect of WW2 Bombing on Demographic Composition

	Effect on Demographics					
	Total Population	**Share of Population (in %)**				
		Single	**Married**	**Female**	**Austrian**	**EU-Citizen**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	38.457***	0.075*	-0.030	-0.049*	-0.182**	0.124***
	(6.266)	(0.040)	(0.032)	(0.029)	(0.090)	(0.037)
Distance (Center)	-91.329***	-0.468	0.877	-0.228	2.211***	-0.275
	(32.719)	(0.658)	(0.702)	(0.402)	(0.853)	(0.457)
Constant	1036.409***	44.175***	36.885***	53.275***	61.313***	12.246***
	(229.704)	(4.565)	(4.874)	(2.792)	(5.918)	(3.170)
No. of Obs.	4,188	3,951	3,951	3,951	3,951	3,951
R-squared	0.63	0.32	0.31	0.18	0.45	0.28
Mean (dep. var.)	426.95	41.00	42.94	51.65	76.48	10.45
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	32.036***	0.132**	-0.042	-0.028	-0.290***	0.168***
	(9.449)	(0.051)	(0.033)	(0.033)	(0.106)	(0.039)
Distance (Center)	-83.002***	-0.947**	1.541***	-0.017	1.553**	-0.160
	(29.787)	(0.458)	(0.434)	(0.216)	(0.601)	(0.271)
Constant	983.038***	47.446***	32.312***	51.750***	65.930***	11.452***
	(208.601)	(3.192)	(3.038)	(1.508)	(4.191)	(1.893)
No. of Obs.	4,220	3,981	3,981	3,981	3,981	3,981
R-squared	0.50	0.19	0.20	0.08	0.34	0.15
Mean (dep. var.)	423.91	40.96	43.01	51.60	76.49	10.49

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is total population and population shares, respectively. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

(offset by a decline in those with apprenticeships). Citizenship data indicate that the earlier shift is driven largely by an influx of people from Eastern European EU member states. In summary, these movements suggest a gradual, long-run process of social renewal, somewhere between mild gentrification and demographic diversification, that unfolded within the reconstructed districts.

Second, I examine the economic side of this social recovery. Table 3.8 analyzes incomes using the same fixed-effects specification. Here, too, the results tell a story of convergence rather than divergence. While grid cells that suffered greater destruction show a very small (but statistically significant) reduction in truncated income of around 0.2 percent, they exhibit no meaningful differences in raw income measures (Columns (1) and (4)). Total income per grid cell rises (Columns (3) and (6)), in line with the increase in population. The overall take-away seems that rebuilt areas did not become low-income neighborhoods; instead, they achieved income levels broadly comparable to those of undamaged neighborhoods despite their wartime disruption.

Income inequality offers a complementary angle. Table 3.9 shows that bombed grid cells display only a marginally higher Gini Index, however this result is not robust across specifications. This suggests that reconstruction did not generate persistent pockets of inequality at the neighborhood level. Evidence in the appendix on labor outcomes (Table C.9) and sectoral employment (Appendix Tables C.10–C.11) echoes this broader pattern: War-affected areas today host slightly more self-employed workers but exhibit no major differences in employment composition or sectoral structure.[54]

From this, a consistent story emerges. The neighborhoods most damaged by wartime bombing did not fall into long-term demographic or economic decline. Instead, they became denser, younger, somewhat more diverse, and economically similar to their undamaged counterparts. These findings highlight an important part of my broader argument: Reconstruction in Vienna not only rebuilt the physical environment but also reshaped the city's social structure in subtle, persistent ways: It showed the city's capacity to absorb a massive shock and reconstitute dense, lived-in neighborhoods.

Lastly, I turn to the question of spillovers: Whether the impact of wartime destruction extended beyond the grid cells that were actually hit. To do this, I run regressions based on the spillover fixed-effects specification proposed in equation (2).

Appendix Table C.16 presents these results using this specification with 1 km (2 km) fixed effects. Each column reports a different post-war demographic outcome Y_i, and each row separates the direct effect ("own destruction") from the effect of destruction in neighboring grid cells. Columns (1), (3), and (5) reproduce the direct effects from Table 3.7, while Columns (2), (4), and (6) introduce destruction within 250 meters of each cell. This setup allows us to see not only how bombed neighborhoods evolved, but also whether nearby areas were pulled into the same trajectory of recovery.

Across the main demographic outcomes, the picture that emerges is one of highly localized effects. Once destruction in adjacent cells is added, the coefficient on own destruction generally becomes smaller, indicating that some variation is indeed shared across space. But the spillover patterns themselves are limited. For population, there is no significant effect from neighboring destruction; for citizenship, however, the small shift away from Austrian residents toward EU citizens remains significant. For income (Appendix Table C.17), controlling for neighboring destruction removes the significance of the direct effect, suggesting that income responses diffused weakly across space. Yet the rise in total incomes at the grid-cell level remains large and stable, underscoring that rebuilt areas regained economic influence even if per-capita measures were more spatially intertwined.

54. This pattern complements the firm-level results below: Rebuilt areas appear to support small-scale, service-oriented local economies with many self-employed operators.

Table 3.8: Estimation of the Effect of WW2 Bombing on Income

	Effect on the Amount of Income (in Euros)					
	Income	**Inc (Tr.)**	**Sum (Inc.)**	**Net inc.**	**Net (Tr.)**	**Sum (Net.)**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	-91.61	-57.17*	659,187***	-65.06	-35.36	535,145***
	(67.99)	(34.41)	(100,159)	(42.90)	(23.17)	(82,386)
Distance (Center)	1,422.88	1,232.11*	-1,847,961***	1,027.49*	863.97*	-1,486,474***
	(973.72)	(637.66)	(669,078)	(608.21)	(449.92)	(546,148)
Constant	23,045.08***	18,050.31***	21,158,972***	18,888.52***	16,806.15***	17,137,957***
	(6,727.29)	(4,403.80)	(4,615,848)	(4,199.37)	(3,107.32)	(3,766,667)
No. of Obs.	3,859	3,859	3,859	3,859	3,859	3,859
R-squared	0.40	0.31	0.58	0.38	0.30	0.58
Mean (dep. var.)	32,777	26,498	9,008,465	25,917	22,734	7,368,705
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	-53.13	-68.51*	547,540***	-44.70	-45.54*	440,447***
	(51.35)	(35.17)	(144,816)	(31.84)	(23.92)	(121,909)
Distance (Center)	673.62	579.76	-1,407,282***	423.66	389.09	-1,146,713***
	(722.56)	(382.21)	(506,411)	(432.03)	(249.55)	(431,751)
Constant	28,189***	22,578***	18,202,781***	23,042***	20,102***	14,865,265***
	(5,016.85)	(2,650.07)	(3,493,674)	(2,999.34)	(1,730.49)	(2,976,279)
No. of Obs.	3,892	3,892	3,892	3,892	3,892	3,892
R-squared	0.28	0.21	0.47	0.27	0.20	0.46
Mean (dep. var.)	32,811	26,536	8,939,368	25,939	22,759	7,311,911

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is income in 2013 Euros. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

3.3.4 Effect on Firm Outcomes

To complement the analysis of population and housing, I next examine how local economic activity evolved in neighborhoods affected by bombing. Table 3.10 summarizes the effects of wartime destruction on firm outcomes using my main specification with 1 km (and later 2 km) grid-cell fixed effects. The results point to a clear pattern of economic resurgence: Grid cells that suffered heavier bombing today host more firms (Column (1)) and employ more workers (Column (2)). In other words, these neighborhoods did not merely rebuild physically, they also regained their role as centers of urban economic life.

The composition of this rebuilding is equally important. The increase in firms is driven

Table 3.9: Estimation of the Effect of WW2 Bombing on Inequality (Gini-Index)

	Effect on Gini-Index (Inequality)	
	Mean of net inequality	**Gini-Index (0-1)**
	(I)	(II)
PANEL A. 1 sq. km grid-cell fixed-effects		
Bomb Count	-67.077	0.000
	(43.055)	(0.000)
Distance (Center)	801.527	-0.003
	(635.653)	(0.005)
Constant	20568.847***	0.378***
	(4219.944)	(0.034)
No. of Obs.	3,701	3,701
R-squared	0.37	0.38
Mean (dep. var.)	25821.29	0.36
PANEL B. 4 sq. km grid-cell fixed-effects		
Bomb Count	-51.324	0.001**
	(31.957)	(0.000)
Distance (Center)	292.571	-0.002
	(449.808)	(0.004)
Constant	23901.363***	0.372***
	(2997.695)	(0.025)
No. of Obs.	3,725	3,725
R-squared	0.25	0.27
Mean (dep. var.)	25799.92	0.36

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variables are income in 2013 Euros and the Gini-index, with values from 0 to 1. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

primarily by small and medium-sized enterprises (Columns (3)–(5)), with no detectable effect on large firms (Column (6)).[55] This suggests that the post-war economic landscape that emerged in bombed areas was not one of large industrial relocation or major corporate investment. Instead, reconstruction appears to have fostered a more neighborhood-based economy, that was built around smaller firms embedded in local demand and community. This resonates with the broader story of Vienna's post-war trajectory: Economic recovery took place block-by-block, often led by local businesses rather than

55. Appendix Table C.12 shows similarly modest but significant shifts in firm structure, reinforcing this pattern.

large-scale industrial actors.

A natural question is whether this economic resurgence reflects deeper agglomeration forces or simply tracks population growth. Column (7) helps disentangle this by looking at firms per capita. Once the size of the resident population is taken into account, the effect of destruction on the number of firms becomes insignificant. This indicates that the rise in firms was driven primarily by the return of people and the accompanying demand for goods and services, rather than by the formation of productivity-enhancing clusters. Put differently, economic recovery followed demographic recovery, maintaining a stable ratio of firms to residents.

To explore the nature of these firms, I turn to sectoral employment in Appendix Tables C.13 and C.14. Here, too, the results point to continuity rather than dramatic transformation. Bombed areas did not evolve into new industrial hubs. Instead, they remained, and gradually reestablished themselves, as mixed-use service-oriented urban districts. The small shifts within services, especially in transport and hospitality, are consistent with neighborhoods that regained their pre-war character: Walkable, residential, and supported by a network of everyday businesses.

On a higher level, the firm-level evidence ties directly into the larger reconstruction narrative. Wartime destruction did not leave behind hollowed-out local economies. Instead, these neighborhoods rebuilt in ways that were consistent with their traditional urban form: Denser housing, more residents, and a number of small and medium enterprises. This small-scale, service-driven recovery highlights that post-war reconstruction in Vienna was not just about replacing buildings, it was about restoring the social and economic life of neighborhoods, allowing them to both renew but keep their character.

Expanding the local view again, I turn towards spillovers in the results on firms. Appendix Table C.18 shows an interesting story: The effect of own destruction again weakens once neighboring wartime damage is included, but importantly, it stays statistically significant for both firm counts and worker numbers. Moreover, only firms display strong spillover effects: Neighboring destruction significantly predicts more firms in a given cell, whereas the spillover for workers is not significant. Additional analysis shows that these spillovers are driven largely by small and medium enterprises (those with fewer than five employees), pointing to a process where the local businesses expanded outward from bombed zones, perhaps as reconstruction created new commercial opportunities along the edges of damaged areas.

3.3.5 Effect on (Public) Amenities

After examining changes in housing and economic activity, the next step is to understand how everyday urban life was rebuilt. To do this, I turn to the (public) provision of amenities. Table 3.11 reports the effect of wartime destruction on key neighborhood services: kindergartens (Column 1), schools (Column 2), pharmacies (Column 3), all doctors (Column 4), and general practitioners (Column 5). Across both the 1 km and 2

Table 3.10: Estimation of the Effect of WW2 Bombing on Firms

	Effect on Firms (and Firm Size)						
	Firms	**Workers**	**Size 0-1**	**Size** < 5	**Size** < 100	**Size** > 100	**Firms/1000**
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
PANEL A. 1 sq. km grid-cell fixed-effects							
Bomb Count	4.200***	17.114**	2.468***	0.884***	0.679***	0.010	-27.593
	(0.847)	(7.531)	(0.442)	(0.231)	(0.211)	(0.013)	(20.750)
Distance (Center)	-15.594***	-99.538***	-10.066***	-4.040***	-3.536***	-0.138**	358.054
	(4.010)	(29.614)	(3.133)	(0.944)	(1.012)	(0.068)	(338.225)
Constant	139.481***	904.053***	89.164***	34.403***	29.760***	1.261***	-2059.043
	(27.361)	(201.513)	(20.640)	(6.127)	(6.577)	(0.443)	(2303.676)
No. of Obs.	3,832	3,832	3,325	3,325	3,325	3,325	3,832
R-squared	0.67	0.49	0.56	0.68	0.61	0.37	0.08
Mean (dep. var.)	36.38	237.08	25.43	8.71	7.18	0.36	371.19
PANEL B. 4 sq. km grid-cell fixed-effects							
Bomb Count	4.192***	18.492*	2.240***	0.983***	0.780**	0.016	-21.130
	(1.272)	(9.603)	(0.662)	(0.318)	(0.338)	(0.020)	(17.639)
Distance (Center)	-12.419***	-91.510***	-7.051***	-3.456***	-3.248***	-0.128**	275.957
	(2.460)	(31.939)	(1.494)	(0.766)	(0.924)	(0.058)	(184.142)
Constant	117.868***	849.272***	69.543***	30.472***	27.782***	1.187***	-1510.247
	(17.037)	(217.047)	(9.978)	(5.051)	(6.031)	(0.376)	(1256.845)
No. of Obs.	3,864	3,864	3,343	3,343	3,343	3,343	3,864
R-squared	0.62	0.39	0.52	0.61	0.50	0.27	0.03
Mean (dep. var.)	36.10	235.54	25.31	8.67	7.15	0.36	371.91

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of firms/workers. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

km fixed-effect specifications, bombed areas show a statistically significant rise in each of these amenities. Some of them, like kindergartens and medical services, increasing quite substantially.[56] These patterns indicate that reconstruction was not just about replacing lost buildings: It involved rebuilding the institutions that supports daily life.

The broader picture that emerges is one of neighborhoods actively reconstituting themselves as functional, lived-in urban spaces. The rise in amenities closely parallels the increases in residential density described earlier. As people returned or new residents moved into reconstructed districts, both public and private providers expanded their business, filling in the gaps left by wartime destruction. In this sense, the rebuilding

56. Appendix Table C.15 shows that a considerable share of this increase is driven by private schools and kindergartens.

of services mirrors the rebuilding of housing: Both reflect a process through which destroyed neighborhoods rebuilt their place in the city's social and economic structure.

Table 3.11: Estimation of the Effect of WW2 Bombing on (Public) Amenities

	Effect on the Provision of Amenities				
	Kindergarten	**Schools**	**Pharmacies**	**Doctors**	**GPs**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	0.041***	0.013*	0.012***	0.210	0.066***
	(0.010)	(0.007)	(0.003)	(0.148)	(0.024)
Distance (Center)	-0.144***	-0.059*	-0.045***	-1.024**	-0.195**
	(0.049)	(0.034)	(0.014)	(0.473)	(0.086)
Constant	1.358***	0.591**	0.387***	8.730**	1.780***
	(0.342)	(0.243)	(0.101)	(3.401)	(0.613)
No. of Obs.	4,188	4,188	4,188	4,188	4,188
R-squared	0.28	0.17	0.16	0.37	0.36
Mean (dep. var.)	0.38	0.19	0.08	1.71	0.47
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	0.037***	0.018**	0.012***	0.396***	0.084***
	(0.009)	(0.008)	(0.004)	(0.120)	(0.025)
Distance (Center)	-0.067**	-0.067***	-0.026***	-0.634*	-0.196***
	(0.030)	(0.019)	(0.007)	(0.321)	(0.053)
Constant	0.818***	0.644***	0.249***	5.845**	1.779***
	(0.210)	(0.138)	(0.053)	(2.319)	(0.383)
No. of Obs.	4,220	4,220	4,220	4,220	4,220
R-squared	0.21	0.10	0.13	0.30	0.31
Mean (dep. var.)	0.38	0.19	0.08	1.70	0.46

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of local amenities (displayed in the first row). The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Yet this expansion has important limits. Table 3.12, which scales amenities by population (per 1,000 inhabitants), reveals that these increases do not persist once we account for the number of residents. In both Panels A and B, the effect of bomb damage becomes insignificant when amenities are measured relative to population. The data therefore suggest that service provision kept pace with demographic recovery rather than exceeding it.

This distinction clarifies the underlying mechanism. The growth in amenities was proportional to population growth, not a sign of agglomeration, oversupply or specialization

Table 3.12: Estimation of the Effect of WW2 Bombing on Amenities (per capita)

	Effect on Provision of Amenities (per 1000 Inhabitants)				
	Kindergarten	**Schools**	**Pharmacies**	**Doctors**	**GPs**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	-0.030	-0.027	0.008	-0.236	0.020
	(0.048)	(0.049)	(0.020)	(0.184)	(0.069)
Distance (Center)	2.462	3.357	1.143	1.506	3.656
	(3.509)	(3.633)	(1.788)	(5.577)	(3.535)
Constant	-14.851	-21.200	-7.135	-1.857	-23.475
	(24.678)	(25.554)	(12.579)	(39.243)	(24.866)
No. of Obs.	4,188	4,188	4,188	4,188	4,188
R-squared	0.07	0.06	0.07	0.07	0.06
Mean (dep. var.)	2.42	2.36	0.90	8.52	2.22
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	-0.083*	-0.105*	0.000	0.024	-0.002
	(0.044)	(0.053)	(0.019)	(0.202)	(0.059)
Distance (Center)	0.273	-1.392	0.584	-3.847	-0.863
	(0.969)	(1.328)	(0.507)	(3.172)	(0.893)
Constant	0.544	12.251	-3.226	35.603	8.305
	(6.857)	(9.405)	(3.586)	(22.489)	(6.317)
No. of Obs.	4,220	4,220	4,220	4,220	4,220
R-squared	0.02	0.02	0.02	0.02	0.02
Mean (dep. var.)	2.40	2.34	0.90	8.48	2.22

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of local amenities per 1000 inhabitants. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

in services. Rebuilt areas did not become unusually well-served districts, they rather became appropriately served districts (with respect to their population growth). This supports the chapter's broader argument: Wartime destruction set in motion a reconstruction process that produced denser, functioning neighborhoods, but without generating exceptional concentrations of public or private amenities. What ultimately emerged were balanced communities that restored normal urban life rather than transforming into new nodes of service-based agglomeration.

Finally, Appendix Table C.19 explores whether services such as kindergartens, schools, and doctors spilled over in surrounding areas. Here, the results are less stable: The sign and significance of spillover coefficients vary across specifications and depend on the level of fixed effects used. What does hold consistently, is that the coefficient on own destruction becomes smaller when adjacent destruction is included, mirroring the broader pattern found for demographics and firms. But unlike in the case of firms, the spillovers themselves are small. Amenities remain strongly tied to the directly affected locations, suggesting that most of the adjustment after the war occurred within the reconstructed neighborhoods rather than moving outward.

3.3.6 Robustness

One major part of the robustness of my results already is reported in the main parts of the results: Varying the size of grid-cell fixed-effects from 1 x 1 km to 2 x 2 km in size allows me to assess how sensitive and localized the effects actually are. The closeness of these results to each other, both in statistical significance and size, is reassuring.

Second, I vary the exact location of the grid to test the robustness of my (or rather Statistik Austria's) definition of a neighborhood. Using a recent methodological contribution by Békés and Harasztosi (2018). I "shake" my grid cells in each direction by 250, 500, 750 meters and re-estimate the main specification: Appendix Figure C.3 shows the results of this exercise as an example for the main income estimates from Column (2) of Table 3.8. Both these results and others (not reported here) are reassuring as there seems only little dependence of the results on the grid placement.

Third, to further explore the robustness of my inference, I examine the spatial structure of my errors: Looking at the 250 x 250 km grid, cells may behave very differently depending on whether they are at the corner or the center of the larger 1km grid. The fixed effect removes the mean across the entire 1km grid, but not spatial variation within it. Thus, I plot the mean of residuals on 4 x 4 cell grids to see if there are any spatial pattern (e.g., darker spots in the middle). I repeat the same procedure for squared residuals to check variance patterns. In Appendix Figure C.4 the results of this exercise are displayed for two main outcomes, population and income. There are no meaningful patterns visible based on the larger grids, on which fixed-effects are based.

Lastly, in results not reported here, I vary the definition of the variable on wartime destruction from only strongly affected buildings to all bomb damage (including light damage) - these are not different in economic and statistical significance from the main results, potentially not surprisingly due to the high spatial correlation of these types of damage.

3.4 Discussion of Main Results

3.4.1 Housing Policies and Occupant Sorting

This section takes stock of the results and discusses their impact compared to the previous literature. Firstly, the results on housing, in particular the increase in buildings and flats, is of great importance. In past research (Redding and Sturm 2024; Takeda and Yamagishi 2024; Hornbeck and Keniston 2017) the direct effects of wartime destruction manifest as changes in building types: In the case of Vienna, all evidence points towards an explanation where wartime destruction in the same location increases the number of buildings being within the pre-war building footprint (i.e. more dense built-up areas) and increases building height.

Other research confirms this suggestive evidence. In particular, Zhu et al. (2025) explore how the bombings of World War II influenced the long-term redevelopment of Vienna. Drawing on detailed *parcel-level data*, they analyze how wartime destruction affected post-war urban growth—focusing on changes in building heights, patterns of property ownership, and population density. The findings show that areas hit by bombs tended to develop taller buildings and exhibited greater fragmentation in private ownership. Moreover, variations in population density between bombed and unbombed neighborhoods point to lasting transformations in the city's spatial structure. This is consistent with both post-war reconstruction priorities (see Appendix D for a detailed description of the city's policy) and architectural trends (high-rises surrounded by greener areas).[57] Meanwhile, wartime destruction seems to have no effect on the types of buildings in the unaffected locations.

Interestingly however, the results show that the impact of wartime destruction *does not result* in substantial alterations to the proportion of households residing in council housing (see the contrast of Table 3.4 and Table 3.5). From 1945 to 1980, there is quite a substantial increase in public housing estates but not in housing units (flats) constructed in areas affected by the bombings. This shows the differences compared to results from recent studies in other settings (e.g. Redding and Sturm (2024)) that find large effects of wartime destruction being mediated through public or council housing. The most obvious explanation is the persistence of the institutional context in Vienna: There was already a strong building program in place pre-war (one third of public housing was built 1919-1934) but also the *strong focus on preserving the historic fabric and socioeconomic composition* of the city. In particular, the municipal and federal reconstruction programs focused on the *"restoration of damaged and destroyed residential buildings. Interest-free loans were initially granted for 100 years, later for 75 or 50 years"* (Österreichisches Staatsarchiv 2025). Many partially or heavily damaged buildings still carry this legacy until today, both in the fact that the rent-control framework for these houses is rooted in their reconstruction with the help of government funds and also with commemorative plaques (see Appendix Figure C.5). This presents a major difference to other post-war rebuilding efforts where the focus was mostly on providing cheap, public or council housing.

Additionally, I display evidence showing that the impact of wartime destruction is happening mainly through residential rather than commercial buildings. In Column (2) of

57. The city government's stated policy of less dense city blocks looks inconsistent with my results only on the face of it: Their policy featured taller, more densely populated buildings while also increasing the level of green areas around them.

Table 3.2, I show a positive and statistically significant effect of wartime destruction on residential buildings, with no evidence of significant effects on any commercial type of building (in results not reported here). Thus my findings suggest that wartime destruction shifted economic activity (somewhat) towards residential use, which is consistent with the focus on residential activity in the city's reconstruction efforts.

3.4.2 Potential Agglomeration Effects

Adding to the impact on housing, the effect on amenities manifests as a large increase on the whole, but no disproportionate increase in amenities per-capita. These empirical findings point to some potential explanations. Firstly, there is no suggestive evidence for agglomeration effects, at least in terms of (public) amenities. While there is an increase in relative amenities compared to unbombed locations, this is only proportional to the residential population. From the previous literature, the availability of amenities could also depend on the surrounding socioeconomic composition of the population, or the type of surrounding buildings (Redding and Sturm 2024; Hornbeck and Keniston 2017). However, in my setting, the post-war (re-)construction of public housing estates in a bombed area only had a marginal impact (and did not contribute to the increase in the number of flats, see Table 3.5).

Additional suggestive evidence for the absence of strong agglomeration effects comes from the effect on firms. In Table 3.10, there is an increase in firms outright, but there is no effect in per-capita terms, considering the local population. In other terms, the increase in population creates a proportional increase in (especially small) businesses. However, it seemingly does not create agglomeration forces that cause the increase in firms to outgrow the increase in residents.

To further assess whether these patterns could be driven by spatial spillovers, i.e. whether destruction in nearby areas affects outcomes in adjacent neighborhoods, I extend the analysis to include destruction in neighboring grid cells. The results in Tables C.16–C.19 indicate that controlling for adjacent destruction typically reduces the absolute magnitude of the own effect, suggesting some spatial dependence. However, I find significant spillover effects only for firms, while spillovers for population and amenities remain weak or unstable across specifications.

Taken together, the results on spillover add an important nuance to the overall story. While economic activity, especially small-scale entrepreneurship, spread somewhat beyond the boundaries of bombed cells, most social and demographic effects remained tightly concentrated. The legacy of wartime destruction was therefore spatially contained: Reconstructed areas rebuilt themselves intensively, while their neighbors were only lightly touched by these dynamics. This reinforces the interpretation of post-war Vienna as a city where recovery happened block-by-block, with spillovers present but limited, and with the strongest transformations taking place exactly where destruction had struck.

One feature present in these results are *neighborhood effects*, but they focus primarily on buildings (Redding and Sturm 2024). However, there is still the possibility for a type of neighborhood effect where people directly affect key components of local amenities, such as crime, the quality of schools, and the demand for non-traded services, something which buildings cannot do. Furthermore, while people can walk around and influence the surrounding neighborhood, buildings have a fixed geographical location and can easily be avoided. The spillover results further support this interpretation: While firms respond to nearby reconstruction and local density, the distribution of public amenities and population composition remains bounded within neighborhood borders. Thus, more research on both businesses and inequality at the very local level is needed.

3.5 Conclusion

This study contributes to the broader literature on spatial economics by showing how a large, localized shock can reshape the physical structure of a city without fundamentally altering its long-term socioeconomic trajectory. Using the Allied bombing of Vienna during World War II as a natural experiment, I document how heavily damaged neighborhoods were not only rebuilt but rebuilt more densely, with more buildings, more flats, and more small businesses than before. Reconstruction produced layered urban landscapes, where pre-1919 buildings stand alongside rapid post-war construction, and fostered renewed, service-oriented local economies. Yet despite this substantial physical and economic transformation, the long-run demographic and socioeconomic shifts were surprisingly modest. Population increased, but income levels today are nearly indistinguishable from those in undamaged areas, declining by less than 1%. Changes in education, inequality, and access to amenities such as schools, childcare, and healthcare facilities are small or nonexistent once population size is taken into account.

The muted long-run differences across neighborhoods suggest that, in Vienna, spatial sorting and agglomeration forces played a weaker role than in other historical settings where destruction triggered persistent divergence. Instead, the evidence points to a reconstruction process that reintegrated rather than restructured the city: Public housing filled immediate gaps, private development drove long-term densification, and the return or arrival of new residents restored local demand in ways that enabled firms and services to reestablish themselves at scale. Local institutions, path dependencies in the built environment, and post-war planning choices may therefore have softened the disruptive effects typically associated with wartime destruction, helping neighborhoods converge toward similar socioeconomic outcomes over time.

These findings open several avenues for future research. A closer examination of the governance structures and policy choices that guided Vienna's post-war rebuilding could illuminate why physical reconstruction was so intense yet long-run social differences remained small. In particular, future work could more directly test theories of spatial path dependence, such as Bleakley and Lin (2012), which show how temporary geographic advantages can produce persistent urban outcomes through coordination effects and agglomeration forces. The Viennese case suggests a potentially different dynamic: Despite a large and localized shock, neighborhoods ultimately re-converged, raising the question of when historical shocks reinforce path dependence and in which cases institutions and reconstruction policies instead restore pre-existing spatial equilibria.

Comparative studies of cities that experienced different forms or intensities of destruction would help clarify when large shocks generate enduring spatial inequality and when they do not. Finally, linking neighborhood-level destruction to micro-level data on individual mobility, preferences, and inter-generational trajectories would deepen our understanding of how communities absorb and adapt to sudden shocks, and why some places emerge transformed while others ultimately look much the same.

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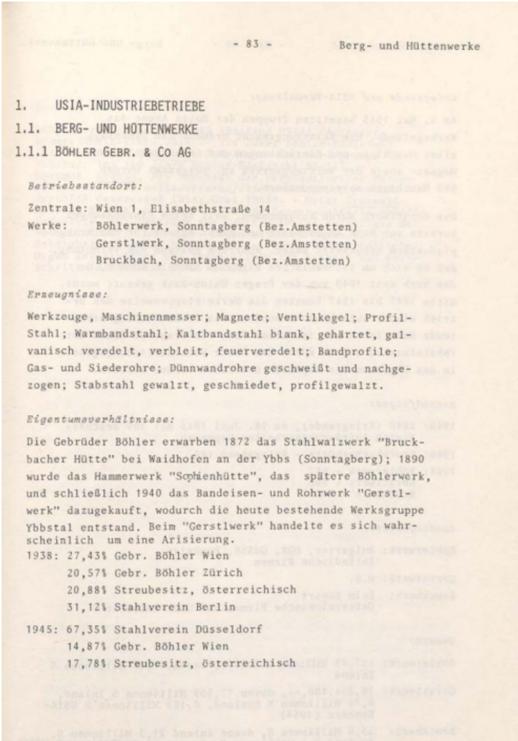
A Appendix for Chapter 1

A.1 Additional Figures

Figure A.1: Example: USIA business in the Soviet Zone



- 83 - Berg- und Hüttenwerke

1. USIA-INDUSTRIEBETRIEBE

1.1. BERG- UND HÜTTENWERKE

1.1.1 BÖHLER GEBR. & CO AG

Betriebsstandort:

Zentrale: Wien 1, Elisabethstraße 14
Werke: Böhlerwerk, Sonntagberg (Bez.Amstetten)
Gerstlwerk, Sonntagberg (Bez.Amstetten)
Bruckbach, Sonntagberg (Bez.Amstetten)

Erzeugnisse:

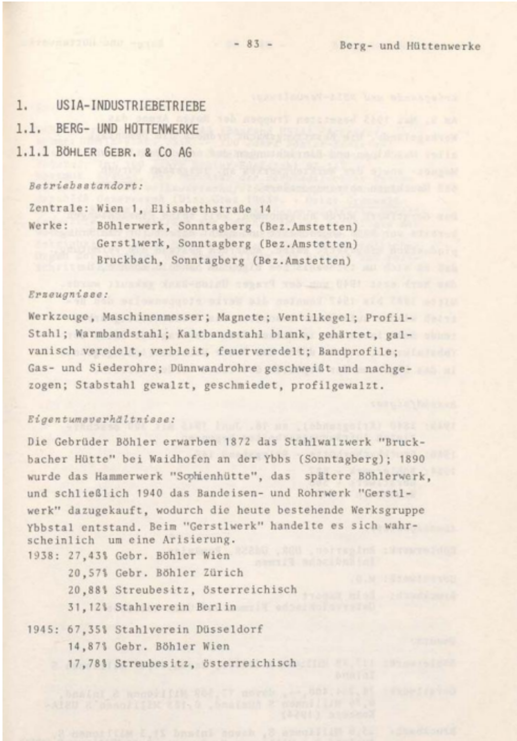
Werkzeuge, Maschinenmesser; Magnete; Ventilkegel; Profil-Stahl; Warmbandstahl; Kaltbandstahl blank, gehärtet, galvanisch veredelt, verbleit, feuerveredelt; Bandprofile; Gas- und Siederohre; Dünnwandrohre geschweißt und nachgezogen; Stabstahl gewalzt, geschmiedet, profilgewalzt.

Eigentumsverhältnisse:

Die Gebrüder Böhler erwarben 1872 das Stahlwalzwerk "Bruckbacher Hütte" bei Waidhofen an der Ybbs (Sonntagberg); 1890 wurde das Hammerwerk "Sophienhütte", das spätere Böhlerwerk, und schließlich 1940 das Bandeisen- und Rohrwerk "Gerstlwerk" dazugekauft, wodurch die heute bestehende Werksgruppe Ybbstal entstand. Beim "Gerstlwerk" handelte es sich wahrscheinlich um eine Arisierung.

1938: 27,43% Gebr. Böhler Wien
20,57% Gebr. Böhler Zürich
20,88% Streubesitz, österreichisch
31,12% Stahlverein Berlin

1945: 67,35% Stahlverein Düsseldorf
14,87% Gebr. Böhler Wien
17,78% Streubesitz, österreichisch



- 83 - Berg- und Hüttenwerke

1. USIA-INDUSTRIEBETRIEBE

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Bruckbach, Sonntagberg (Bez.Amstetten)

Erzeugnisse:

Werkzeuge, Maschinenmesser; Magnete; Ventilkegel; Profil-Stahl; Warmbandstahl; Kaltbandstahl blank, gehärtet, galvanisch veredelt, verbleit, feuerveredelt; Bandprofile; Gas- und Siederohre; Dünnwandrohre geschweißt und nachgezogen; Stabstahl gewalzt, geschmiedet, profilgewalzt.

Eigentumsverhältnisse:

Die Gebrüder Böhler erwarben 1872 das Stahlwalzwerk "Bruckbacher Hütte" bei Waidhofen an der Ybbs (Sonntagberg); 1890 wurde das Hammerwerk "Sophienhütte", das spätere Böhlerwerk, und schließlich 1940 das Bandeisen- und Rohrwerk "Gerstlwerk" dazugekauft, wodurch die heute bestehende Werksgruppe Ybbstal entstand. Beim "Gerstlwerk" handelte es sich wahrscheinlich um eine Arisierung.

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31,12% Stahlverein Berlin

1945: 67,35% Stahlverein Düsseldorf
14,87% Gebr. Böhler Wien
17,78% Streubesitz, österreichisch

Notes: Both pictures are taken from original documents in Klambauer (1978).

A.2 Additional Tables

Table A.1: Robustness: Distance Groups

RD Distance	(1) <100km	(2) <100km	(3) <50km	(4) <50km	(5) <25km	(6) <25km
Pre-WWII differences						
1927 × Soviet zone	0.001	0.001	0.002	0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Base-year (1930) differences						
Soviet zone	0.001	0.001	0.001	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Post-WWII differences						
1945 × Soviet zone	0.013***	0.014***	0.011***	0.010***	0.009***	0.006
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
1949 × Soviet zone	0.017***	0.016***	0.014***	0.011***	0.010***	0.005*
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
1956 × Soviet zone	0.011***	0.011***	0.007***	0.006**	0.004**	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
1962 × Soviet zone	0.001	0.001	-0.000	-0.001	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
1971 × Soviet zone	-0.001	-0.001	-0.001	-0.001	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1983 × Soviet zone	-0.001**	-0.002***	-0.001	-0.002*	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
19901 × Soviet zone	-0.001	-0.001*	-0.001	-0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2008 × Soviet zone	-0.003***	-0.003***	-0.003***	-0.002***	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
No. municipal.	1,680	1,680	1,031	1,031	514	514
No. periods	23	23	23	23	23	23
No. observations	35,241	23,854	21,618	13,198	10,779	6,219
Dem. Controls	No	Yes	No	Yes	No	Yes
Mean of dep. var.	0.01	0.01	0.01	0.01	0.01	0.01
S.d. of dep. var.	0.02	0.02	0.02	0.02	0.01	0.01

Notes: In this table the dependent variable is the Communist Party vote share, the specification above shows the distance within municipalities are considered for this sample. The method of estimation is least squares. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table A.2: Robustness: Different Distance Pairs

Sample:	(1) Border	(2) 0-10km	(3) 10-20km	(4) 20-30km	(5) 30-40km
Pre-WWII differences					
1927 × Soviet zone	-0.000				
	(0.000)				
Base-year (1930) differences					
Soviet zone	-0.000*	0.002	-0.003	-0.007	0.004
	(0.000)	(0.002)	(0.002)	(0.005)	(0.003)
Post-WWII differences					
1945 × Soviet zone	0.005	-0.002	-0.004	0.001	-0.024
	(0.004)	(0.006)	(0.008)	(0.012)	(0.016)
1949 × Soviet zone	0.009***	0.005	0.006	0.005	0.004
	(0.003)	(0.005)	(0.009)	(0.009)	(0.015)
1956 × Soviet zone	0.005**	-0.000	0.001	0.005	0.001
	(0.002)	(0.003)	(0.006)	(0.007)	(0.010)
1962 × Soviet zone	-0.003**	-0.005**	-0.005	0.002	-0.003
	(0.002)	(0.002)	(0.004)	(0.007)	(0.008)
1971 × Soviet zone	-0.000	-0.004**	0.003	0.004	-0.006
	(0.001)	(0.002)	(0.002)	(0.005)	(0.004)
1983 × Soviet zone	0.000	-0.002	0.002	0.007	-0.004
	(0.001)	(0.002)	(0.002)	(0.005)	(0.004)
1991 × Soviet zone	0.001	-0.002	0.003	0.007	-0.005
	(0.000)	(0.002)	(0.002)	(0.005)	(0.003)
2008 × Soviet zone	0.000	-0.002	0.003	0.004	-0.007**
	(0.000)	(0.002)	(0.002)	(0.005)	(0.003)
Pair-Year FE	Yes	Yes	Yes	Yes	Yes
No. pairs	93	128	228	210	194
No. unique municipal.	95	157	244	218	199
No. periods	23	24	24	23	23
No. observations	1,860	1,644	1,236	928	508
R-squared	0.65	0.61	0.67	0.66	0.72
Mean of dep. var.	0.01	0.01	0.01	0.01	0.01
S.d. of dep. var.	0.02	0.01	0.02	0.02	0.02

Notes: In this table the dependent variable is the Communist Party vote share, for different distance groups. The city of Linz is excluded, since the demarcation line disunited the city. The *1934 controls* include the population share in agriculture, in manufacturing, and of males, municipality size (in sq. km), and the number of inhabitants. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table A.3: Vote Share across Parties

Dep. Var.:	(1) KPOE	(2) SPOE	(3) FPOE
Pre-WWII differences			
1927 × Soviet zone	-0.000	0.001	-0.001
	(0.000)	(0.006)	(0.003)
Base-year (1930) differences			
Soviet zone	-0.002**	0.053***	0.002
	(0.001)	(0.013)	(0.002)
Post-WWII differences			
1945 × Soviet zone	0.005	0.000	
	(0.004)	(0.012)	
1949 × Soviet zone	0.009***	-0.010	-0.045***
	(0.003)	(0.010)	(0.009)
1956 × Soviet zone	0.005**	-0.012	-0.024***
	(0.002)	(0.009)	(0.005)
1962 × Soviet zone	-0.003*	-0.014	-0.015***
	(0.002)	(0.009)	(0.004)
1971 × Soviet zone	-0.000	-0.021*	-0.008**
	(0.001)	(0.012)	(0.004)
1983 × Soviet zone	0.000	-0.032***	-0.008**
	(0.001)	(0.010)	(0.003)
1990 × Soviet zone	0.001	-0.032***	-0.027***
	(0.000)	(0.011)	(0.005)
2008 × Soviet zone	0.000	-0.038***	-0.023***
	(0.000)	(0.011)	(0.004)
Pair-year FE	Yes	Yes	Yes
No. pairs	93	93	93
No. unique municipal.	95	95	95
No. periods	23	23	9
No. observations	1,860	1,860	1,674
R-squared	0.71	0.83	0.91
Mean of dep. var.	0.01	0.38	0.06
S.d. of dep. var.	0.02	0.16	0.06

Notes: In this table the dependent variable is the vote share of different political parties. The city of Linz is excluded, since the demarcation line disunited the city. The *1934 controls* include the population share in agriculture, in manufacturing, and of males, municipality size (in sq. km), and the number of inhabitants. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table A.4: Turnout / Democratic Participation

Specification:	(1) No Controls	(2) Dem. Controls
Pre-WWII differences		
1927 × Soviet zone	0.007	0.007
	(0.005)	(0.005)
Base-year (1930) differences		
Soviet zone	-0.047***	-0.046***
	(0.009)	(0.008)
Post-WWII differences		
1945 × Soviet zone	0.054***	0.054***
	(0.010)	(0.010)
1949 × Soviet zone	0.042***	0.042***
	(0.009)	(0.009)
1956 × Soviet zone	0.038***	0.038***
	(0.008)	(0.008)
1962 × Soviet zone	0.020**	0.020**
	(0.008)	(0.008)
1971 × Soviet zone	0.032***	0.032***
	(0.008)	(0.008)
1983 × Soviet zone	0.038***	0.038***
	(0.008)	(0.008)
1990 × Soviet zone	0.031***	0.031***
	(0.009)	(0.009)
2008 × Soviet zone	0.060***	0.060***
	(0.011)	(0.011)
Pair-year FE	Yes	Yes
No. pairs	93	93
No. unique municipal.	95	95
No. periods	23	23
No. observations	1,860	1,860
R-squared	0.87	0.87
Mean of dep. var.	0.91	0.91
S.d. of dep. var.	0.07	0.07

Notes: In this table the dependent variable is the turnout for national elections. The city of Linz is excluded, since the demarcation line disunited the city. The *1934 controls* include the population share in agriculture, in manufacturing, and of males, municipality size (in sq. km), and the number of inhabitants. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table A.5: Robustness: Border-Placebo

Specification:	(1) U.S. – U.K.	(2) U.S. – France
Pre-WWII differences		
1927 × Placebo/American zone	0.000	—
	(0.000)	(—)
Base-year (1930) differences		
Placebo/American zone	0.004*	-0.001
	(0.002)	(0.001)
Post-WWII differences		
1945 × Placebo/American zone	0.009	0.006
	(0.008)	(0.008)
1949 × Placebo/American zone	0.007	0.004
	(0.004)	(0.004)
1956 × Placebo/American zone	0.005*	-0.003
	(0.002)	(0.003)
1962 × Placebo/American zone	-0.003	-0.002
	(0.002)	(0.002)
1971 × Placebo/American zone	-0.002	0.001
	(0.002)	(0.001)
1983 × Placebo/American zone	-0.004*	0.000
	(0.002)	(0.001)
1990 × Placebo/American zone	-0.004*	-0.001
	(0.002)	(0.001)
2008 × Placebo/American zone	-0.004	-0.003
	(0.003)	(0.002)
Pair-year FE	Yes	Yes
No. pairs	68	21
No. unique municipal.	70	22
No. periods	23	23
No. observations	2,338	858
R-squared	0.84	0.66
Mean of dep. var.	0.01	0.01

Notes: In this table the dependent variable is the Communist Party vote share. The city of Linz is excluded, since the demarcation line disunited the city. The *1934 controls* include the population share in agriculture, in manufacturing, and of males, municipality size (in sq. km), and the number of inhabitants. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table A.6: Differential Effect of Encampments in the Soviet Zone

	All Camps		Occ. only	
Specification:	**(1)**	**(2)**	**(3)**	**(4)**
Pre-WWII differences				
1927 × Camp	-0.003*	-0.004*	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)
Base-year (1930) differences				
Camp	0.001	0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Post-WWII differences				
1945 × Camp	0.013**	0.010*	-0.002	-0.008
	(0.005)	(0.006)	(0.006)	(0.006)
1949 × Camp	0.013***	0.009*	-0.000	-0.006
	(0.005)	(0.005)	(0.006)	(0.005)
1956 × Camp	0.010**	0.007	0.000	-0.004
	(0.004)	(0.004)	(0.004)	(0.005)
1962 × Camp	0.006**	0.004	0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)
1971 × Camp	0.001	0.001	0.000	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
1983 × Camp	-0.000	-0.001	0.000	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)
1990 × Camp	-0.000	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
2008 × Camp	-0.001	-0.001	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Dem. Controls	No	Yes	No	Yes
No. unique municipal.	866	866	866	866
No. periods	23	23	23	23
No. observations	17,900	13,949	17,900	13,949
R-squared	0	0	0	0
Mean of dep. var.	0.01	0.01	0.01	0.01
S.d. of dep. var.	0.02	0.03	0.02	0.03

Notes: In this table the dependent variable is the Communist Party vote share, the specification above follows equation (3). The method of estimation is least squares. Robust standard errors (which allow for clustering by municipality and heteroskedasticity) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table A.7: Differential Effect of USIA & Encampments in the Soviet Zone

Specification:	USIA (1)	Camps (2)	Both (3)	Interact. (4)
USIA Estimates				
1927 × USIA mun	-0.004**		-0.002	-0.003
	(0.002)		(0.002)	(0.004)
USIA mun (Base-year 1930)	-0.001		-0.001	-0.001
	(0.001)		(0.001)	(0.001)
1945 × USIA mun	0.046***		0.046***	0.047***
	(0.007)		(0.008)	(0.010)
1949 × USIA mun	0.043***		0.043***	0.044***
	(0.006)		(0.007)	(0.009)
1956 × USIA mun	0.033***		0.033***	0.033***
	(0.006)		(0.006)	(0.009)
1962 × USIA mun	0.021***		0.021***	0.022***
	(0.005)		(0.005)	(0.006)
1971 × USIA mun	0.009***		0.009***	0.010***
	(0.002)		(0.002)	(0.003)
1983 × USIA mun	0.003**		0.004**	0.004**
	(0.002)		(0.002)	(0.002)
1990 × USIA mun	0.002		0.003*	0.004**
	(0.002)		(0.002)	(0.002)
2008 × USIA mun	0.001		0.002	0.002*
	(0.001)		(0.001)	(0.001)
Camp Estimates				
1927 × Camp		-0.004*	-0.003	-0.003
		(0.002)	(0.002)	(0.002)
Camp (Base-year 1930)		0.000	0.001	0.000
		(0.002)	(0.002)	(0.002)
1945 × Camp		0.010*	0.001	0.003
		(0.006)	(0.006)	(0.007)
1949 × Camp		0.009*	0.001	0.002
		(0.005)	(0.005)	(0.006)
1956 × Camp		0.007	-0.000	-0.000
		(0.004)	(0.004)	(0.004)
1962 × Camp		0.004	-0.000	0.001
		(0.003)	(0.003)	(0.003)
1971 × Camp		0.001	-0.001	-0.000
		(0.002)	(0.002)	(0.002)
1983 × Camp		-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)
1990 × Camp		-0.001	-0.001	-0.000
		(0.002)	(0.002)	(0.002)
2008 × Camp		-0.001	-0.001	-0.001
		(0.002)	(0.002)	(0.002)
Interaction Estimates				
1927 × interact.				0.000
				(.)
Base-year (1930) interaction				0.001
				(0.004)
1945 × interact.				-0.006
				(0.014)
1949 × interact.				-0.004
				(0.012)
1956 × interact.				-0.000
				(0.012)
1962 × interact.				-0.003
				(0.008)
1971 × interact.				-0.004
				(0.005)
1983 × interact.				-0.002
				(0.004)
1990 × interact.				-0.003
				(0.004)
2008 × interact.				-0.002
				(0.004)
Controls	Yes	Yes	Yes	Yes
No. unique municipal.	866	866	866	866
No. periods	23	23	23	23
No. observations	13,949	13,949	13,949	13,949

Notes: *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

B Appendix for Chapter 2

B.1 Additional Figures

Figure B.1: Stylized Example of Bordering Area Pairs

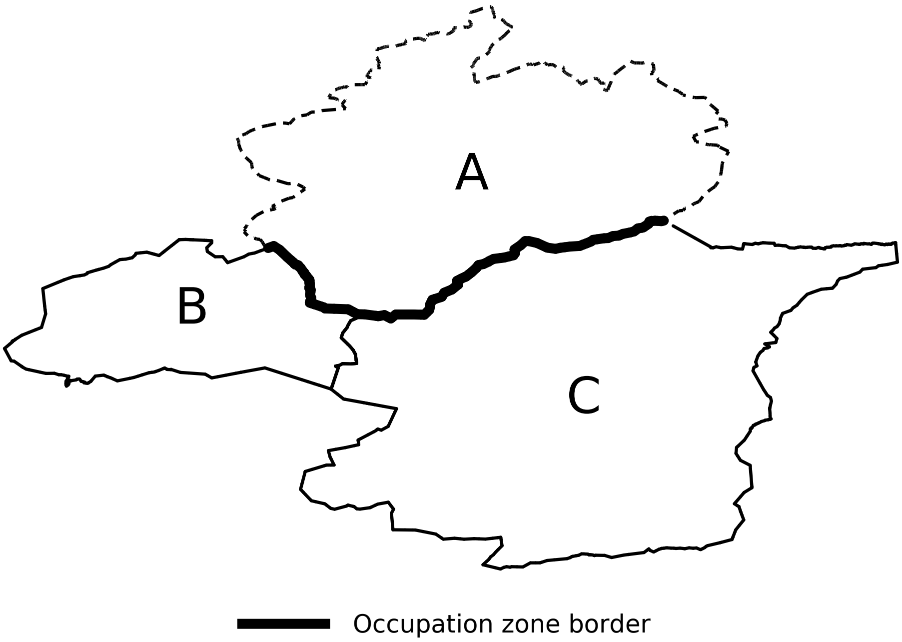
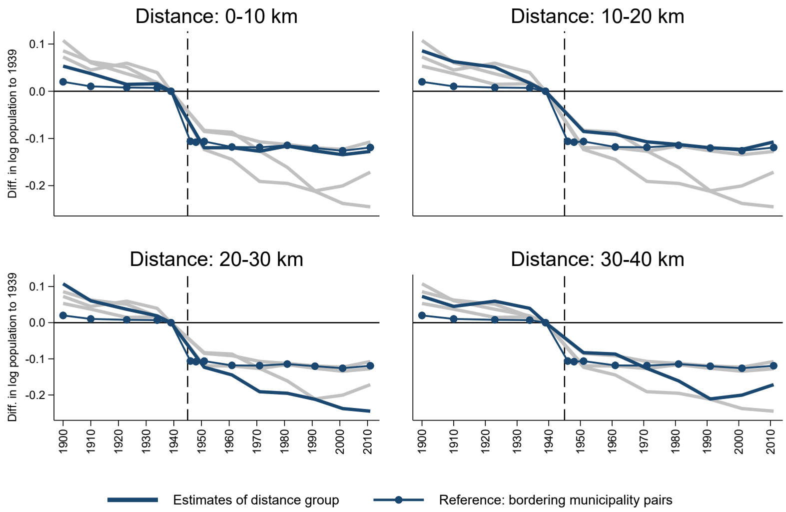
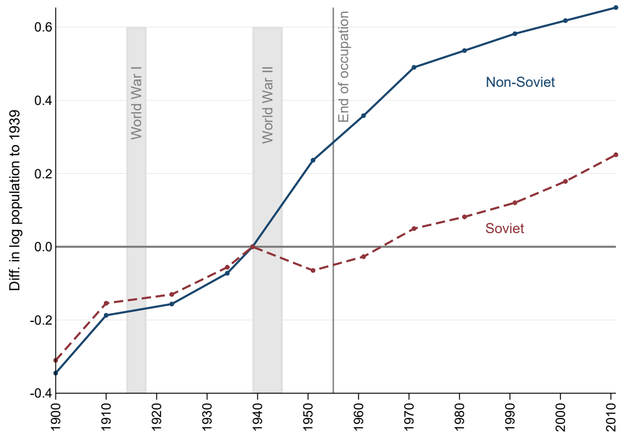


Figure B.2: Estimation of the Effect of the Soviet Occupation on Population: Robustness



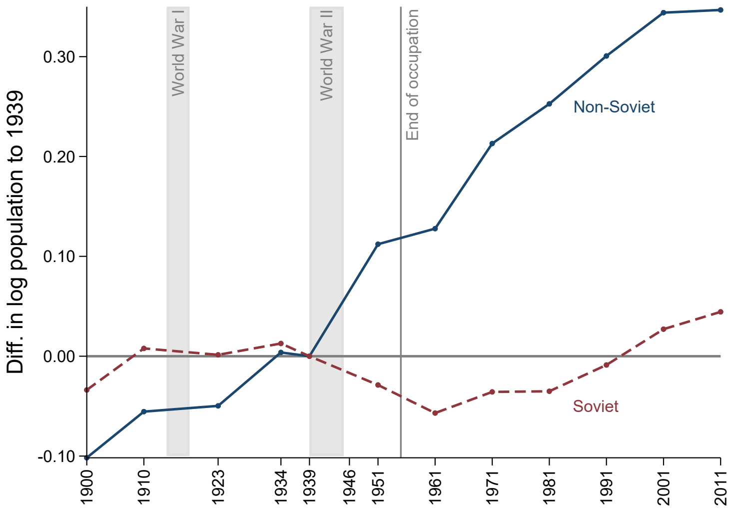
Notes: Geographic areas are municipalities in Austria. Table B.3 in the Appendix shows corresponding estimation results for Austria.

Figure B.3: Development of Population in the Soviet and in the non-Soviet Zone in Cities

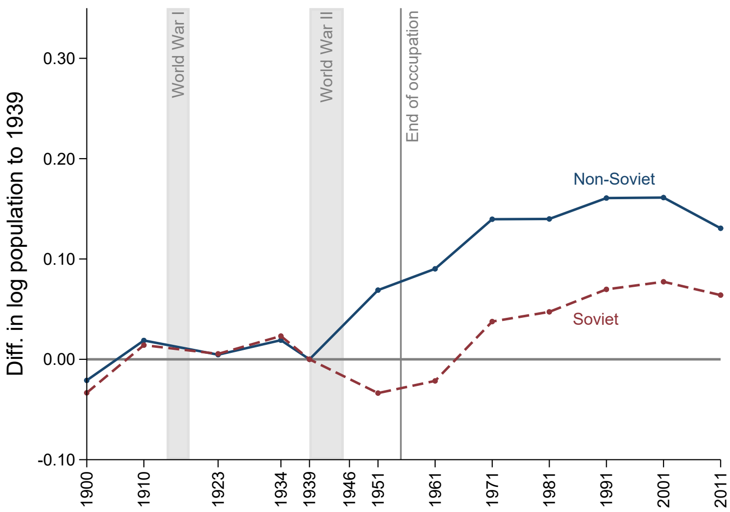


Notes: This figure uses data from all Austrian municipalities with a population of 10,000 or more in 2011 (except Linz and Vienna, which where administered by both the Western and Soviet Occupation forces). The vertical line marks the end of occupation.

Figure B.4: Development of Population in the Soviet and in the non-Soviet Zone



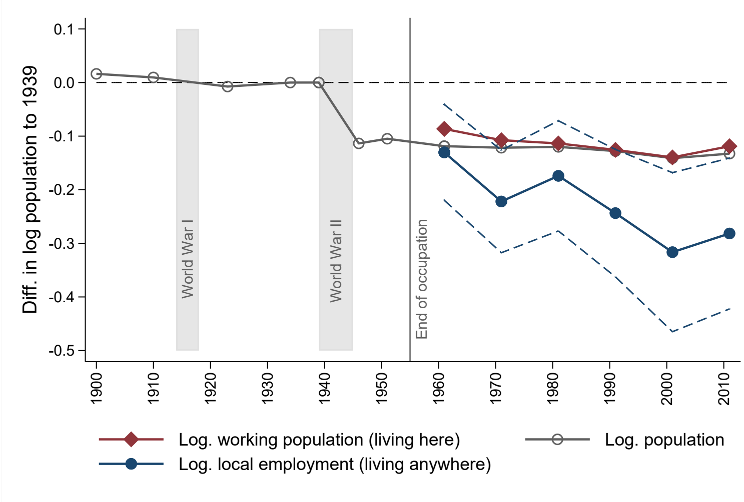
(a) All municipalities (except Vienna and Linz)



(b) Bordering municipalities

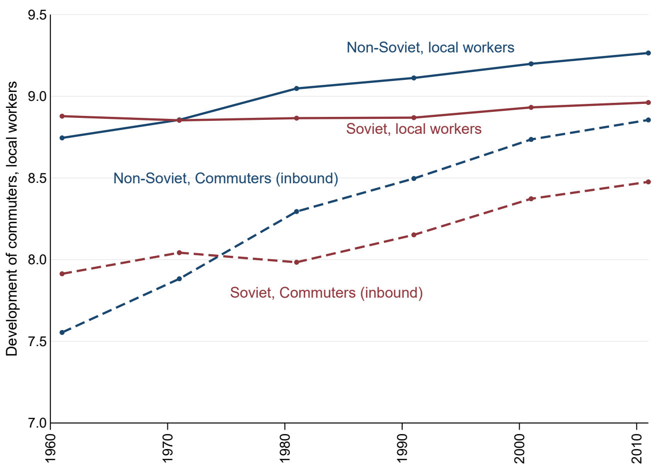
Notes: The upper panel uses data from all municipalities (except Vienna and Linz). The lower panel uses only data from municipalities situated along the demarcation line between the Soviet and the non-Soviet zones. The vertical line marks the end of occupation.

Figure B.5: Estimation of the Effect of the Soviet Occupation on Workers



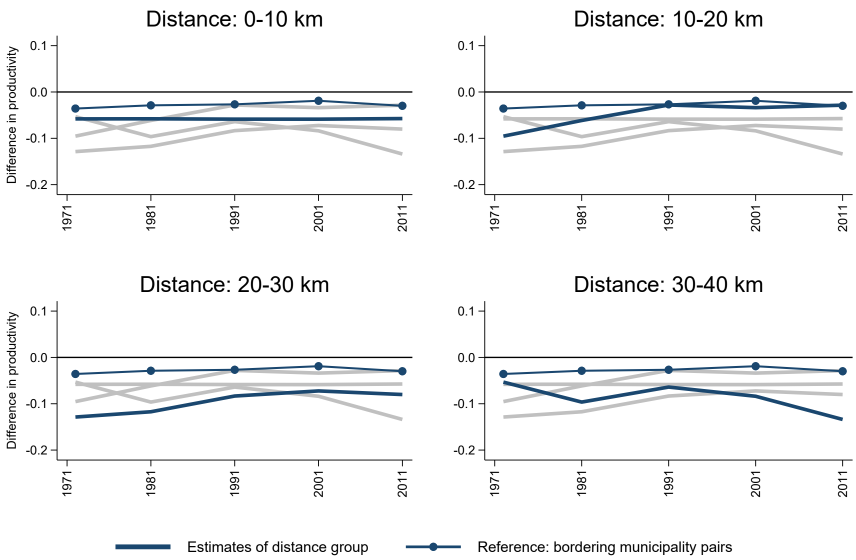
Notes: Estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of the respective variable. Table 2.5 shows corresponding estimation results. Dashed lines indicate 95% confidence intervals.

Figure B.6: Development of Commuters/Local Workers in the Soviet and in the non-Soviet Zone in Cities



Notes: This figure uses data from Austrian municipalities with a population of 10,000 or more in 2011 (except Linz and Vienna, which where administered by both the Western and Soviet Occupation forces). The dependent variable is equal to the log of the respective variable.

Figure B.7: Estimation of the Effect of the Soviet Occupation on Productivity: Robustness



Notes: Geographic areas are municipalities in Austria. Table B.9 in the Appendix shows corresponding estimation results for Austria.

B.2 Additional Tables

Table B.1: Estimation of the Effect of the Soviet Occupation on Population: Robustness Checks

	Log Population						
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Pre-WWII differences							
1900 × Soviet zone	0.020	0.020	0.020	0.020	0.020	0.017	0.021
	(0.021)	(0.019)	(0.027)	(0.032)	(0.029)	(0.025)	(0.024)
1910 × Soviet zone	0.010	0.010	0.010	0.010	0.010	0.014	0.006
	(0.016)	(0.013)	(0.018)	(0.024)	(0.022)	(0.016)	(0.017)
1923 × Soviet zone	0.008	0.008	0.008	0.008	0.008	0.014	-0.000
	(0.014)	(0.012)	(0.017)	(0.022)	(0.017)	(0.016)	(0.015)
1934 × Soviet zone	0.007	0.007	0.007	0.007	0.007	0.007	0.002
	(0.008)	(0.006)	(0.009)	(0.012)	(0.012)	(0.009)	(0.009)
Base-year (1939) differences							
Soviet zone	0.109	0.109	0.109	0.109	0.109	-0.160	0.326**
	(0.165)	(0.115)	(0.163)	(0.182)	(0.278)	(0.158)	(0.133)
Post-WWII differences							
1946 × Soviet zone	-0.106***	-0.106***	-0.106***	-0.106***	-0.106**	-0.109***	-0.103***
	(0.030)	(0.022)	(0.031)	(0.038)	(0.047)	(0.032)	(0.026)
1948 × Soviet zone	-0.108***	-0.108***	-0.108***	-0.108***	-0.108***	-0.121***	-0.100***
	(0.015)	(0.012)	(0.017)	(0.021)	(0.022)	(0.016)	(0.015)
1951 × Soviet zone	-0.106***	-0.106***	-0.106***	-0.106***	-0.106***	-0.122***	-0.101***
	(0.017)	(0.014)	(0.020)	(0.024)	(0.026)	(0.018)	(0.021)
1961 × Soviet zone	-0.118***	-0.118***	-0.118***	-0.118***	-0.118***	-0.136***	-0.117***
	(0.020)	(0.017)	(0.024)	(0.027)	(0.029)	(0.019)	(0.024)
1971 × Soviet zone	-0.119***	-0.119***	-0.119***	-0.119***	-0.119***	-0.136***	-0.113***
	(0.026)	(0.021)	(0.030)	(0.035)	(0.039)	(0.026)	(0.026)
1981 × Soviet zone	-0.114***	-0.114***	-0.114***	-0.114***	-0.114**	-0.133***	-0.109***
	(0.031)	(0.026)	(0.037)	(0.040)	(0.048)	(0.031)	(0.035)
1991 × Soviet zone	-0.120***	-0.120***	-0.120***	-0.120**	-0.120**	-0.134***	-0.118***
	(0.036)	(0.031)	(0.044)	(0.048)	(0.055)	(0.037)	(0.042)
2001 × Soviet zone	-0.126***	-0.126***	-0.126**	-0.126**	-0.126*	-0.144***	-0.120**
	(0.041)	(0.034)	(0.048)	(0.053)	(0.064)	(0.041)	(0.047)
2011 × Soviet zone	-0.119***	-0.119***	-0.119**	-0.119**	-0.119*	-0.138***	-0.108**
	(0.041)	(0.034)	(0.049)	(0.054)	(0.063)	(0.042)	(0.046)
Pair-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. pairs	93	93	93	93	93	50	45
No. unique municipal.	95	95	95	95	95	100	90
No. periods	14	14	14	14	14	14	14
No. observations	2,604	2,604	2,604	2,604	2,604	1,400	1,260
R-squared	0.48	0.48	0.48	0.48	0.48	0.47	0.53
Mean of dep. var.	7.66	7.66	7.66	7.66	7.66	7.69	7.59
S.d. of dep. var.	0.78	0.78	0.78	0.78	0.78	0.77	0.64

Notes: This table summarizes estimation results based on municipality-level data. The dependent variable is equal to the log of population. Each specification includes the variables listed and pair-wise year fixed effects, where pairs are given by neighboring municipalities along the demarcation line. The method of estimation is least squares. Standard errors are in parentheses below (see below for a description of Table B.1). *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Description of Table B.1:

1. Our standard approach for reference (see column (1) in Table B.3). Standard errors are clustered within each municipality, no matter in which pair the municipality is.
2. Standard errors are clustered at the municipality-level within each pair.
3. Standard errors are clustered within each pair.
4. Standard errors are clustered within each municipality in the Soviet zone plus all bordering municipalities in the non-Soviet zones.
5. Standard errors are clustered within each municipality in the Non-Soviet zones plus all bordering municipalities in the Soviet zones.
6. Dataset is transformed so that for each municipality in the Soviet zone, there is a synthetic control municipality in the non-Soviet zones. The synthetic control municipality is the mean of all bordering municipalities in the non-Soviet zones.
7. Dataset is transformed so that for each municipality in the non-Soviet zones, there is a synthetic control municipality in the Soviet zone. The synthetic control municipality is the mean of all bordering municipalities in the Soviet zone.

Table B.2: Estimation of the Effect of the Soviet Occupation on Population in Austria using standard DiD

	Bordering Municipalities	Court Districts	Administrative Districts	Municipalities Border-States	All Municipalities
	(I)	(II)	(III)	(IV)	(V)
Pre-WWII differences					
1900 × Soviet zone	-0.019	0.058***	0.057***	0.029***	0.068***
	(0.035)	(0.015)	(0.014)	(0.010)	(0.009)
1910 × Soviet zone	-0.012	0.040***	0.039***	0.031***	0.063***
	(0.027)	(0.011)	(0.011)	(0.008)	(0.007)
1923 × Soviet zone	-0.002	0.035***	0.035***	0.019***	0.051***
	(0.025)	(0.009)	(0.008)	(0.006)	(0.005)
1934 × Soviet zone	-0.000	0.015***	0.014**	-0.001	0.009**
	(0.015)	(0.006)	(0.005)	(0.004)	(0.003)
Base-year (1939) differences					
Soviet zone	0.098	0.144***	0.163***	0.313***	0.387***
	(0.129)	(0.054)	(0.051)	(0.034)	(0.032)
Post-WWII differences					
1951 × Soviet zone	-0.101***	-0.083***	-0.081***	-0.114***	-0.141***
	(0.028)	(0.009)	(0.008)	(0.006)	(0.005)
1961 × Soviet zone	-0.109***	-0.091***	-0.086***	-0.126***	-0.185***
	(0.038)	(0.013)	(0.012)	(0.008)	(0.007)
1971 × Soviet zone	-0.099*	-0.106***	-0.102***	-0.155***	-0.249***
	(0.056)	(0.018)	(0.017)	(0.011)	(0.010)
1981 × Soviet zone	-0.091	-0.097***	-0.091***	-0.169***	-0.288***
	(0.072)	(0.022)	(0.020)	(0.013)	(0.012)
1991 × Soviet zone	-0.089	-0.095***	-0.085***	-0.173***	-0.309***
	(0.085)	(0.025)	(0.024)	(0.015)	(0.014)
2001 × Soviet zone	-0.082	-0.095***	-0.082***	-0.163***	-0.317***
	(0.095)	(0.029)	(0.027)	(0.017)	(0.016)
2011 × Soviet zone	-0.065	-0.078**	-0.068**	-0.140***	-0.302***
	(0.103)	(0.032)	(0.030)	(0.019)	(0.018)
Year FE	Yes	Yes	Yes	Yes	Yes
No. unique municipal.	105	615	676	1,725	2,351
No. periods	12	12	12	12	12
No. observations	1,260	7,380	8,112	20,700	28,212
R-squared	0.01	0.01	0.01	0.03	0.04
Mean of dep. var.	7.36	7.22	7.20	7.28	7.24
S.d. of dep. var.	0.71	0.72	0.72	0.76	0.84

Notes: This table summarizes estimation results based on municipality-level data from 1900, 1910, 1923, 1934, 1939, 1946, 1948, 1951, 1961, 1971, 1981, 1991, 2001, 2011. The cities Linz, Steyr, and Vienna are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of population. Each specification includes the variables listed. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.3: Estimation of the Effect of the Soviet Occupation on Population in Austria

	Bordering municipalities	Distance to the demarcation line of 0-10km	10-20km	20-30km	30-40km
	(I)	(II)	(III)	(IV)	(V)
Pre-WWII differences					
1900 × Soviet zone	0.020	0.053***	0.086***	0.108***	0.073***
	(0.019)	(0.017)	(0.014)	(0.012)	(0.019)
1910 × Soviet zone	0.010	0.037***	0.063***	0.061***	0.045***
	(0.013)	(0.011)	(0.012)	(0.012)	(0.016)
1923 × Soviet zone	0.008	0.015	0.051***	0.037***	0.059***
	(0.012)	(0.010)	(0.009)	(0.010)	(0.013)
1934 × Soviet zone	0.007	0.016**	0.017***	0.019***	0.040***
	(0.006)	(0.008)	(0.005)	(0.006)	(0.012)
Base-year (1939) differences					
Soviet zone	0.109	-0.074	0.045	0.024	-0.037
	(0.115)	(0.071)	(0.067)	(0.062)	(0.052)
Post-WWII differences					
1946 × Soviet zone	-0.106***				
	(0.022)				
1948 × Soviet zone	-0.108***				
	(0.012)				
1951 × Soviet zone	-0.106***	-0.120***	-0.085***	-0.123***	-0.083***
	(0.014)	(0.017)	(0.012)	(0.008)	(0.014)
1961 × Soviet zone	-0.118***	-0.120***	-0.091***	-0.144***	-0.087***
	(0.017)	(0.020)	(0.016)	(0.011)	(0.017)
1971 × Soviet zone	-0.119***	-0.127***	-0.107***	-0.191***	-0.126***
	(0.021)	(0.021)	(0.020)	(0.016)	(0.021)
1981 × Soviet zone	-0.114***	-0.116***	-0.113***	-0.195***	-0.161***
	(0.026)	(0.026)	(0.022)	(0.021)	(0.026)
1991 × Soviet zone	-0.120***	-0.126***	-0.120***	-0.212***	-0.211***
	(0.031)	(0.031)	(0.022)	(0.024)	(0.032)
2001 × Soviet zone	-0.126***	-0.134***	-0.123***	-0.238***	-0.200***
	(0.034)	(0.033)	(0.023)	(0.028)	(0.037)
2011 × Soviet zone	-0.119***	-0.127***	-0.107***	-0.245***	-0.171***
	(0.034)	(0.033)	(0.025)	(0.032)	(0.043)
Pair-Year FE	Yes	Yes	Yes	Yes	Yes
No. pairs	93	128	228	210	194
No. unique municipal.	95	157	244	218	199
No. periods	14	12	12	12	12
No. observations	2,604	3,072	5,472	5,040	4,656
R-squared	0.48	0.64	0.53	0.55	0.59
Mean of dep. var.	7.66	7.29	7.23	7.23	7.19
S.d. of dep. var.	0.78	0.67	0.78	0.67	0.67

Notes: This table summarizes estimation results based on municipality-level data from 1900, 1910, 1923, 1934, 1939, 1946, 1948, 1951, 1961, 1971, 1981, 1991, 2001, 2011. The cities Linz, Steyr, and Vienna are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of population. Each specification includes the variables listed. Specifications (I)-(V) controls in addition for pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line). The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.4: The Effect of the Placebo Demarcation Lines on Population

	Dep. var.: Log Population			
	Placebo demarcation line		Other zone borders	
	State borders	Danube river	US-UK	US-French
	(I)	(II)	(III)	(IV)
Pre-WW2 differences				
1900 × Placebo/American zone	-0.001	0.030	-0.066**	0.015
	(0.033)	(0.034)	(0.029)	(0.055)
1910 × Placebo/American zone	0.009	-0.011	-0.023	0.017
	(0.026)	(0.033)	(0.024)	(0.047)
1923 × Placebo/American zone	-0.052***	-0.012	0.006	-0.074*
	(0.018)	(0.026)	(0.023)	(0.041)
1934 × Placebo/American zone	0.001	0.008	-0.001	-0.037
	(0.011)	(0.010)	(0.017)	(0.029)
Base-year (1939) differences				
Placebo/American zone	-0.006	-0.962***	0.099	0.080
	(0.100)	(0.218)	(0.127)	(0.166)
Post-WW2 differences				
1951 × Placebo/American zone	0.024	-0.001	0.037*	0.016
	(0.018)	(0.019)	(0.020)	(0.030)
1961 × Placebo/American zone	0.014	-0.023	0.038	-0.009
	(0.016)	(0.025)	(0.024)	(0.056)
1971 × Placebo/American zone	-0.023	-0.003	0.005	-0.004
	(0.022)	(0.038)	(0.032)	(0.062)
1981 × Placebo/American zone	-0.020	0.010	0.012	-0.000
	(0.024)	(0.042)	(0.037)	(0.068)
1991 × Placebo/American zone	-0.030	0.053	0.018	0.024
	(0.029)	(0.047)	(0.041)	(0.081)
2001 × Placebo/American zone	0.004	0.060	0.006	0.020
	(0.035)	(0.057)	(0.047)	(0.083)
2011 × Placebo/American zone	0.011	0.044	0.006	-0.033
	(0.041)	(0.072)	(0.051)	(0.086)
Pair-year FE	Yes	Yes	Yes	Yes
No. pairs	56	34	68	21
No. unique municipal.	56	41	70	22
No. periods	12	12	12	12
No. observations	1,344	816	1,632	504
R-squared	0.63	0.72	0.53	0.75
Mean of dep. var.	7.43	7.80	7.25	7.17

Notes: This table summarizes estimation results based on municipality-level data. The dependent variable is equal to the log of population. Each specification includes the variables listed and pair-wise year fixed effects (where pairs are given by neighboring municipalities along the placebo demarcation line). The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.5: Effect of the Soviet Occupation on Population (Robustness - RDD-DiD)

Sample definition:	Bordering Municipalities				
	Baseline Estimates	Excl. Upper Austrian Pairs	Excl. Styrian Pairs	Excl. Big Pairs	Excl. Small Pairs
	(I)	(II)	(III)	(IV)	(V)
Pre-WW2 differences					
1900 × Soviet zone	0.020	0.009	-0.003	0.021	0.006
	(0.021)	(0.028)	(0.034)	(0.021)	(0.031)
1910 × Soviet zone	0.010	0.004	-0.003	0.011	-0.002
	(0.016)	(0.021)	(0.026)	(0.016)	(0.025)
1923 × Soviet zone	0.008	0.008	0.009	0.009	-0.009
	(0.014)	(0.016)	(0.032)	(0.014)	(0.016)
1934 × Soviet zone	0.007	0.012	-0.008	0.008	0.002
	(0.008)	(0.008)	(0.022)	(0.009)	(0.011)
Base-year (1939) differences					
Soviet zone	0.109	0.179	-0.013	0.148	-0.211
	(0.165)	(0.199)	(0.314)	(0.176)	(0.281)
Post-WW2 differences					
1946 × Soviet zone	-0.106***	-0.073**	-0.091	-0.106***	-0.085**
	(0.030)	(0.034)	(0.100)	(0.033)	(0.038)
1948 × Soviet zone	-0.108***	-0.092***	-0.133***	-0.112***	-0.089***
	(0.015)	(0.017)	(0.034)	(0.016)	(0.016)
1951 × Soviet zone	-0.106***	-0.087***	-0.142***	-0.110***	-0.089***
	(0.017)	(0.018)	(0.044)	(0.018)	(0.019)
1961 × Soviet zone	-0.118***	-0.120***	-0.108**	-0.123***	-0.093***
	(0.020)	(0.022)	(0.047)	(0.021)	(0.023)
1971 × Soviet zone	-0.119***	-0.130***	-0.076	-0.126***	-0.073**
	(0.026)	(0.030)	(0.051)	(0.027)	(0.035)
1981 × Soviet zone	-0.114***	-0.138***	-0.046	-0.122***	-0.072
	(0.031)	(0.033)	(0.065)	(0.032)	(0.045)
1991 × Soviet zone	-0.120***	-0.155***	-0.025	-0.126***	-0.076
	(0.036)	(0.035)	(0.084)	(0.039)	(0.050)
2001 × Soviet zone	-0.126***	-0.168***	-0.033	-0.133***	-0.072
	(0.041)	(0.042)	(0.095)	(0.043)	(0.059)
2011 × Soviet zone	-0.119***	-0.153***	-0.045	-0.126***	-0.074
	(0.041)	(0.044)	(0.093)	(0.043)	(0.058)
Pair-Year FE	Yes	Yes	Yes	Yes	Yes
No. observations	2,604	2,016	1,778	2,352	1,344
No. pairs	93	72	93	84	48
No. unique municipalities	95	74	65	92	58
No. periods	14.00	14.00	14.00	14.00	14.00
R-squared	0.48	0.45	0.68	0.41	0.32
Mean of dep. var.	7.66	7.62	7.73	7.58	8.01

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of population. The control variables in each specification are interacted with year dummies. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcations line) are included. Robust standard errors (allowing for clustering by municipality within a pair and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.6: Effect of the Soviet Occupation on Population (Robustness - Std. DiD)

Sample definition:	Bordering Districts				
	Baseline Estimates	Excl. Upper Austrian Mun.	Excl. Styrian Mun.	Excl. Big Mun.	Excl. Small Mun.
	(I)	(II)	(III)	(IV)	(V)
Pre-WWII differences					
1900 × Soviet zone	0.057***	0.044**	0.061***	0.044**	0.084***
	(0.014)	(0.018)	(0.017)	(0.022)	(0.017)
1910 × Soviet zone	0.039***	0.035***	0.060***	0.033**	0.053***
	(0.011)	(0.014)	(0.014)	(0.016)	(0.014)
1923 × Soviet zone	0.035***	0.047***	0.037***	0.026**	0.048***
	(0.008)	(0.010)	(0.011)	(0.012)	(0.012)
1934 × Soviet zone	0.014**	0.017***	0.025***	0.020**	0.013*
	(0.005)	(0.006)	(0.008)	(0.008)	(0.007)
Post-WWII differences					
1951 × Soviet zone	-0.081***	-0.069***	-0.166***	-0.110***	-0.064***
	(0.008)	(0.008)	(0.018)	(0.013)	(0.011)
1961 × Soviet zone	-0.086***	-0.095***	-0.153***	-0.107***	-0.076***
	(0.012)	(0.012)	(0.028)	(0.018)	(0.016)
1971 × Soviet zone	-0.102***	-0.117***	-0.198***	-0.131***	-0.088***
	(0.017)	(0.017)	(0.035)	(0.024)	(0.023)
1981 × Soviet zone	-0.091***	-0.114***	-0.221***	-0.119***	-0.075***
	(0.020)	(0.020)	(0.041)	(0.029)	(0.029)
1991 × Soviet zone	-0.085***	-0.113***	-0.251***	-0.113***	-0.067*
	(0.024)	(0.024)	(0.045)	(0.033)	(0.035)
2001 × Soviet zone	-0.082***	-0.110***	-0.291***	-0.100***	-0.070*
	(0.027)	(0.028)	(0.049)	(0.038)	(0.040)
2011 × Soviet zone	-0.068**	-0.088***	-0.316***	-0.088**	-0.053
	(0.030)	(0.032)	(0.052)	(0.042)	(0.044)
Base-year (1939) differences					
Soviet zone	0.163***	0.307***	-0.144*	-0.001	0.046
	(0.051)	(0.062)	(0.074)	(0.052)	(0.044)
Year FE	Yes	Yes	Yes	Yes	Yes
No. unique municipal.	676	676	676	676	676
No. periods	12	12	12	12	12
No. observations	8,112	5,964	4,764	3,588	4,524
R-squared	0.01	0.03	0.04	0.02	0.01
Mean of dep. var.	7.20	7.14	7.32	7.77	6.74
S.d. of dep. var.	0.72	0.73	0.70	0.51	0.50

Notes: This table summarizes estimation results based on municipality-level data from 1900, 1910, 1923, 1934, 1939, 1946, 1948, 1951, 1961, 1971, 1981, 1991, 2001, 2011. The cities Linz, Steyr, and Vienna are excluded, since in both cases the demarcations disunited the city. The dependent variable is equal to the log of population. Each specification includes the variables listed. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.7: Estimation of the Effect of the Soviet Occupation on Local Workers

	Robustness: Local workers			
	Distance to the demarcation line of			
	0-10km	10-20km	20-30km	30-40km
	(I)	(II)	(III)	(IV)
1961 × Soviet zone	-0.169**	-0.117***	-0.051	-0.072**
	(0.067)	(0.038)	(0.039)	(0.031)
1971 × Soviet zone	-0.238***	-0.167***	-0.218***	-0.064
	(0.060)	(0.046)	(0.047)	(0.040)
1981 × Soviet zone	-0.192***	-0.168***	-0.281***	-0.090**
	(0.072)	(0.057)	(0.052)	(0.045)
1991 × Soviet zone	-0.280***	-0.284***	-0.338***	-0.096*
	(0.081)	(0.065)	(0.057)	(0.057)
2001 × Soviet zone	-0.345***	-0.217***	-0.433***	-0.201***
	(0.099)	(0.065)	(0.061)	(0.066)
2011 × Soviet zone	-0.335***	-0.247***	-0.486***	-0.255***
	(0.091)	(0.071)	(0.063)	(0.072)
Pair-Year FE	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes
No. pairs	88	180	170	137
No. unique municipal.	115	199	179	141
No. periods	6	6	6	6
No. observations	1,056	2,160	2,040	1,644
R-squared	0.87	0.89	0.86	0.89
Mean of dep. var.	6.28	6.01	5.98	6.15
S.d. of dep. var.	1.01	1.19	0.97	0.93

Notes: This table summarizes estimation results based on municipality-level data. The dependent variable is equal to the share of local workers. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.8: Estimation of the Effect of the Soviet Occupation on Commuting Behavior

	Commuting workers			
	Distance to the demarcation line of			
	0-10km	10-20km	20-30km	30-40km
	(I)	(II)	(III)	(IV)
1971 × Soviet zone	0.084***	0.011**	-0.017***	-0.003
	(0.014)	(0.006)	(0.004)	(0.002)
1981 × Soviet zone	0.088***	-0.000	-0.002	-0.005*
	(0.019)	(0.008)	(0.005)	(0.002)
1991 × Soviet zone	0.100***	0.029***	0.015**	0.020***
	(0.021)	(0.009)	(0.006)	(0.004)
2001 × Soviet zone	0.159***	0.058***	0.022***	0.009***
	(0.023)	(0.010)	(0.006)	(0.003)
Pair-Year FE	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes
No. pairs	912	912	912	912
No. unique municipal.	971	971	971	971
No. periods	4	4	4	4
No. observations	1,044	1,827	1,675	1,560
R-squared	0.51	0.48	0.54	0.60
Mean of dep. var.	0.15	0.06	0.03	0.02
S.d. of dep. var.	0.15	0.08	0.06	0.03

Notes: This table summarizes estimation results based on municipality-level data. The dependent variable is equal to the share of commuters. In addition, pair-wise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.9: Estimation of the Effect of the Soviet Occupation on Productivity (in Terms of Wage Residuals)

	Robustness for Productivity			
	Distance to the demarcation line of			
	0-10km	10-20km	20-30km	30-40km
	(I)	(II)	(III)	(IV)
1972 × Soviet zone	-0.058***	-0.095***	-0.129***	-0.053***
	(0.014)	(0.013)	(0.017)	(0.018)
1981 × Soviet zone	-0.058***	-0.062***	-0.117***	-0.096***
	(0.012)	(0.010)	(0.014)	(0.013)
1991 × Soviet zone	-0.059***	-0.028***	-0.083***	-0.064***
	(0.010)	(0.010)	(0.012)	(0.010)
2001 × Soviet zone	-0.059***	-0.034***	-0.072***	-0.084***
	(0.012)	(0.010)	(0.014)	(0.011)
2011 × Soviet zone	-0.057***	-0.028**	-0.080***	-0.134***
	(0.013)	(0.011)	(0.015)	(0.013)
Pair-year FE	Yes	Yes	Yes	Yes
Flex. control variables	Yes	Yes	Yes	Yes
No. pairs	131	229	210	195
No. unique municipal.	155	238	214	199
No. periods	5	5	5	5
No. observations	445,352	787,101	435,200	565,561
R-squared	0.84	0.87	0.82	0.82
Mean of dep. var.	-0.02	-0.01	0.01	0.01

Notes: This table summarizes estimation results based on municipality-level data. The cities Linz and Steyr are excluded, since in both cases the demarcations disunited the city. The dependent variable is the wage residuals for bordering municipalities. The control variables in each specification are interacted with year dummies and include a Soviet zone dummy and the log population in 1934 and 1939. In addition, pair-wise year fixed effects, where pairs are given by neighboring municipalities along the (pseudo-) demarcation line, are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.10: Estimation of the Effect of the Soviet Occupation on Trust in Austria

	Trust towards others in 2008/09		
	(I)	(II)	(III)
Soviet zone	0.005	-0.023	-0.021
	(0.024)	(0.023)	(0.025)
Exogenous control variables			
Female		0.077***	0.082***
		(0.025)	(0.027)
Born in 1970's		-0.086***	-0.105***
		(0.020)	(0.020)
Born in 1980's		-0.053***	-0.078***
		(0.018)	(0.016)
Endogenous control variables			
Educ. (apprenticeship)			0.062**
			(0.025)
Educ. (middle school)			0.106***
			(0.038)
Educ. (high school)			0.306***
			(0.059)
Educ. (tertiary)			-0.304***
			(0.039)
Married			0.024
			(0.018)
Divorced			-0.006
			(0.059)
Widowed			-0.087
			(0.078)
Numer of children			-0.022**
			(0.009)
Urban area			-0.011
			(0.023)
Pair FE	Yes	Yes	Yes
No. pairs	22	22	22
No. unique districts	23	23	23
No. unique indiv.	878	878	878
No. observations	1,998	1,998	1,998
R-squared	0.03	0.03	0.07
Mean of dep. var.	0.42	0.42	0.42
S.d. of dep. var.	0.49	0.49	0.49

Notes: This table summarizes estimation results based on district-level data. The city Linz is excluded, since the demarcation disunited the city. The dependent variable is an indicator variable which takes the value 1 if the survey participants state they trust others. The specification includes the variable listed and controls which are plausibly exogenous (and endogenous controls in (III) as well). In addition, pairwise year fixed effects (where pairs are given by neighboring municipalities along the demarcation line) are included. The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.11: Estimation of the Effect of the Soviet Occupation on Firms and Workers by Sector

					Robustness: long series			
	Workers		Firms		Workers		Firms	
	manuf.	service	manuf.	service	manuf.	service	manuf.	service
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Pre-WWII differences								
1902 × Soviet zone					0.006	0.047	-0.011	0.159
					(0.165)	(0.210)	(0.159)	(0.213)
Base-year (1930) differences								
Soviet zone	-0.194	-0.235	-0.217	-0.199	0.030	-0.154	-0.098	-0.156
	(0.257)	(0.234)	(0.206)	(0.198)	(0.324)	(0.304)	(0.176)	(0.247)
Post-WWII differences								
1954 × Soviet zone			-0.090***	-0.078***			-0.083**	-0.062**
			(0.029)	(0.023)			(0.037)	(0.028)
1964 × Soviet zone	0.000	0.000	-0.125**	-0.172***	0.000	0.000	-0.134**	-0.172***
	(.)	(.)	(0.045)	(0.035)	(.)	(.)	(0.049)	(0.043)
1973 × Soviet zone	-0.303***	-0.261**	-0.166**	-0.207***	-0.389***	-0.224	-0.216**	-0.212**
	(0.102)	(0.106)	(0.075)	(0.058)	(0.121)	(0.143)	(0.078)	(0.075)
1981 × Soviet zone	-0.383***	-0.282**	-0.208**	-0.226***	-0.464***	-0.247	-0.227**	-0.233***
	(0.106)	(0.108)	(0.077)	(0.060)	(0.150)	(0.145)	(0.082)	(0.073)
1991 × Soviet zone	-0.370***	-0.332**	-0.222***	-0.298***	-0.417**	-0.303	-0.255***	-0.304***
	(0.125)	(0.133)	(0.075)	(0.082)	(0.192)	(0.175)	(0.077)	(0.101)
2001 × Soviet zone	-0.341**	-0.362**	-0.265***	-0.319***	-0.406*	-0.338	-0.314***	-0.314**
	(0.144)	(0.147)	(0.074)	(0.087)	(0.227)	(0.198)	(0.081)	(0.109)
2011 × Soviet zone	-0.341**	-0.370**	-0.300***	-0.309***	-0.432	-0.336	-0.364***	-0.300**
	(0.158)	(0.171)	(0.069)	(0.103)	(0.245)	(0.236)	(0.075)	(0.136)
Pair-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	240	240	320	320	182	182	234	234
No. pairs	20	20	20	20	13	13	13	13
No. unique districts	21	21	21	21	15	15	15	15
No. periods	6	6	8	8	7	7	9	9
R-squared	0.66	0.81	0.66	0.73	0.60	0.90	0.78	0.78
Mean of dep. var.	8.91	8.88	6.47	7.26	9.11	8.75	6.69	7.31

Notes: This table summarizes estimation results based on district-level data. The cities Linz is excluded, since the demarcation disunited the city. The dependent variable is equal to the log of population/workers. The specification includes the variables listed and pair-wise year fixed effects (where pairs are given by neighboring districts along the demarcation line). The method of estimation is least squares. Robust standard errors (allowing for clustering by municipality and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

B.3 Data Sources and Literature

Table B.12: Census Data at the Municipality Level in Austria

Variable	Year	Source
Population	1900-2011	"Ein Blick auf die Gemeinde." (http://www.statistik.at/blickgem). Statistik Austria. Wien.
Population (food-stamps)	1943-45	Statistische Übersichten für den Reichsgau Oberdonau. 1943-45. Statistisches Amt für die Alpen- und Donaugaue. Wien.
Population (food-stamps)	1946	"Gemeindeverzeichnis von Österreich." 1948. Statistisches Zentralamt. Wien.
Population	1948	"Gemeindeverzeichnis von Österreich." 1949. Statistisches Zentralamt. Wien.
Workers, local workers	1961	"Ergebnisse der Volkszählung vom 21. März 1961." 1963. Statistisches Zentralamt. Wien
Workers, local workers	1971	"Ergebnisse der Volkszählung vom 12. Mai 1971." 1972. Statistisches Zentralamt. Wien
Workers, local workers	1981	"Volkszählung 1981." 1985. Statistisches Zentralamt. Wien
Workers, local workers	1991	"Volkszählung 1991." 1993. Statistisches Zentralamt. Wien
Workers, local workers	2001	"Ein Blick auf die Gemeinde." (http://www.statistik.at/blickgem). Statistik Austria. Wien.
Workers, local workers	2011	"Ein Blick auf die Gemeinde." (http://www.statistik.at/blickgem). Statistik Austria. Wien.
Frontier workers	1971–2001	"Individual level census data (5% sample)." Statistik Austria. Wien.

Table B.13: Firm and Worker Data at the District Level in Austria

Variable	Year	Source
Firms, workers	1902	“Ergebnisse der gewerblichen Betriebszählung vom 3. Juni 1902 in den im Reichsrate vertretenen Königreichen und Ländern.” K. K. Statistischen Zentralkommission. Wien.
Firms, workers	1930	“Gewerbliche Betriebszählung in der Republik Österreich vom 14. Juni 1930.” Bundesamt für Statistik. Wien.
Population by location of birth	1951	“Ergebnisse der Volkszählung vom 1. Juni 1951. Tabellenband 1.” Österreichisches Statistisches Zentralamt. Wien.
Firms	1954	“Nichtlandwirtschaftliche Betriebszählung vom 1. September 1954.” Österreichisches Statistisches Zentralamt. Wien.
Firms, workers	1964	“Betriebsstätten in Österreich. Ergebnisse der Vorerhebung zur nichtlandwirtschaftlichen Betriebszählung vom 10. Oktober 1964.” Österreichisches Statistisches Zentralamt. Wien.
Firms, workers	1981	“Arbeitsstättenzählung 1981.” Österreichisches Statistisches Zentralamt. Wien.
Firms, workers	1991	“Arbeitsstättenzählung 1991.” Österreichisches Statistisches Zentralamt. Wien.
Firms, workers	2001	“Arbeitsstättenzählung 2001.” Statistik Austria. Wien.
Firms, workers	2011	“Arbeitsstättenzählung 2011.” Statistik Austria. Wien.

Table B.14: Overview – Literature on Conflict and Occupation

Study	Year	Journal	Country	Treatment	Outcomes
Imbens and Klaauw	1995	JBES	Netherlands	**Long-run** effects of conscription	10 years after, former conscripts have 5% lower earnings than members of their birth cohort
Ghobarah, Huth, and Russett	2003	APSR	Global	**Long-run** effect of civil wars	Health outcomes 1999 are equal to the direct suffering during the war 1991 – 1997
Abadie and Gardeazabal	2003	AER	Spain	Terrorism in Basque Country (and following truce)	Terrorism decreased GDP/capita by 10 pp. in 1960s – Truce 30 years later increased firm value 1998/99
Eckstein and Tsiddon	2004	JME	Israel	Terrorism in the early 2000s	Decrease in annual consumption per capita by 5% of GDP
Lopez and Wodon	2005	JAE	Rwanda	Rwanda genocide; counterfactual without conflict	GDP/capita, child mortality & schooling are 25% – 30% higher in counterfactual
Guidolin and La Ferrara	2007	AER	Angola	End in violent civil war	Abnormal returns of diamond mines declined (!) by 4 percentage points in short-term
Abadie and Gardeazabal	2008	EER	Global	Macroeconomic effects of terrorism worldwide	A std. dev. increase in terrorism risk is associated with a decrease of FDI by 5% of GDP
Kondylis	2010	JDE	Bosnia	Difference in local levels of violence	Labor force participation of displaced is lower compared to those who stayed
Miguel and Roland	2011	JDE	Vietnam	Bombing intensity across districts	No negative impacts on local poverty rates, consumption, infrastructure or literacy in the long-run
Chamarbagwala and Morán	2011	JDE	Guatemala	Civil War and access to schooling	Up to 30% decrease in schooling, effect stronger for females than males
Besley and Mueller	2012	AER	Northern Ireland	Northern Irish peace process	Neg. correlation between killings and house prices — peace dividend when violence stops
Besley, Fetzer, and Mueller	2015	JEEA	Somalia	Welfare effects of onset of piracy	Piracy leads to 8% – 12% increase in shipping costs (longer routes)
Pinotti	2015	EJ	Southern Italy	Economic costs of organized crime	Presence of mafia lowers GDP per capita by 16%
Ochsner	2023	–	Austria	**Long-run** effect of short, exploitative occupation	Local intra-state reallocation of skilled labor, negative economic consequences
Acemoglu, Hassan, and Tahoun	2018	RFStud	Egypt	Effect of Arab spring protests	Firm value of enterprises connected to group in power decreased by protests
Vishwasrao, Schneider, and Chiang	2019	Kyklos	Global	**Long-run** effects of military occupation	Panel of 214 countries from 1950 to 2013 – Transformative occupations produce positive growth,

Notes: This table summarizes the related literature on conflict and occupation – making no claim to be exhaustive. The details for all listed studies can be found in the list of references in the paper.

B.4 A Simple Spatial Equilibrium Model

In this section, we employ the spatial equilibrium model of Moretti (2011) to derive predictions of the Allied occupation of Austria on her spatial equilibrium for the period after 1955. The resulting model should inform the interpretation of our empirical results. Our focus is a scenario where the population is initially equally distributed across two locations. We refer to this situation as a balanced spatial equilibrium (i.e., under identical parameter values, population is equalized across locations). We then ask the question, under which circumstances a temporary shock (such as the Allied occupation) could move the economy from its initially balanced spatial equilibrium to an unbalanced spatial equilibrium (i.e., population is unevenly distributed across locations). It turns out that agglomeration economies and migration frictions are necessary features for the temporary shock to have a permanent effect.

B.4.1 Model Setup

Households

A mass one of workers choose their residence between two locations, conditional on local wages, housing cost, local amenities, and idiosyncratic location preferences. The indirect utility function of a worker i in location $c \in \{a, b\}$ is

$$U_{ic} = w_c - r_c + A_c + e_{ic}, \tag{B.1}$$

where w_c is the wage rate in location c, r_c is the cost of housing, and A_c is a measure of local amenities. The term e_{ic} represents idiosyncratic preferences for a certain location and is distributed such that the difference between preferences for two locations is uniformly distributed between $-s$ and s:

$$e_{ia} - e_{ib} \sim U[-s, s].$$

The parameter s governs the importance of location preferences for workers.

Households choose their location c to maximize indirect utility. Let N_c be the mass of workers who decide to live in location c.[58] The housing stock in each location follows the supply function

$$r_c = z + kN_c, \tag{B.2}$$

where k is the elasticity of supply of housing.

Firms

Firms in each location use labor and capital to produce a single output good according to a Cobb-Douglas production function

$$\ln Y_c = X_c + hN_c + (1-h)K_c, \tag{B.3}$$

58. To be precise, N_c is the log mass of workers in location c.

where N_c is the log mass of workers, K_c is the log of capital, and h is the labor cost share. The term X_c is location-specific log total factor productivity and is determined by agglomeration economies and frictions, such that

$$X_c = \begin{cases} x + \gamma N_c, & \text{if } N_c \leq N_d,\ c \neq d \\ x + \gamma N_c - \delta, & \text{if } N_c > N_d,\ c \neq d. \end{cases} \tag{B.4}$$

The parameter γ governs the strength of linear agglomeration economies. The term δ represents frictions to population movements, departing from a balanced spatial equilibrium ($N_c = 0.5$). Frictions are modeled as a downward shift in total factor productivity as the population increases.[59] There are two possible interpretations of this friction term: (i) It captures a reduction of production possibilities, since resources are needed to integrate immigrants (such as, to increase physical capacities). (ii) Cost of immigration comprising tangible and intangible cost of newly arriving workers.[60]

The price of the output good is normalized to unity. Firms hire labor from the local labor market at a wage rate w_c and rent capital from the world market at a price q. All markets are competitive and each firm is small.

Since each single firm is small, it does not take agglomeration economies into account when it hires labor and capital. Hence, the first order conditions for profit maximization are:

$$w_c = X_c - (1-h)N_c + (1-h)K_c + \log h \tag{B.5}$$
$$q = X_c + hN_c - hK_c + \log(1-h), \tag{B.6}$$

where w_c is the local wage rate, and q is the world price of capital. Hence, the local wage rate is fully determined by local total factor productivity X_c, since the combination of equation (B.6) with equation (B.5) implies: $w_c = \frac{1}{h}X_c - \frac{1-h}{h}q + \frac{1-h}{h}\log(1-h) + \log h$.

B.4.2 Equilibrium

An equilibrium in this economy consists of population N_c, a capital stock K_c, a wage rate w_c, cost of housing r_c for $c \in \{a, b\}$, such that workers choose location c to maximize indirect utility taking prices as given; firms choose labor and capital input to maximize profit taking prices as given; and prices are such that labor and housing markets clear.

59. Since we are mainly interested in a scenario where we start from a balanced equilibrium, we keep things simple and introduce frictions only for the expanding location. Further, we assume that first-nature characteristics are equal across locations.

60. Another way to introduce frictions to migration would be to impose a fixed utility penalty on workers if they change their location, ie. $c_t \neq c_{t+1}$. This would be mathematically more involved, since a state variable would need to control for the current location of each worker. In that model there would be two scenarios with possible multiple equilibria: (i) one with strong agglomeration economies as in our preferred model, and (ii) one with large migration costs which outweigh any dispersion forces and hence no individual would voluntarily move back to their preferred location after a temporary population shock.

A stable equilibrium is an equilibrium in which a small mass ε of workers can change location without creating an incentive for other workers to change location too.

A worker i will live in location b if $U_{ib} \geq U_{ia}$. Using the first order conditions of the firms problem and the indirect utility function, we see that the utility difference of worker i between locations depends on the relative populations already in place.

$$
\begin{aligned}
U_{ib} - U_{ia} &= (w_b - w_a) - (r_b - r_a) + (A_b - A_a) - (e_{ia} - e_{ib}) && \text{(B.7)}\\
&= \frac{1}{h}(X_b - X_a) - k(N_b - N_a) - s(N_b - N_a) && \text{(B.8)}\\
&= \begin{cases} (N_b - N_a)\left(\frac{1}{h}\gamma - k - s\right), & \text{if } N_b = N_a \\ (N_b - N_a)\left(\frac{1}{h}\gamma - k - s\right) - \frac{1}{h}\delta, & \text{if } N_b > N_a. \end{cases} && \text{(B.9)}
\end{aligned}
$$

The fixed term for migration frictions generates a discontinuity when the population distribution departs from the balanced case. Hence, in any unbalanced equilibrium the fixed migration frictions need to outweigh higher wages through agglomeration economies. This idea is summarized in the following proposition.

In the described economy generically, both, a stable balanced and an unbalanced equilibrium exist if and only if (i) agglomeration economies exceed dispersion forces ($\frac{1}{h}\gamma > k + s + \frac{1}{h}\delta$) and (ii) there are positive migration frictions ($\delta > 0$).

The possible equilibrium combinations are summarized in Table B.15. Proposition B.4.2 states that only in the lower right panel, multiple stable equilibria exist.

Table B.15: Possible Stable Equilibria

	Dispersion forces exceed agglomeration economies ($\frac{1}{h}\gamma < k + s + \frac{1}{h}\delta$)	Agglomeration economies exceed dispersion forces ($\frac{1}{h}\gamma > k + s + \frac{1}{h}\delta$)
No frictions ($\delta = 0$)	Balanced equ.	Unbalanced equ.
Frictions ($\delta > 0$)	Balanced equ.	Balanced equ. + Unbalanced equ.

B.4.3 Equilibrium Selection

Proposition B.4.2 states that only if there are strong agglomeration economies and frictions to migration, multiple stable equilibria exist. While the total welfare would be higher in an unbalanced equilibrium, workers can not coordinate their location choices to deviate from a balanced equilibrium. An exogenous shock to local amenities (such as the Allied occupation), causing migration between locations, could act as a coordination device. If the migration shock is sufficiently large, and hence generates a wage differential between locations through agglomeration economies which outweigh migration frictions, the economy is pushed to the unbalanced equilibrium. The threshold for the size of the necessary population difference is given by

$$(N_b - N_a)^\star = \frac{\delta}{h\left(\frac{1}{h}\gamma - k - s\right)}. \tag{B.10}$$

Every population shock that exceeds $(N_b - N_a)^\star$ will push the economy to an unbalanced equilibrium.

We conclude from the model that a temporary local amenity shock (the Allied occupation) can only push the economy from a stable balanced equilibrium to an unbalanced equilibrium if the economy features (i) frictions to migration, and (ii) agglomeration economies that exceed the dispersion forces (see lower right panel in Table B.15).

C Appendix for Chapter 3

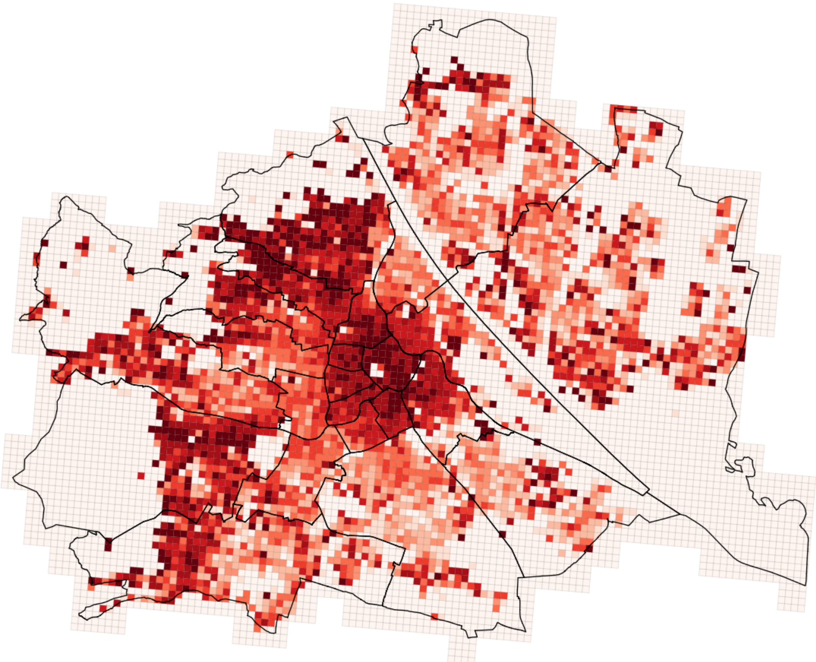
C.1 Additional Figures

Figure C.1: Bombing Data for Vienna - "Kriegssachschadensplan 1946"

Notes: The image above is a screenshot of one of 48 digitized maps in Wiener Stadt- und Landesarchiv (2025c).

Figure C.2: Inequality and Firm Outcomes across Vienna

Panel A: Stylized Facts - Gini Index per Grid-Cell



Panel B: Stylized Facts - Number of Firms per Grid-Cell

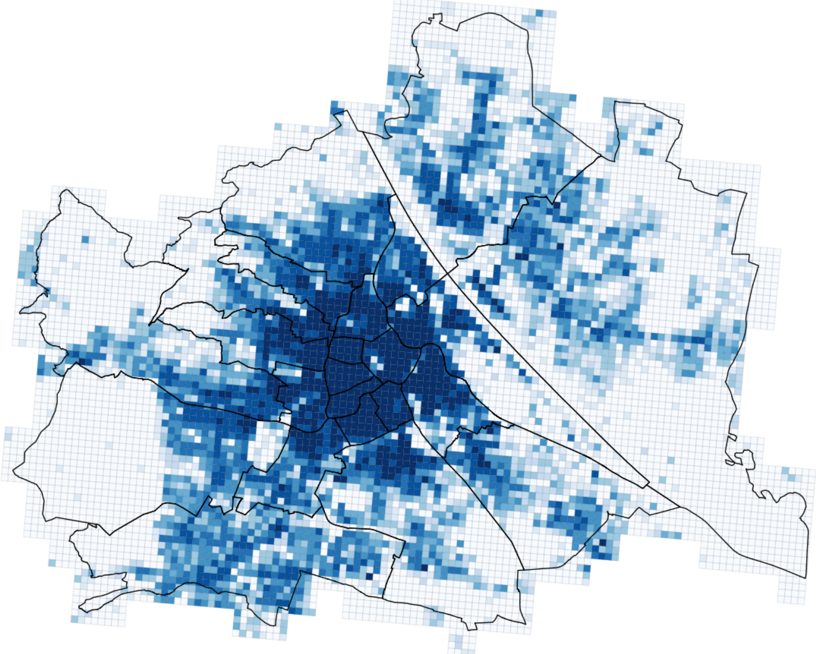
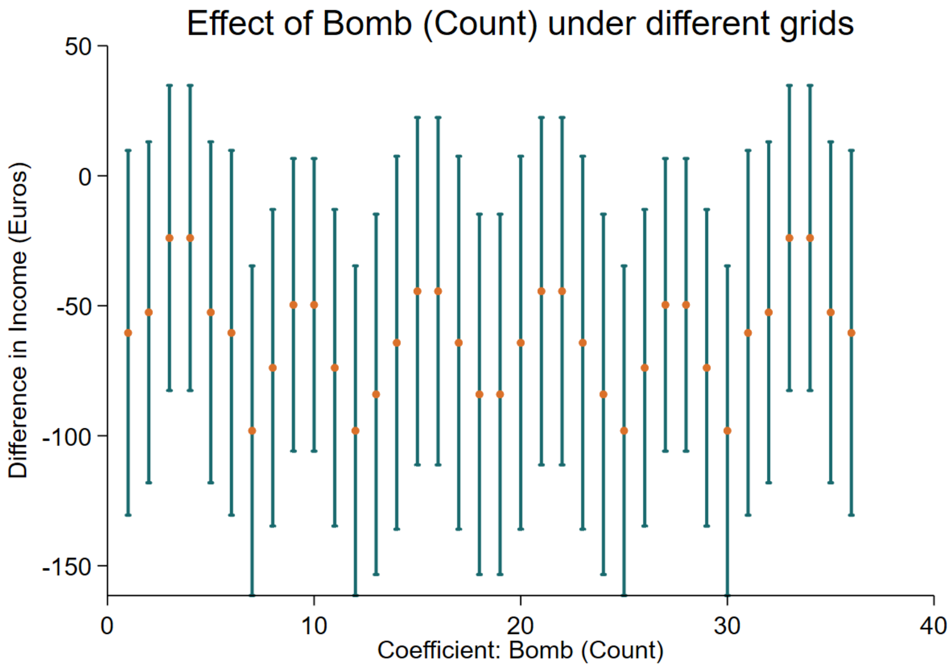
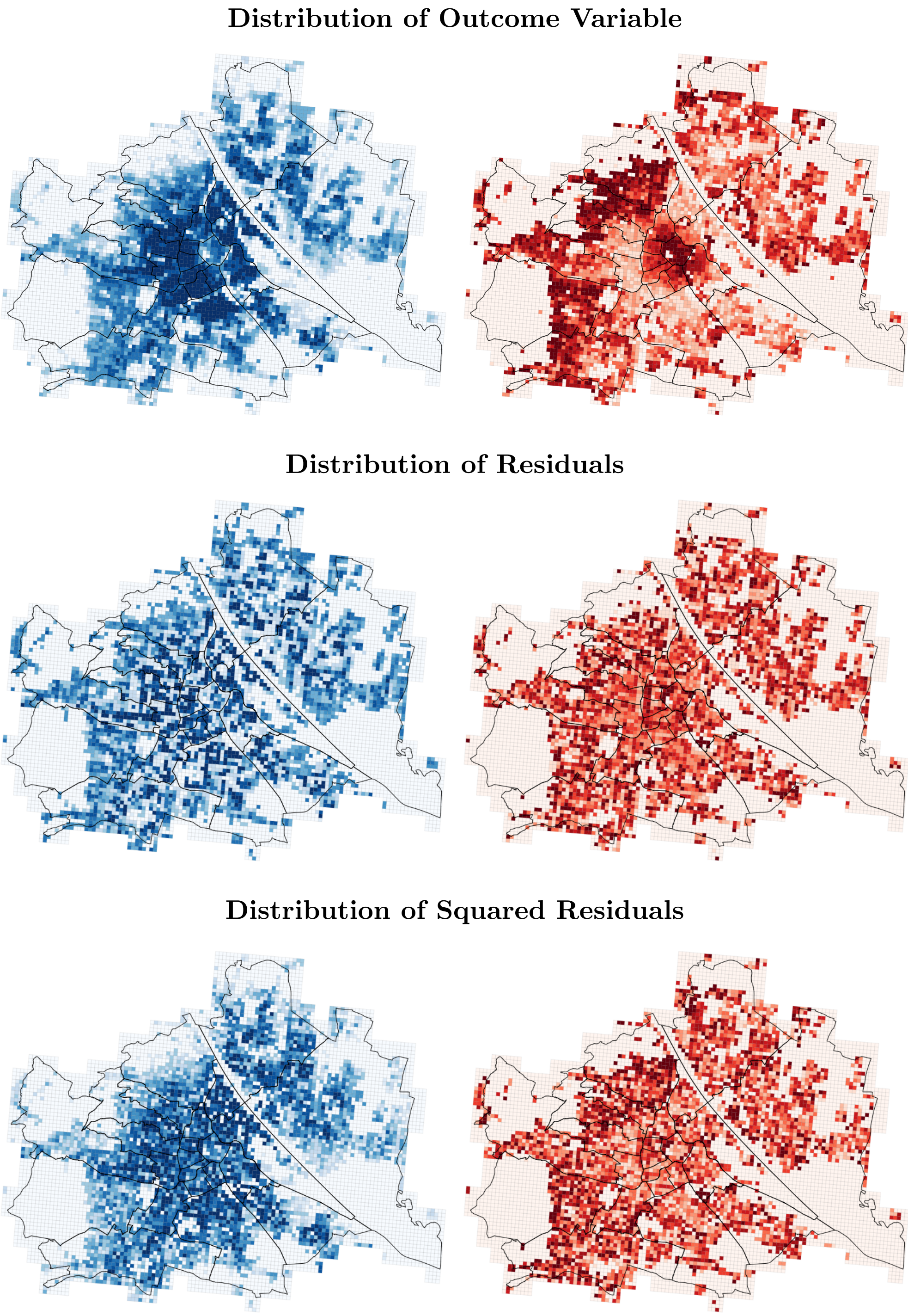


Figure C.3: Robustness: ”Grid and Shake”



Notes: The outcome variable for the graph is income, the different estimations displayed above are permutations of the same regression using the method of Békés and Harasztosi (2018).

Figure C.4: Robustness: Geography of (Squared) Residuals



Notes: The outcome variable for the graphs shaded blue on the left is population, for the ones shaded red on the right it is income.

Figure C.5: Plaque on a rebuilt housing estate

Translated Text: *"This building was damaged during the war years of 1939/45 and was restored in 1959/60 under Federal Chancellor Julius Raab using funds from the Federal Ministry of Trade and Reconstruction." (Image by the author)*

C.2 Additional Tables

Table C.1: Overview of Dependent Variable - Bombing of Vienna

	Dependent Variable: Bombing			
	Mean	**Std. Dev.**	**Min**	**Max**
	(I)	(II)	(III)	(IV)
Damage (All)				
Bomb Indicator	0.20	0.40	0	1
Bomb Count	1.67	5.13	0	56
Bomb Share (0–1)	0.04	0.12	0	2
Damage (Heavy & Burned-Out)				
Bomb Indicator	0.16	0.37	0	1
Bomb Count	0.83	2.77	0	35
Bomb Share (0–1)	0.02	0.07	0	1.2
Observations	4223			

Notes: This table summarizes the variables used for the quasi-natural experiment. The explanatory variable is an indicator variable (which is equal to 1 if there was any bombing), the number of buildings bombed per grid-cell and the share of buildings bombed per grid-cell).

Table C.2: Estimation of the Effect of WW2 Bombing on Flat Size

	Effect on Flat Size					
	1 Room	**2 Rooms**	**3 Rooms**	**4 Rooms**	**5 Rooms**	**6+ Rooms**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	-0.002	0.056	0.165**	-0.021	-0.071*	-0.126**
	(0.047)	(0.062)	(0.067)	(0.052)	(0.041)	(0.054)
Distance (Center)	-0.092	-1.208*	-1.689**	-0.057	1.581**	1.465
	(0.874)	(0.703)	(0.854)	(0.927)	(0.730)	(0.928)
Constant	10.842*	20.981***	34.828***	25.618***	4.303	3.428
	(6.044)	(4.861)	(5.909)	(6.419)	(5.052)	(6.427)
No. of Obs.	3,937	3,937	3,937	3,937	3,937	3,937
R-squared	0.25	0.33	0.31	0.21	0.33	0.42
Mean (dep. var.)	10.20	12.68	23.30	25.20	15.17	13.44
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	-0.063	0.097	0.156***	-0.055	-0.070*	-0.064
	(0.064)	(0.060)	(0.054)	(0.056)	(0.040)	(0.053)
Distance (Center)	-0.183	-0.964**	-1.462**	0.156	0.295	2.157***
	(0.493)	(0.485)	(0.597)	(0.560)	(0.486)	(0.714)
Constant	11.546***	19.246***	33.281***	24.134***	13.203***	-1.409
	(3.443)	(3.374)	(4.152)	(3.907)	(3.386)	(4.971)
No. of Obs.	3,969	3,969	3,969	3,969	3,969	3,969
R-squared	0.12	0.20	0.20	0.09	0.22	0.30
Mean (dep. var.)	10.22	12.64	23.26	25.17	15.19	13.52

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the share of different number of rooms. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.3: Estimation of the Effect of WW2 Bombing on Real Estate Prices (by Type)

	Effect on Real Estate (by Type)			
	Land	**Buildings**	**Flats (all)**	**Flats (by type)**
	(I)	(II)	(III)	(IV)
Distance (Center)	0.110***	0.022	0.077***	0.074***
	(0.036)	(0.060)	(0.023)	(0.023)
Restitution	0.445***	0.027		
	(0.101)	(0.186)		
Aryanization	0.508**	0.220*		
	(0.255)	(0.126)		
War damage	1.240***	0.249	-0.279	
	(0.191)	(0.160)	(0.288)	
Construction	0.631***			
	(0.083)			
Farmland	-0.648**			
	(0.266)			
Forest	-0.717			
	(0.789)			
Age at Contract (Cat. 1)		-0.002		
		(0.157)		
Age at Contract (Cat. 2)		-0.086		
		(0.115)		
Age at Contract (Cat. 3)		-0.054		
		(0.126)		
Age at Contract (Cat. 4)		-0.033		
		(0.146)		
Garage (Building)		-0.164		
		(0.555)		
Inventory (Building)		0.021		
		(0.252)		
Cellar		0.476		
		(0.636)		
Age at Contract (Cat. 1)			-0.011	-0.001
			(0.205)	(0.205)
Age at Contract (Cat. 2)			-0.433**	-0.410**
			(0.193)	(0.196)
Age at Contract (Cat. 3)			-0.443**	-0.410**
			(0.204)	(0.206)
Age at Contract (Cat. 4)			-0.156	-0.139
			(0.227)	(0.228)
Age at Contract (Cat. 5)			-0.949***	-0.925***
			(0.218)	(0.219)
Age at Contract (Cat. 6)			-0.713***	-0.688***
			(0.207)	(0.208)
Garage (Flat)			-0.240	-0.246
			(0.275)	(0.274)
Flat (New)			0.036	0.022
			(0.182)	(0.183)
Inventory (Flat)			-0.003	-0.009
			(0.239)	(0.240)
War damage (Affected)				-0.481
				(0.352)
War damage (Destroyed)				0.358
				(0.457)
Constant	0.977	5.699***	4.419***	4.517***
	(0.937)	(1.531)	(1.256)	(1.235)
No. of Obs.	1,133	1,050	575	575
R-squared	0.32	0.40	0.42	0.42
Mean (dep.var.)	4.20	6.26	8.20	8.20

Notes: This table summarizes estimation results based on estate-level data supplied by Haider, Lampe, and Rieder (2021). The dependent variable is equal to the log of real-estate price (normalized in 1991 ATS). All regressions include year fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.4: Estimation of the Effect of WW2 Bombing on Real Estate Prices (by Year)

	Effect on Real Estate (by Year)		
	1951	**1961**	**1991**
	(I)	(II)	(III)
Distance (Center)	0.009	0.190***	0.036*
	(0.065)	(0.045)	(0.021)
Restitution	0.356	0.254	0.000
	(0.246)	(0.255)	(.)
War damage	0.726***	0.389**	0.784***
	(0.161)	(0.177)	(0.159)
Construction	0.536***	0.599***	0.217*
	(0.120)	(0.151)	(0.111)
Contract Flat	0.000	1.050***	2.606***
	(.)	(0.157)	(0.129)
Contract Building	-0.703**	0.000	1.328***
	(0.325)	(.)	(0.147)
Contract Land	-1.994***	-1.119***	0.000
	(0.320)	(0.136)	(.)
Constant	4.913***	-0.745	3.651***
	(1.416)	(1.422)	(1.315)
No. of Obs.	754	741	926
R-squared	0.30	0.32	0.42
Mean (dep. var.)	4.31	5.45	7.77

Notes: This table summarizes estimation results based on estate-level data supplied by Haider, Lampe, and Rieder (2021). The dependent variable is equal to the log of real-estate price (normalized in 1991 ATS). The method of estimation is least squares. Robust standard errors (allowing for heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.5: Estimation of the Effect of WW2 Bombing on Age Distribution

	Effect on Age Distribution				
	under 15	**15–34**	**35–49**	**50–64**	**over 65**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	0.001	0.110**	-0.028	-0.017	-0.066
	(0.025)	(0.045)	(0.023)	(0.028)	(0.049)
Distance (Center)	0.588	-0.567	-0.333	0.075	0.237
	(0.386)	(0.604)	(0.464)	(0.496)	(0.765)
Constant	9.852***	26.954***	25.325***	19.344***	18.525***
	(2.677)	(4.191)	(3.224)	(3.440)	(5.308)
No. of Obs.	3,951	3,951	3,951	3,951	3,951
R-squared	0.20	0.35	0.19	0.23	0.23
Mean (dep. var.)	13.93	23.12	22.99	19.85	20.11
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	-0.020	0.180***	-0.025	-0.043	-0.092
	(0.025)	(0.056)	(0.025)	(0.033)	(0.067)
Distance (Center)	0.117	-0.649	0.044	0.361	0.127
	(0.242)	(0.461)	(0.292)	(0.304)	(0.537)
Constant	13.131***	27.464***	22.694***	17.421***	19.290***
	(1.694)	(3.206)	(2.047)	(2.120)	(3.740)
No. of Obs.	3,981	3,981	3,981	3,981	3,981
R-squared	0.08	0.23	0.07	0.10	0.11
Mean (dep. var.)	13.93	23.10	22.98	19.90	20.09

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the share of population at different age brackets. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.6: Estimation of the Effect of WW2 Bombing on Place of Birth

	Effect on Distribution of Region of Origin					
	Austrian	**EU Core**	**EU Ext.**	**Fmr. Yug.**	**Turkey**	**Africa**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	-0.182**	0.035**	0.089***	0.047	0.014	0.001
	(0.090)	(0.018)	(0.032)	(0.039)	(0.022)	(0.008)
Distance (Center)	2.211***	-0.182	-0.093	-1.026**	-0.141	-0.175*
	(0.853)	(0.227)	(0.403)	(0.437)	(0.198)	(0.091)
Constant	61.313***	4.856***	7.390***	12.305***	3.034**	2.059***
	(5.918)	(1.574)	(2.796)	(3.034)	(1.377)	(0.630)
No. of Obs.	3,951	3,951	3,951	3,951	3,951	3,951
R-squared	0.45	0.33	0.23	0.34	0.38	0.26
Mean (dep. var.)	76.48	3.62	6.83	5.23	2.07	0.85
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	-0.290***	0.057***	0.111***	0.085*	-0.008	0.001
	(0.106)	(0.016)	(0.036)	(0.049)	(0.024)	(0.007)
Distance (Center)	1.553**	-0.309**	0.149	-0.309	-0.310**	-0.153***
	(0.601)	(0.144)	(0.231)	(0.287)	(0.146)	(0.057)
Constant	65.930***	5.739***	5.713***	7.296***	4.229***	1.904***
	(4.191)	(1.006)	(1.612)	(2.001)	(1.019)	(0.398)
No. of Obs.	3,981	3,981	3,981	3,981	3,981	3,981
R-squared	0.34	0.25	0.10	0.23	0.27	0.14
Mean (dep. var.)	76.49	3.64	6.84	5.22	2.06	0.84

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the share of peoples' origin country. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.7: Estimation of the Effect of WW2 Bombing on Education

	Effect on Education			
	Middle School	**Apprenticeship**	**High School**	**University**
	(I)	(II)	(III)	(IV)
PANEL A. 1 sq. km grid-cell fixed-effects				
Bomb Count	-0.045	-0.144***	0.065**	0.124**
	(0.069)	(0.044)	(0.033)	(0.061)
Distance (Center)	-1.461*	0.102	0.420	0.938
	(0.795)	(0.742)	(0.529)	(0.699)
Constant	32.993***	37.948***	15.882***	13.177***
	(5.526)	(5.152)	(3.670)	(4.856)
No. of Obs.	3,951	3,951	3,951	3,951
R-squared	0.39	0.50	0.29	0.59
Mean (dep. var.)	22.82	38.53	18.85	19.79
PANEL B. 4 sq. km grid-cell fixed-effects				
Bomb Count	-0.039	-0.210***	0.065**	0.184***
	(0.069)	(0.052)	(0.032)	(0.067)
Distance (Center)	-0.480	1.454***	-0.161	-0.813
	(0.595)	(0.535)	(0.348)	(0.661)
Constant	26.158***	28.625***	19.931***	25.287***
	(4.156)	(3.748)	(2.434)	(4.625)
No. of Obs.	3,981	3,981	3,981	3,981
R-squared	0.28	0.39	0.19	0.48
Mean (dep. var.)	22.78	38.56	18.87	19.79

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the share of peoples' highest educational attainment. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.8: Estimation of the Effect of WW2 Bombing on People Still in Education

	Effect on Still in Education				
	Primary	**Middle**	**Apprenticeship**	**High**	**University**
	(I)	(II)	(III)	(IV)	(V)
PANEL A. 1 sq. km grid-cell fixed-effects					
Bomb Count	-0.010	-0.017**	-0.008**	-0.005	0.064**
	(0.010)	(0.008)	(0.004)	(0.012)	(0.025)
Distance (Center)	0.046	-0.279**	0.024	0.488*	-0.309
	(0.147)	(0.130)	(0.097)	(0.280)	(0.283)
Constant	3.319***	3.721***	0.729	1.972	7.403***
	(1.023)	(0.903)	(0.672)	(1.943)	(1.965)
No. of Obs.	3,951	3,951	3,951	3,951	3,951
R-squared	0.17	0.22	0.19	0.21	0.34
Mean (dep. var.)	3.63	1.77	0.89	5.35	5.32
PANEL B. 4 sq. km grid-cell fixed-effects					
Bomb Count	-0.017	-0.016**	-0.009**	-0.018*	0.083***
	(0.011)	(0.008)	(0.004)	(0.010)	(0.025)
Distance (Center)	-0.044	0.002	-0.000	0.301*	-0.354
	(0.093)	(0.084)	(0.047)	(0.158)	(0.221)
Constant	3.946***	1.764***	0.894***	3.283***	7.704***
	(0.648)	(0.585)	(0.331)	(1.104)	(1.536)
No. of Obs.	3,981	3,981	3,981	3,981	3,981
R-squared	0.07	0.10	0.10	0.11	0.25
Mean (dep. var.)	3.63	1.77	0.89	5.36	5.31

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the share of people if different types of schooling. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.9: Estimation of the Effect of WW2 Bombing on Labor Outcomes

	Effect on Labor Outcomes					
	Employed	**Unemployed**	**Workers**	**Managers**	**Self-Emp.**	**Support Fam.**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	0.035	-0.002	-0.021	-0.041	0.061**	0.001
	(0.041)	(0.018)	(0.042)	(0.027)	(0.027)	(0.002)
Distance (Center)	0.279	-0.076	-0.719	0.587*	0.256	-0.124
	(0.666)	(0.239)	(0.656)	(0.309)	(0.448)	(0.164)
Constant	43.735***	4.597***	92.918***	-0.550	6.374**	1.258
	(4.622)	(1.660)	(4.547)	(2.143)	(3.109)	(1.139)
No. of Obs.	3,951	3,951	3,941	3,941	3,941	3,941
R-squared	0.22	0.29	0.37	0.28	0.29	0.35
Mean (dep. var.)	45.70	4.07	87.91	3.48	8.20	0.40
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	0.073*	0.010	-0.062	-0.020	0.077**	0.005
	(0.042)	(0.021)	(0.038)	(0.019)	(0.030)	(0.005)
Distance (Center)	0.489	-0.177	-0.205	0.009	0.063	0.133
	(0.480)	(0.174)	(0.618)	(0.282)	(0.336)	(0.142)
Constant	42.276***	5.284***	89.382***	3.454*	7.692***	-0.529
	(3.357)	(1.214)	(4.306)	(1.972)	(2.338)	(0.996)
No. of Obs.	3,981	3,981	3,971	3,971	3,971	3,971
R-squared	0.09	0.18	0.22	0.11	0.19	0.16
Mean (dep. var.)	45.75	4.06	87.90	3.50	8.20	0.40

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of people (un-)employed. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.10: Estimation of the Effect of WW2 Bombing on Sectoral Employment

	Effect on Sector of Employment		
	Agriculture	**Industry**	**Services**
	(I)	(II)	(III)
PANEL A. 1 sq. km grid-cell fixed-effects			
Bomb Count	0.010*	0.037	-0.046
	(0.006)	(0.032)	(0.032)
Distance (Center)	0.122	-0.091	-0.031
	(0.407)	(0.576)	(0.656)
Constant	0.309	13.569***	86.122***
	(2.822)	(3.998)	(4.551)
No. of Obs.	3,941	3,941	3,941
R-squared	0.53	0.24	0.35
Mean (dep. var.)	1.16	12.97	85.86
PANEL B. 4 sq. km grid-cell fixed-effects			
Bomb Count	0.014	0.001	-0.014
	(0.019)	(0.032)	(0.035)
Distance (Center)	0.276	0.394	-0.670
	(0.528)	(0.328)	(0.515)
Constant	-0.748	10.244***	90.504***
	(3.689)	(2.293)	(3.599)
No. of Obs.	3,971	3,971	3,971
R-squared	0.29	0.16	0.23
Mean (dep. var.)	1.18	12.99	85.83

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the sectoral employment share. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.11: Estimation of the Effect of WW2 Bombing on Service Employment

	Effect on Service Employment							
	Commerce	**Transport**	**Hospitality**	**Inform.**	**Finance**	**Public**	**Education**	**Health**
	(I)	(II)	(III)	(IV)	(V)	(VI)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects								
Bomb Count	-0.007	-0.024	0.018	0.039**	0.003	-0.055*	0.024	0.011
	(0.022)	(0.018)	(0.025)	(0.016)	(0.017)	(0.028)	(0.022)	(0.024)
Distance (Center)	0.133	0.090	-0.496	0.296	0.306	0.057	-0.231	0.221
	(0.539)	(0.380)	(0.303)	(0.301)	(0.271)	(0.404)	(0.423)	(0.396)
Constant	14.523***	4.403*	8.847***	2.672	2.392	8.484***	10.076***	5.968**
	(3.737)	(2.634)	(2.104)	(2.087)	(1.881)	(2.803)	(2.935)	(2.745)
No. of Obs.	3,941	3,941	3,941	3,941	3,941	3,941	3,941	3,941
R-squared	0.22	0.23	0.23	0.18	0.20	0.19	0.19	0.17
Mean (dep. var.)	15.44	5.01	5.42	4.75	4.52	8.83	8.50	7.51
PANEL B. 4 sq. km grid-cell fixed-effects								
Bomb Count	-0.016	-0.022	0.037	0.029**	0.008	-0.092***	0.027	0.001
	(0.023)	(0.019)	(0.025)	(0.014)	(0.015)	(0.031)	(0.021)	(0.022)
Distance (Center)	0.572	0.460**	-0.393**	-0.069	0.139	0.086	-0.178	-0.059
	(0.360)	(0.213)	(0.195)	(0.167)	(0.184)	(0.235)	(0.212)	(0.222)
Constant	11.495***	1.823	8.132***	5.200***	3.540***	8.312***	9.686***	7.919***
	(2.517)	(1.488)	(1.365)	(1.171)	(1.284)	(1.644)	(1.477)	(1.552)
No. of Obs.	3,971	3,971	3,971	3,971	3,971	3,971	3,971	3,971
R-squared	0.11	0.10	0.13	0.10	0.12	0.09	0.10	0.09
Mean (dep. var.)	15.46	5.00	5.43	4.75	4.51	8.83	8.47	7.51

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the sectoral employment share in services. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.12: Estimation of the Effect of WW2 Bombing on Firms (Count)

	Effect on Firm Size (Count)			
	Size 0–1	**Size** < 5	**Size** < 100	**Size** > 100
	(I)	(II)	(III)	(IV)
PANEL A. 1 sq. km grid-cell fixed-effects				
Bomb Count	0.380***	-0.033	-0.264***	-0.083***
	(0.095)	(0.054)	(0.091)	(0.023)
Distance (Center)	0.953	-0.654	-0.240	-0.060
	(1.362)	(0.801)	(1.114)	(0.256)
Constant	55.896***	24.207***	18.329**	1.569
	(8.982)	(5.291)	(7.341)	(1.690)
No. of Obs.	3,325	3,325	3,325	3,325
R-squared	0.33	0.16	0.32	0.18
Mean (dep. var.)	62.58	19.87	16.47	1.09
PANEL B. 4 sq. km grid-cell fixed-effects				
Bomb Count	0.247***	0.049	-0.211***	-0.085***
	(0.086)	(0.057)	(0.080)	(0.030)
Distance (Center)	0.978	-0.239	-0.700	-0.038
	(1.075)	(0.488)	(0.845)	(0.172)
Constant	55.857***	21.362***	21.351***	1.430
	(7.122)	(3.239)	(5.615)	(1.144)
No. of Obs.	3,343	3,343	3,343	3,343
R-squared	0.19	0.07	0.19	0.09
Mean (dep. var.)	62.57	19.83	16.51	1.09

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of firms. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.13: Estimation of the Effect of WW2 Bombing on Firms (by Sector)

	Effect on Firm Size (by Sector)		
	Agriculture	**Industry**	**Services**
	(I)	(II)	(III)
PANEL A. 1 sq. km grid-cell fixed-effects			
Bomb Count	0.013	0.062	-0.075*
	(0.011)	(0.041)	(0.043)
Distance (Center)	1.014**	-0.081	-0.933
	(0.501)	(0.689)	(0.823)
Constant	-4.855	9.842**	95.013***
	(3.308)	(4.540)	(5.428)
No. of Obs.	3,325	3,325	3,325
R-squared	0.56	0.27	0.37
Mean (dep. var.)	1.84	9.37	88.78
PANEL B. 4 sq. km grid-cell fixed-effects			
Bomb Count	0.028	0.029	-0.057
	(0.025)	(0.055)	(0.060)
Distance (Center)	0.755	0.301	-1.057
	(0.725)	(0.521)	(0.732)
Constant	-3.146	7.336**	95.810***
	(4.814)	(3.459)	(4.862)
No. of Obs.	3,343	3,343	3,343
R-squared	0.30	0.15	0.23
Mean (dep. var.)	1.87	9.36	88.77

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the sectoral share of firms. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.14: Estimation of the Effect of WW2 Bombing on Firms (Service)

	Effect on Firms (Service)							
	Commerce	**Transport**	**Hospitality**	**Inform.**	**Finance**	**Public**	**Education**	**Health**
	(I)	(II)	(III)	(IV)	(V)	(VI)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects								
Bomb Count	-0.007	-0.041	-0.078**	-0.053	-0.031	-0.021	-0.060**	-0.005
	(0.061)	(0.029)	(0.038)	(0.033)	(0.024)	(0.013)	(0.030)	(0.032)
Distance (Center)	-0.447	0.600	-0.327	-1.342**	0.158	-0.028	-0.170	0.458
	(0.900)	(0.509)	(0.602)	(0.591)	(0.292)	(0.124)	(0.400)	(0.648)
Constant	19.221***	0.653	7.327*	15.520***	1.321	0.562	4.996*	4.938
	(5.942)	(3.356)	(3.967)	(3.901)	(1.926)	(0.815)	(2.643)	(4.277)
No. of Obs.	3,325	3,325	3,325	3,325	3,325	3,325	3,325	3,325
R-squared	0.26	0.22	0.23	0.17	0.14	0.13	0.15	0.18
Mean (dep. var.)	16.27	4.56	5.09	6.63	2.33	0.35	3.81	7.95
PANEL B. 4 sq. km grid-cell fixed-effects								
Bomb Count	0.037	-0.091**	-0.049*	-0.062**	-0.015	-0.022**	-0.053**	0.043
	(0.051)	(0.036)	(0.029)	(0.025)	(0.022)	(0.009)	(0.025)	(0.027)
Distance (Center)	0.123	-0.012	-0.723**	-0.029	-0.259*	-0.191**	0.117	0.285
	(0.660)	(0.345)	(0.328)	(0.343)	(0.150)	(0.073)	(0.216)	(0.352)
Constant	15.429***	4.753**	9.919***	6.898***	4.045***	1.640***	3.089**	6.060**
	(4.386)	(2.290)	(2.187)	(2.275)	(0.996)	(0.484)	(1.436)	(2.337)
No. of Obs.	3,343	3,343	3,343	3,343	3,343	3,343	3,343	3,343
R-squared	0.14	0.12	0.12	0.08	0.06	0.04	0.06	0.09
Mean (dep. var.)	16.28	4.58	5.09	6.64	2.32	0.35	3.81	7.99

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the sectoral share of service firms. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.15: Estimation of the Effect of WW2 Bombing on Amenities – Private vs. Public

	Effect on (Private) Amenities					
	Kindergarten	**KG (private)**	**KG (public)**	**Schools**	**Sch (private)**	**Sch (public)**
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count	0.040***	0.001	0.145	-0.002	0.062***	0.005
	(0.009)	(0.003)	(0.129)	(0.003)	(0.023)	(0.004)
Distance (Center)	-0.128***	-0.016	-0.793*	-0.035*	-0.165*	-0.013
	(0.048)	(0.014)	(0.405)	(0.019)	(0.086)	(0.016)
Constant	1.197***	0.161	6.658**	0.288**	1.536**	0.136
	(0.338)	(0.098)	(2.920)	(0.134)	(0.614)	(0.112)
No. of Obs.	4,188	4,188	4,188	4,188	4,188	4,188
R-squared	0.25	0.13	0.33	0.11	0.36	0.09
Mean (dep. var.)	0.33	0.05	1.21	0.04	0.43	0.05
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count	0.039***	-0.002	0.314***	-0.001	0.080***	0.004
	(0.008)	(0.003)	(0.103)	(0.004)	(0.024)	(0.003)
Distance (Center)	-0.060**	-0.007	-0.424	-0.015*	-0.190***	-0.003
	(0.028)	(0.008)	(0.275)	(0.008)	(0.052)	(0.009)
Constant	0.717***	0.102*	3.929*	0.147***	1.706***	0.065
	(0.193)	(0.053)	(1.991)	(0.054)	(0.373)	(0.062)
No. of Obs.	4,220	4,220	4,220	4,220	4,220	4,220
R-squared	0.18	0.07	0.26	0.04	0.31	0.04
Mean (dep. var.)	0.33	0.05	1.20	0.04	0.43	0.05

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of local amenities. The main explanatory variable is equal to the number of buildings bombed per grid-cell. An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.16: Estimation of the Spillover Effect on Demographics

	Effect on Demographics					
	Population		**Austrian**		**EU-Citizen**	
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count (own area)	38.457***	33.357***	-0.182**	-0.073	0.124***	0.086**
	(6.266)	(5.474)	(0.090)	(0.086)	(0.037)	(0.041)
Bomb Count (buffer)		20.526		-0.441**		0.156
		(12.730)		(0.193)		(0.099)
Distance (Center)	-91.329***	-84.173***	2.211***	2.046**	-0.275	-0.217
	(32.719)	(32.093)	(0.853)	(0.858)	(0.457)	(0.463)
Constant	1036.409***	973.685***	61.313***	62.735***	12.246***	11.743***
	(229.704)	(225.445)	(5.918)	(5.975)	(3.170)	(3.223)
No. of Obs.	4,188	4,188	3,951	3,951	3,951	3,951
R-squared	0.63	0.63	0.45	0.45	0.28	0.28
Mean (dep. var.)	426.95	426.95	76.48	76.48	10.45	10.45
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count (own area)	32.036***	31.888***	-0.290***	-0.091	0.168***	0.079**
	(9.449)	(5.976)	(0.106)	(0.063)	(0.039)	(0.040)
Bomb Count (buffer)		0.373		-0.502**		0.224***
		(16.066)		(0.223)		(0.072)
Distance (Center)	-83.002***	-82.866***	1.553**	1.358**	-0.160	-0.073
	(29.787)	(28.560)	(0.601)	(0.622)	(0.271)	(0.276)
Constant	983.038***	981.894***	65.930***	67.543***	11.452***	10.731***
	(208.601)	(200.668)	(4.191)	(4.383)	(1.893)	(1.944)
No. of Obs.	4,220	4,220	3,981	3,981	3,981	3,981
R-squared	0.50	0.50	0.34	0.34	0.15	0.15
Mean (dep. var.)	423.91	423.91	76.49	76.49	10.49	10.49

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is population/share of citizenship groups. The main explanatory variables are equal to the number of buildings bombed (in the same grid-cell/in the surrounding grid-cells). An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.17: Estimation of the Spillover Effect on Income

	Effect on Income					
	Income		**Inc. (Tr.)**		**Sum (Inc.)**	
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count (own area)	-91.61	-48.22	-57.17*	-21.25	659,187***	594,085***
	(67.99)	(69.17)	(34.41)	(35.51)	(100,159)	(89,299)
Bomb Count (buffer)		-175.06		-144.92		262,656
		(152.82)		(93.15)		(200,652)
Distance (Center)	1422.88	1355.40	1232.11*	1176.25*	-1,847,961***	-1,746,714***
	(973.72)	(982.89)	(637.66)	(646.46)	(669,078)	(670,476)
Constant	23,045***	23,625***	18,050***	18,530.69***	21,158,972***	20,288,330***
	(6727.29)	(6810.35)	(4403.80)	(4482.06)	(4,615,848)	(4,644,317)
No. of Obs.	3,859	3,859	3,859	3,859	3,859	3,859
R-squared	0.40	0.40	0.31	0.31	0.58	0.59
Mean (dep. var.)	32,777	32,777	26,498	26,498	9,008,465	9,008,465
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count (own area)	-53.13	-82.02	-68.51*	-35.13	547,540***	555,678***
	(51.35)	(80.51)	(35.17)	(44.63)	(144,816)	(114,133)
Bomb Count (buffer)		72.96		-84.32		-20,552
		(201.32)		(116.87)		(243,468)
Distance (Center)	673.62	702.77	579.76	546.08	-1,407,282***	-1,415,492***
	(722.56)	(709.54)	(382.21)	(380.75)	(506,411)	(491,313)
Constant	28,189***	27,949***	22,578***	22,856***	18,202,781***	18,270,320***
	(5016.85)	(4920.16)	(2650.07)	(2643.26)	(3,493,674)	(3,402,485)
No. of Obs.	3,892	3,892	3,892	3,892	3,892	3,892
R-squared	0.28	0.28	0.21	0.21	0.47	0.47
Mean (dep. var.)	32,811	32,811	26,536	26,536	8,939,368	8,939,368

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is income in 2013 Euros. The main explanatory variables are equal to the number of buildings bombed (in the same grid-cell/in the surrounding grid-cells). An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.18: Estimation of the Spillover Effect on Firms

	Effect on Firms					
	Firms		**Workers**		**Size** < 5	
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count (own area)	4.200***	2.768***	17.114**	15.247**	0.884***	0.488***
	(0.847)	(0.592)	(7.531)	(5.944)	(0.231)	(0.153)
Bomb Count (buffer)		5.768***		7.526		1.589***
		(1.986)		(14.511)		(0.485)
Distance (Center)	-15.594***	-13.399***	-99.538***	-96.675***	-4.040***	-3.349***
	(4.010)	(3.657)	(29.614)	(28.089)	(0.944)	(0.831)
Constant	139.481***	120.632***	904.053***	879.461***	34.403***	28.665***
	(27.361)	(24.856)	(201.513)	(190.674)	(6.127)	(5.392)
No. of Obs.	3,832	3,832	3,832	3,832	3,325	3,325
R-squared	0.67	0.67	0.49	0.49	0.68	0.69
Mean (dep. var.)	36.38	36.38	237.08	237.08	8.71	8.71
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count (own area)	4.192***	2.607***	18.492*	15.244***	0.983***	0.485***
	(1.272)	(0.701)	(9.603)	(5.409)	(0.318)	(0.143)
Bomb Count (buffer)		4.003*		8.202		1.261**
		(2.251)		(15.967)		(0.621)
Distance (Center)	-12.419***	-10.830***	-91.510***	-88.255***	-3.456***	-2.884***
	(2.460)	(2.519)	(31.939)	(29.991)	(0.766)	(0.750)
Constant	117.868***	104.844***	849.272***	822.589***	30.472***	25.949***
	(17.037)	(18.395)	(217.047)	(203.912)	(5.051)	(5.217)
No. of Obs.	3,864	3,864	3,864	3,864	3,343	3,343
R-squared	0.62	0.63	0.39	0.39	0.61	0.62
Mean (dep. var.)	36.10	36.10	235.54	235.54	8.67	8.67

Notes: Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of firms/workers. The main explanatory variables are equal to the number of buildings bombed (in the same grid-cell/in the surrounding grid-cells). An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table C.19: Estimation of the Spillover Effect on Amenities

	Effect on Amenities					
	Kindergarten		**Schools**		**Doctors**	
	(I)	(II)	(III)	(IV)	(V)	(VI)
PANEL A. 1 sq. km grid-cell fixed-effects						
Bomb Count (own area)	0.041***	0.029**	0.013*	0.006	0.210	0.082
	(0.010)	(0.012)	(0.007)	(0.009)	(0.148)	(0.098)
Bomb Count (buffer)		0.048*		0.030		0.514**
		(0.025)		(0.019)		(0.230)
Distance (Center)	-0.144***	-0.128**	-0.059*	-0.048	-1.024**	-0.845*
	(0.049)	(0.051)	(0.034)	(0.034)	(0.473)	(0.504)
Constant	1.358***	1.211***	0.591**	0.500**	8.730**	7.160*
	(0.342)	(0.363)	(0.243)	(0.241)	(3.401)	(3.697)
No. of Obs.	4,188	4,188	4,188	4,188	4,188	4,188
R-squared	0.28	0.28	0.17	0.17	0.37	0.37
Mean (dep. var.)	0.38	0.38	0.19	0.19	1.71	1.71
PANEL B. 4 sq. km grid-cell fixed-effects						
Bomb Count (own area)	0.037***	0.025*	0.018**	0.007	0.396***	0.097**
	(0.009)	(0.013)	(0.008)	(0.007)	(0.120)	(0.047)
Bomb Count (buffer)		0.029		0.027*		0.752***
		(0.031)		(0.016)		(0.283)
Distance (Center)	-0.067**	-0.057*	-0.067***	-0.057***	-0.634*	-0.358
	(0.030)	(0.030)	(0.019)	(0.020)	(0.321)	(0.391)
Constant	0.818***	0.731***	0.644***	0.561***	5.845**	3.541
	(0.210)	(0.212)	(0.138)	(0.146)	(2.319)	(2.943)
No. of Obs.	4,220	4,220	4,220	4,220	4,220	4,220
R-squared	0.21	0.21	0.10	0.10	0.30	0.31
Mean (dep. var.)	0.38	0.38	0.19	0.19	1.70	1.70

*Notes:*Every column in this table represents one individual regression and summarizes estimation results based on 250x250 m grid-level data. The dependent variable is the number of local amenities. The main explanatory variables are equal to the number of buildings bombed (in the same grid-cell/in the surrounding grid-cells). An additional control variable in each specification is distance (to the city center). All regressions include 1x1 km (2x2 km) grid-cell fixed-effects. The method of estimation is least squares. Robust standard errors (allowing for clustering by 1x1 km (2x2 km) grid-cell and heteroskedasticity of unknown form) are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

C.3 Example of Bombing Mission

Mission No. 178: *19 February 1945 – Target: Vienna South Station Area*[61]

"With Mission No. 178 the Group missed another opportunity to register a satisfactory visual bombing score at Vienna. The target was the South Station. North of Judenburg, engine failure forced the formation leader, Major Poole, to leave the formation. After the bomb load had been dropped on a target of opportunity at Wolfsburg, Austria, the plane staggered back to base on two engines. Captain Thackston, who had been flying the Deputy lead position, took over the formation lead. Bad weather split up the formation and only fourteen planes bombed the primary target for a discouraging score of 3 percent. Three of the planes over Vienna were holed by flak and two men were injured. Nine airplanes which had lost the formation made a visual run on the dry docks at Fiume, Italy, but completely overshot the target."

Figure C.6: Planning Documents and Aerial Photograph from the Bombing of Vienna

Notes: The map on the left is from a contemporary US airforce briefing. The photography on the right is an aerial picture taken from a bomber above Vienna. Both pictures are taken from original documents in 461st Bombardment Group (2025).

61. This is an excerpt of bombing reports by the 461st Bombardment Group of the Fifteenth US Air Force. These have been preserved under the following resource: 461st Bombardment Group (2025)

C.4 Background on Housing and Reconstruction

The Reconstruction of Vienna after World War II (1945–1946)[62]

After the end of World War II in 1945, Vienna was physically damaged and socially disrupted. The Austrian capital faced immense challenges in restoring essential services, rebuilding infrastructure, and planning for it's future. The city's reconstruction efforts unfolded within the broader context of the Allied occupation (1945–1955), which shaped political, economic, and logistical realities (Denk 2008).

Initial Steps: Leadership and Priorities

Immediately after the war, Johann Gundacker, then Director of the Vienna City Building Office (*"Stadtbauamt"*), started planning the city's reconstruction. He developed a practical roadmap that prioritized: Clearing and restoring critical roads and transportation routes, repairing tram and bus services, re-establishing electricity, water, and sewage infrastructure. These immediate actions were necessary to restore a basic level of urban function and mobility in a severely damaged city.

The 1945/46 Commission ("Enquete für den Wiederaufbau")

Recognizing the complexity of long-term reconstruction, city officials organized an expert commission (*"Enquete"*), which first convened on July 9, 1945 in Vienna's City Senate Hall, with around 170 professionals participating. These included representatives from Austrian federal ministries (e.g. trade, agriculture, welfare), the police and state security, Vienna's municipal departments, universities, economic chambers, and various urban enterprises. The commission was chaired by Anton Weber, the city councillor responsible for the Building Office. Although the Enquete had no formal decision-making power, it served as a platform to generate expert recommendations and proposals (Wiener Stadt- und Landesarchiv 2025a).

The Enquete was not simply about rebuilding what was lost – it aimed for a transformation of Vienna. It's experts emphasized the need for a structured, sustainable, and modern city, looking beyond the immediate crisis. Key goals included:

- Reducing overcrowding in the city by dispersing development
- Integrating Vienna with its surrounding regions, especially the northern area of *"Transdanubia"* (across the Danube River)
- Revising outdated zoning plans and building codes

62. This section is an adaptation of Wiener Stadt- und Landesarchiv (2025a) that has been translated and summarized, with details from original documents from Stadtbauamt Wien (1946).

- Promoting green spaces and protecting key natural and historic areas (e.g., Prater, Lobau)
- Planning for flood protection, which was especially important in discussions on engineering works

A strong technological optimism shaped the planning, particularly around automobile traffic, with proposals for highways, gas stations, and transit corridors playing a central role. Public transport electrification, new power plants, and even the idea of building a new airport were also on the table.

The 14-Point Program for Vienna's Reconstruction

In January 1946, the Enquete concluded its work by publishing a 14-point program. This plan was structured around three phases: Immediate measures (*"Sofortprogramm"*), Reconstruction program (*"Wiederaufbauprogramm"*) and Future development strategy (*"Zukunftsprogramm"*). It emphasized not merely rebuilding, but building better with a long-term, people-focused vision. The 14 Points of the *Reconstruction Program* are (Stadtbauamt Wien 1946):

- The human being is at the center of all reconstruction and planning efforts.
- Establish the legal and administrative foundations necessary for rebuilding.
- Regional planning is essential for coordinated reconstruction.
- The city must be de-densified to improve livability and reduce crowding.
- The city must be functionally separated (zoning residential, industrial, commercial areas).
- Develop a deliberate green space policy to improve urban quality of life.
- Create systems for construction consulting to guide efficient building.
- Implement standardization and norms in construction for quality and efficiency.
- Focus on organic reconstruction – not just rebuilding the old, but creating a better city.
- Expand the physical area of Vienna to accommodate future growth.
- Key considerations from the committee on urban regulation, architecture, and cityscape.
- Key considerations from the committee on engineering and transportation.
- Key considerations from the committee on technical construction and energy.
- Key considerations from the committee on construction economy and financing.

Figure C.7: Commission for Rebuilding of Vienna

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Punkt 1

Der Mensch steht im Mittelpunkt aller Überlegungen und Planungen im Wiederaufbau

Wenn in der Vergangenheit an oberster Stelle die Erfordernisse der Wirtschaft oder einer abstrakten Staatsidee standen, denen sich der einzelne bedingungslos unterzuordnen hatte, so gilt als wichtigste zeitgenössische Forderung der Grundsatz, daß der Mensch selbst, das einzelne Individuum, der Maßstab sein soll, nach welchem alle Planungen und Überlegungen im Wiederaufbau geschehen müssen. Jede Maßnahme, die getroffen wird, muß dahingehend untersucht werden, ob auf sie der menschliche Maßstab anwendbar ist. Die soziale Grundidee ist daher ausschlaggebend, der soziale Staat als Folge die Basis, auf der die größtmögliche Freiheit jedem einzelnen geboten werden kann; die daraus in gleichem Maße erwachsende verpflichtende größere Verantwortung und Rücksichtnahme allen anderen gegenüber zeigt den Grad der relativen Freiheit an, deren Grenzen die Vernunft zieht.

Die geglückte Gestaltung sinnvollen Wiederaufbaues wird von der Einhaltung dieses ersten grundlegenden Punktes weitgehend abhängig sein.

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Punkt 4

Die Stadt muß aufgelockert werden

Die unnatürliche chaotische Zusammenballung großer Menschenmassen auf engem Großstadtraum in den letzten hundert Jahren ist eine der Ursachen der sozialen Spannungen und der sanitären Mißstände im Staate. Die allgemeine Erkenntnis der Fachwelt hat dazu geführt, daß Maßnahmen von baulicher Seite getroffen werden, um diese Überdichte zu verringern. Schon die Bauordnung sieht hiebei für das Einzelobjekt gewisse Vorschriften — wie Beschränkung des Verbauungsprozentsatzes durch Lichteinfallswinkel und ähnliches — vor.

Im weiteren Rahmen, also im Städtebaulichen, müssen in Zukunft ähnliche Vorschriften der **Auflockerung** gelten. Die Stadt muß in Zukunft lockerer verbaut werden; aber nicht nur in ihren neu zu erbauenden Teilen sondern auch in den bereits vorhandenen müssen geringere Wohndichten erreicht werden. Als Richtschnur für die Dichte der Bevölkerung kann gelten: nach dem London County Plan, dem Plan für den Umbau der Weltmetropole nach diesem Krieg, wird die Errichtung von drei Hauptdichtezonen vorgesehen, wobei die Dichte mit Variationen in jeder Zone nach örtlichen oder anderen Gesichtspunkten prinzipiell vom Zentrum aus abnehmen soll, und die Durchschnittsdichten von 200, 136 und 70 Personen pro Morgen betragen sollen. Diese Dichte sollte für alle europäischen Großstädte in Zukunft angestrebt werden. In kleineren Städten und Landgemeinden bestehen ähnliche Werte meist schon, sie werden vielfach sogar unterschritten. Umso besser.

Ein Weg zu dieser für die Großstadt geforderten Auflockerung ist die **Entkernung**. Die unnatürliche Dichte in den Städten ist hauptsächlich durch Verbauung der Hinter

Notes: The excerpts above are pictures taken from original documents in Stadtbauamt Wien (1946).