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Foreign Troops, Local Economies: The Economic Effects of US Withdrawals from Germany*

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Abstract

This paper analyzes the local economic impacts of troop deployments. We exploit variation from the historic large-scale US troop withdrawal from Germany triggered by the end of the Cold War, to estimate the effect on local labor markets and public finances. We use administrative data provided by the US Department of Defense to quantify the size of the troop withdrawal at the municipal level. Using a synthetic difference-in-differences estimator, we find negative effects on local labor markets: for each withdrawn US soldier, the number of local jobs decreases by 0.53. The decrease in economic activity results in a reduction of revenues which municipalities balance by lowering their expenditures, while increasing business and property tax multipliers. In individual-level analyses, we document that workers displaced by the closure of a US military base have persistently lower employment rates. Moreover, their daily wages remain around 9.2 percent lower fifteen years after the layoff. The negative impact on labor outcomes is particularly pronounced for women, older workers, and those employed in regions with more unfavorable initial labor market conditions.

Keywords: military base closure, local labor markets, worker displacement, fiscal multipliers, spatial spillovers, public finance

JEL Classification: J21, J23, J63, H56, H71, H72, R11, R51

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1 Introduction

Germany is a wealthy country, and they have to pay. [...] They make a fortune off the troops. [...] They build cities around our troops.

Donald Trump, 08/05/2020, Fox News Videos

As of December 2025, the United States stations approximately 170,000 active-duty troops in foreign countries around the world (Defense Manpower Data Center, 2025). These troop deployments are driven by geopolitical and military objectives, but their economic consequences are borne locally. When troops withdraw, they remove not only military presence, but also demand for local labor, goods, and services. Such withdrawals can therefore constitute large negative economic shocks for host communities. Yet despite their policy relevance, causal evidence on the local economic effects of troop withdrawals is very scarce.

This is due to various identification challenges. (1) Foreign troop deployments and withdrawals are typically correlated with political conflicts, rendering them endogenous. (2) Moreover, withdrawals are usually endogenous due to thorough economic planning (Nakamura and Steinsson, 2014). (3) In typical withdrawal processes, troops are frequently relocated back and forth between bases, hindering the analysis of dynamics and long-term impacts (Zou, 2018). (4) Finally, secrecy surrounding military data further restricts research to coarse geographic levels.

Our study overcomes these issues. We exploit the historically unique context of rapid and sizeable US troop withdrawals from Germany during the first half of the 1990*s*. The troop reduction was triggered by an abrupt change in the geopolitical climate following the end of the Cold War, and thus caught local policymakers by surprise. During the short period of approximately five years, $200,000$ US soldiers (∼80%) were withdrawn from Germany. These soldiers were concentrated in only four German states and thus represented a substantial share of the local population. The withdrawal decisions were made unilaterally by the US Department of Defense primarily on military grounds. Accordingly, and due to the high pace of the withdrawal, neither US nor German public policy stakeholders influenced the process. We utilize this ideal historical setup to investigate the local economic impact of US troop withdrawals in Germany.

To this end, we combine detailed information on US military deployments with state-of-the-art econometric methods to examine the effect of military troop withdrawals on local labor markets and municipal public finances. We digitize the administrative Department of Defense Base Structure Reports (BSRs), which contain detailed information on troop strengths at the military base level. We hand-collect all geolocations of US bases in Germany to aggregate this information at the municipal level. This enables a clean quantification of the withdrawal. The location choices of US bases by the Department of Defense were primarily based on military considerations to prepare for a potential Soviet invasion. These determining factors were largely orthogonal to economic characteristics reducing selection concerns. We alleviate possibly remaining concerns by employing the synthetic difference-in-differences (SDID) estimator (Arkhangelsky et al., 2021; Clarke et al., 2024), treating the first closure announcement as event time zero to account for anticipation effects. Moreover, we validate identification via placebo treatment year checks.

Conceptually, foreign troop withdrawals affect the hosting regions mainly at two economic margins: the local labor market and the local public finances of municipalities. Troop withdrawals constitute a negative demand shock, as soldiers cease local consumption, military bases stop purchasing goods and services in the local economy and domestic civilians employed by the foreign force are laid off. This demand shock implies negative impacts on the local labor market. German municipalities raise a substantial share of their local revenues by levying a local business tax (LBT). Consequently, the reduced economic activity leads to a decrease in local (tax) revenues.

The empirical analysis is organized around five research questions: *(1) What are the effects on local labor markets?* We find that the troop withdrawal reduced employment in affected municipalities. We draw on the universe of social security records provided by the Institute for Employment Research (IAB) to estimate the effects on municipal employment triggered by the negative demand shock. Detailed information on establishments' industry classification, combined with information on the spatial location of US military bases, allows us to identify US bases in the social security data. We deploy the SDID estimator to estimate the effect on employment aggregated at the municipality level. Municipalities within a 15 km radius around treated jurisdictions are dropped from the donor pool to avoid potential spillover effects that could contaminate our estimates. Our estimation results show that full-time employment in treated municipalities decreased by -7.1%. The overall effect is only to a small extent driven by individuals who were directly employed by the US military: 61% of the reduction in full-time employment is indirect, i.e., it accrues from workers losing their jobs in the local economy. We do not find any statistically significant effect on local workers' wages, which may be due to changes in the composition of the local labor force.

(2) What are the effects on local public finances? We document that the negative effect

on local economic activity translates into reduced municipal revenues and expenditure. For this analysis, we consult bilaterally with each of the relevant German State Statistical Offices to assemble a comprehensive administrative public finance dataset spanning an extended time period.[1] Our estimation results document that municipal revenues decrease by around −9.0% in treated municipalities. Local governments react by cutting expenditures by −9.3%. The drop in overall revenues can be traced back to several channels. For example, business tax revenues (−10.3%), as well as intergovernmental transfers (−4.8%) decrease. Additionally, we exploit a particularly interesting feature of the German setting: municipalities have discretionary tax-setting power for property and business taxes. This allows us to study tax-setting responses on the fine-grained municipal level. We find a significant 4.4% increase in the property tax B multiplier on private property. In line with findings in the related literature (Helm and Stuhler, 2024; Thunecke et al., 2026), the tax multiplier on the more elastic business tax base does not react as strongly to the shock (2.6%).

(3) What are the long-run consequences? Our findings indicate that the adverse effects on local labor markets and municipal public finances are persistent. We utilize a convenient feature of our historic setting: once US military troops withdrew from a municipality, they did not return. This allows us not only to estimate the dynamic effect of troop withdrawal on local outcomes but also the long-run consequences. The average treatment effect after ten years is −4.8% for full-time workers. We show that total revenues remain at −6.6% while total expenditures level at −10.1%, indicating consolidation efforts of these municipalities. Similarly, the hikes in local tax multipliers persist in the long run. Regarding dynamics, the effects take approximately five years to fully materialize after the announcement of a withdrawal.

(4) How do the effects spill over spatially? We explore potential spatial spillover effects on the nearby local labor market and municipal public finances. For this analysis, we modify our estimation sample by dropping directly treated municipalities from the sample, while recoding jurisdictions in their close surroundings as treated units. We find that the negative effects on the labor market are highly localized. We do not find a statistically significant negative impact on full-time employment in direct neighbors of treated municipalities. The results even tentatively hint at the opposite: some of the workers losing their jobs in directly treated municipalities find work in neighboring municipalities. Focusing on local public finances, our results indicate that municipalities in the areas surrounding treated units are also adversely affected. The differential spillover effects on labor markets

[1] We gather data for Baden-Wuerttemberg, Bavaria, Hesse, and Rhineland-Palatinate for the period 1980-2022 for most variables.

vis-à-vis public finance are consistent with how the outcomes are measured: employment effects are assigned to the municipality of work, whereas public-finance effects are tied to the place of residence. In the short term, public finances are primarily affected in baseline-treated municipalities and among their direct neighbors. In the long run, there is a convergence of effects within a 15 km radius of treated municipalities, leveling off at approximately -3%.

(5) What is the effect on workers directly laid off by the US military? We supplement the municipal-level results with worker-level analyses that explore the impact of base closure-induced displacement on workers' labor market outcomes in the short and long term. We draw on microdata from the IAB to identify workers who were employed at a US military base and were displaced shortly after the base's closure announcement. We follow prior literature on workers' displacement and employ a propensity score matching approach to evaluate the effect of displacement on treated workers' labor market outcomes (e.g. Illing et al., 2024; Bertheau et al., 2023; Schmieder et al., 2023). We find that the employment probability of displaced workers decreases by around 50 percentage points in the year of layoff. Fifteen years after displacement, affected workers are still roughly 20 percentage points less likely to be employed than workers in a matched sample. Similarly, the daily wages of displaced workers are 9.2% lower than those of control workers fifteen years after displacement. We show that, conditional on finding a new job, treated workers' new workplaces are usually located in different municipalities from their previous workplaces. The negative impact on labor market outcomes is especially pronounced for women, older workers, and in regions with initially unfavorable labor market conditions.

To link our findings to the existing literature and to make the results intuitive for policymakers, we translate our reduced form estimates into easy-to-interpret multipliers. Each withdrawn US soldier reduces the number of full-time jobs by 0.53. Per USD of reduced foreign military spending, municipal revenues decrease by $0.28. If viewed as a place-based policy, military spending is ineffective in creating local jobs: we find the cost per created job to be $110,400 of military spending, which is approximately six times higher than for targeted place-based policies in Germany (Siegloch et al., 2025).

Beyond the identification advantages discussed above, our setting offers several additional methodological and empirical benefits. (1) The German federalist landscape allows for detailed spatial analyses of public finance reactions and spillovers due to municipalities' discretionary tax-setting power. (2) Moreover, we deal with the largest withdrawal of troops from a foreign country in times of peace. In 1989 alone, US military spending of $17.48 billion (in 2024 USD) had a direct impact on the German economy (Sharp, 1990). (3) Finally, the fall of the Iron Curtain marks a pivotal historic moment, lending the context inherent relevance.

This paper connects to various strands of literature. First, we contribute to research on economic shocks and local labor market adjustments (Becker et al., 2021; Gathmann et al., 2020; Notowidigdo, 2020; Hakobyan and McLaren, 2016; Autor et al., 2013) as well as public finance reactions (Brunner et al., 2022; Berset et al., 2022; Feler and Senses, 2017; Gadenne, 2017) quantifying the effects from a novel type of shock: a large-scale withdrawal of foreign troops. Our focus on long-run consequences is particularly informative as evidence in this field is scarce (Antolin-Diaz and Surico, 2025; Helm and Stuhler, 2024). We also provide insights on tax responses (Thunecke et al., 2026; Fremerey et al., 2025), contributing to the limited literature on the dynamics of tax-setting responses in municipalities facing a fiscal revenue shock (Helm and Stuhler, 2024). Second, the troop withdrawal can be understood as a special type of place-based policy. By providing key metrics, such as the cost per created job, we directly connect to this strand of literature (e.g. Siegloch et al., 2025; Garin and Rothbaum, 2025; Serrato and Wingender, 2016; Kline and Moretti, 2014; Greenstone et al., 2010). We thereby also contribute fine-grained estimates to the spatially coarse macroeconomic literature on military spending multipliers (Nakamura and Steinsson, 2014), highlighting differences between domestic and foreign military spending (Auerbach and Gorodnichenko, 2012). Third, in addition to analyzing the effect of foreign troop withdrawals on local public finances and labor markets at the municipal level, we also examine the worker-level impacts of US troop withdrawals. Thus, we contribute to the literature on mass layoffs by analyzing long-run individual-level labor market outcomes of displaced workers (e.g., Athey et al., 2026; Bertheau et al., 2023; Schmieder et al., 2023; Gathmann et al., 2020; Schmieder et al., 2010; Couch and Placzek, 2010; Jacobson et al., 1993).[2]

Finally, we contribute to the scarce literature on the economic effects of troop with-

[2]Note that the impact of foreign military base closures on local economies may differ substantially from that of closures of large domestic firms, often studied in the mass-layoff literature (e.g., Gathmann et al., 2020). Foreign military bases are distinct from other firms in several important ways. First, foreign military bases do not produce products or services for sale to other companies or private households. Instead, they are substantial local consumers, demanding local labor, as well as numerous goods and services, directly in the local economy and indirectly through the demand of soldiers and their families living on the base. Second, local policymakers can influence the location choice and retention of an industrial establishment through local subsidies or similar measures. In addition, local economic conditions may directly impact the establishment's economic outcomes. On the other hand, the location choice and, especially, the timing of the closure of a foreign military base cannot be influenced by local policymakers and do not depend on local economic conditions. Third, a large share of the labor force at a foreign military base comprises soldiers. When the military base closes, these soldiers immediately leave the local economy and return to their home country. This is a stark contrast to other local employers, where most employees remain in the local labor market and continue to consume in the local economy when the establishment closes.

drawals. To our knowledge, the earliest paper in this context is Hooker and Knetter (2001), who analyze US domestic troop withdrawals between 1971 and 1994. Zou (2018) analyzes the same setting at the county level between 1990 and 2010. To address endogeneity in the domestic relocation process, the author combines instrumental-variables estimation and matching methods in a static DID setting. Zou (2018) finds that each withdrawn soldier reduces the number of local jobs by 1.2. In line with lower economic integration of foreign troops, our estimate is much smaller at 0.53. Komarek and Wagner (2021) deploy Zou's (2018) identification strategy and investigate the impacts of the same withdrawal process on public finances on the commuting zone level. Similar to our analysis, the authors find a negative effect on aggregate revenues and expenditures. However, driven by the population reduction reported in Zou (2018), real per-capita revenues and expenditures are unaffected. This is different in our setting: consistent with the lower economic integration of foreign troops and other studies analyzing population adjustments in the wake of economic distress (Autor et al., 2013; Dauth et al., 2014), we find no significant effect on municipal population size, immigration, or emigration in treated municipalities, which explains the divergence in the per-capita results in Zou (2018).[3] Our findings highlight that foreign withdrawals can have markedly different impacts than domestic withdrawals. This distinction is especially relevant for countries hosting foreign troops, for which our results may have greater external validity than existing evidence from domestic withdrawals. Adjustment mechanisms central to domestic settings, such as population decline and changes in the local tax base, may be weaker when withdrawing troops are less economically integrated into host municipalities. Distinguishing between domestic and foreign withdrawals is therefore crucial for assessing the local economic effects of foreign military drawdowns and for designing policies that support exposed host regions.

We conduct a rich, dynamic analysis on a spatially fine-grained level, examining the long-term effects to the present day. Our spillover analyses provide detailed evidence on the geography of economic adjustment. Moreover, we supplement the municipal-level labor market results with a worker-level analysis, contributing novel micro-level evidence on the long-run impact of troop withdrawals on displaced workers. To the best of our knowledge, we are the first to quantify the public finance and worker-level impacts of a foreign withdrawal.[4] Finally, we summarize our comprehensive economic analysis in

[3] At the same time, our individual-level results reveal that laid-off workers frequently switch their place of work to other municipalities.

[4] A working paper related to our setting is aus dem Moore and Spitz-Oener (2012), who investigate county-level labor market outcomes of the 1990 US troop withdrawal from Germany on the district level. Other related studies do not yet have a working paper, are case studies, focus on different outcomes, have

easy-to-interpret metrics to provide intuitive results for policymakers.

Foreign troop deployments have increasingly entered the public debate following Donald Trump's first presidency, the Russian invasion of Ukraine, and the China-Taiwan conflict. To make informed policy decisions, it is crucial to understand the numerous economic consequences for both the hosting and the sending nations. We contribute detailed evidence on the economic margin. However, we stress that we do not address the various associated political benefits, such as serving as international security guarantees, providing strategic bases for joint military operations, and generally reinforcing international relations. Moreover, we do not address potential societal (e.g. Schindler and Westcott, 2021) or political impacts (Krumins and Zhukov, 2026) in the hosting regions. From a policy perspective, considering these dimensions is essential for making informed decisions about optimal deployment and hosting.

The remainder of the paper is structured as follows. Section 2 introduces the historical background, discusses the German federal tax-setting structure, and clarifies effect channels. Section 3 describes our data sources. Section 4 gives an overview of the empirical strategy. The results are presented in Section 5. Implications of our findings are discussed in Section 6. Section 7 concludes.

2 Background

2.1 Historical Context

After the end of World War II, Germany was divided into four occupation zones administered by France, Great Britain, the Soviet Union, and the United States. Although Germany regained conditional sovereignty under the Occupation Statute of 1949, the Allies retained the right to station military forces on German soil, with full sovereignty only restored following reunification in 1991. The legal foundation of foreign troop deployments is defined in the NATO 'Agreement between the Parties to the North Atlantic Treaty regarding the Status of their Forces' (short: troop statute, North Atlantic Treaty Organization, 1951) as well as the 'Supplementary Agreement to the NATO Status of Forces Agreement regarding foreign troops stationed in the Federal Republic of Germany' (short: SOFA, North Atlantic Treaty Organization, 1959).

The American occupation zone stretched over southwest Germany, including the later

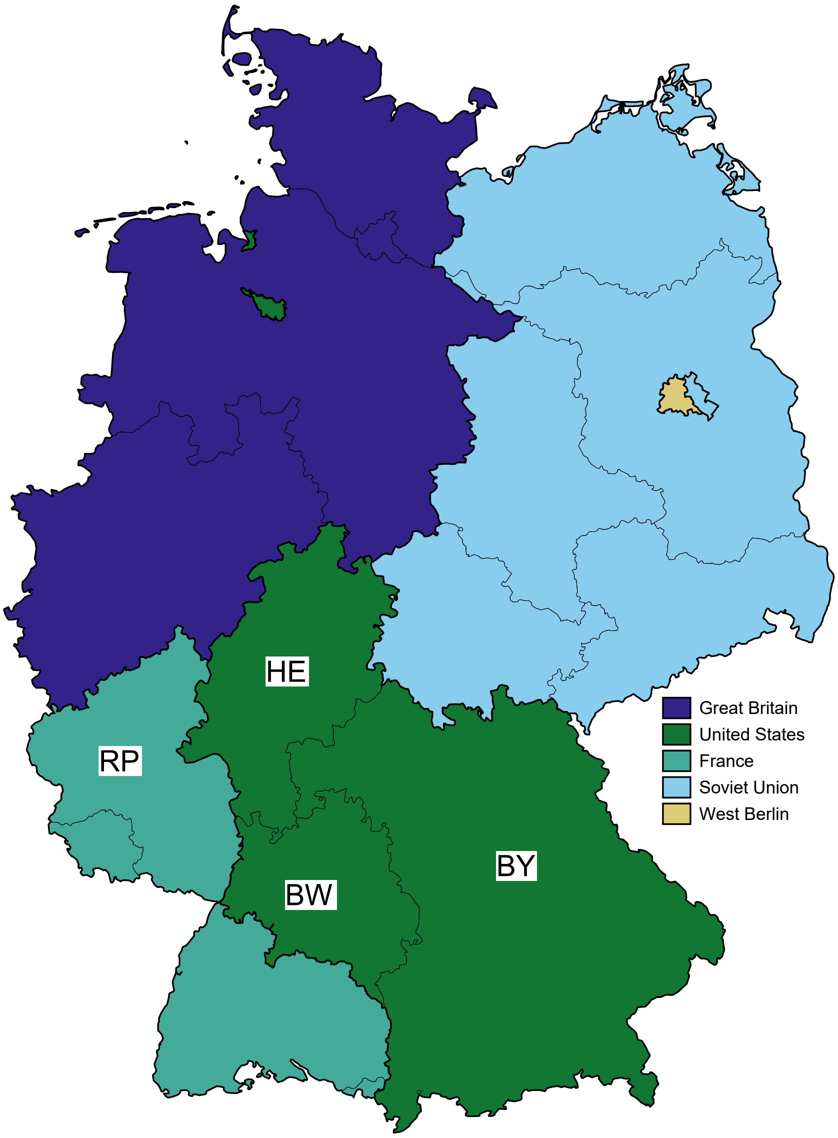
much smaller temporal scopes, and/or do not address the endogeneity issues discussed above (François and Rabaté, 2026; Dahlberg et al., 2024; Calia et al., 2021; Nickelsburg, 2020; Paloyo et al., 2010; Andersson et al., 2007).

founded states of Bavaria and Hesse, as well as the northern part of Baden-Wuerttemberg. In addition, parts of West-Berlin and Bremerhaven were under American control. Figure 1 shows a map of the occupation zones together with German state borders. Initially, the US forces did not plan to stay permanently. However, the rising political tensions with the Soviet Union during the 1940s prompted the US to rethink its strategy. Subsequently, the US military transformed from a force of occupation to one of protection (Cunningham and Klemmer, 1995) and substantially increased its troop presence in Germany. This development took place from the end of the 1940s until the mid-1950s. Figure 2 shows how, during this time, the number of US military personnel increased in Germany, as well as in other European countries. It also displays that the high level of approximately 250,000 US soldiers in Germany was maintained throughout the whole period of the Cold War, with only temporary reductions due to the Vietnam War. Different from the situation in Germany, the initially high US troop levels in other European countries quickly declined between 1955 and 1970. The figure also shows that the US troop presence in Germany was exceptional in magnitude. From 1970 onwards, Germany hosted between four to five times more US troops than all other European countries combined.

Although the occupation zones were clearly demarcated, the stationing of troops was not restricted to the area administered by each nation. Instead, article 12 of the 'Allied Declaration Regarding the Defeat of Germany' maintains the right to station forces in all parts of Germany (United States Department of State, 2023). Figure 3 shows the distribution of US installations in 1990. The dots represent installations with any personnel in 1990. In total, there were 345 installations with non-zero personnel numbers. US installations were concentrated in the US occupation zone and the northern part of the French occupation zone. This location of US bases in Germany was not random. The base locations were chosen to form a defense belt against a potential Soviet invasion and were thus largely orthogonal to economic conditions. Accordingly, four German states were most prominently affected by the US stationing: Bavaria, Baden-Wuerttemberg, Hesse, and Rhineland-Palatinate. We focus on these four states since around 94% of the total number of personnel and almost 90% of US installations were located within their boundaries in 1990. The municipality-level analysis covers three of the four states as municipal fiscal data for Rhineland-Palatinate is not available (see Section 4.1).

Other nations also stationed troops in Germany after the end of WWII. Figure A.1 shows the location of all military installations between 1945 and today by nation, excluding Soviet bases, which were solely located in the territory of the former GDR. The figure highlights that in the southern part of Germany, France, the US, and Germany were almost exclusively the only nations maintaining installations.

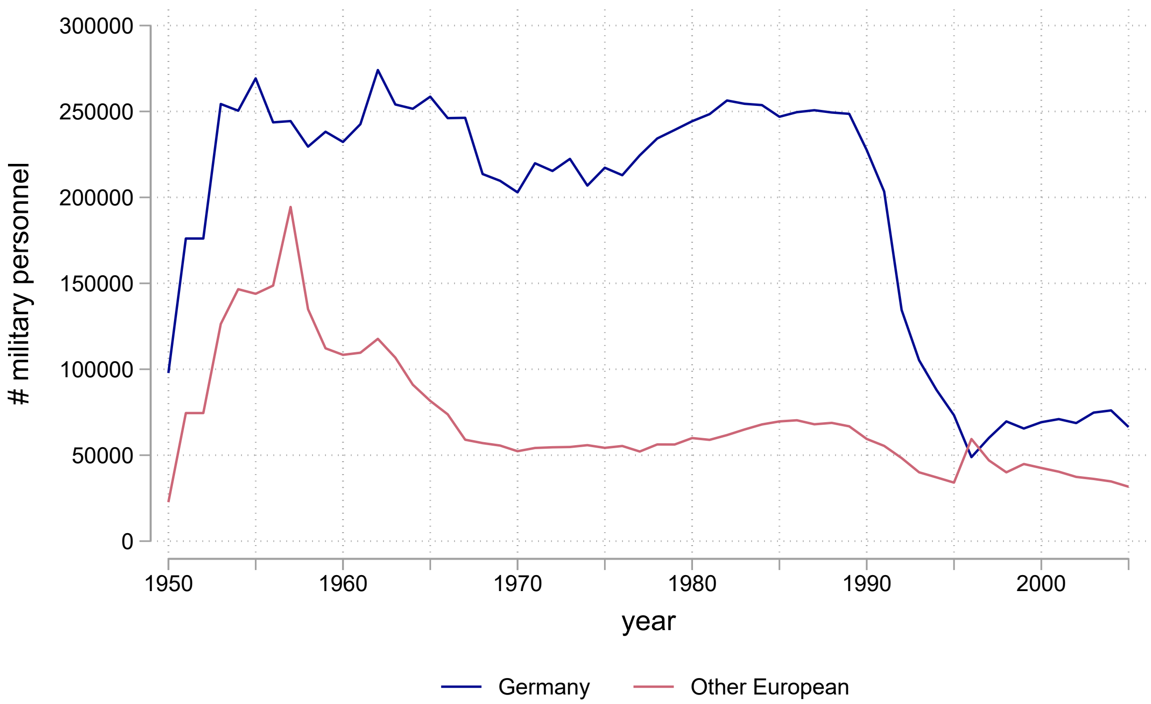
Figure 1: Occupation Zones



Notes: The figure shows the post-WWII German occupation zones together with German state borders. The US zone covers Bavaria (BY), the northern part of Baden-Wuerttemberg (BW) and Hesse (HE). The French occupation zone covers the southern part of Baden-Wuerttemberg, Rhineland-Palatinate (RP) and Saarland. US troops were mainly concentrated in the US occupation zone and the northern part of the French occupation zone, that is, in Bavaria, northern Baden-Wuerttemberg, Hesse and Rhineland-Palatinate.

The conclusion of the Cold War marked a pivotal moment for US military presence in Germany. The sudden geopolitical shift sparked negotiations on reductions in conventional armed forces in Europe between NATO countries and members of the Warsaw Pact. This process gained momentum following the fall of the Berlin Wall on November 9, 1989. The ongoing political transformation in Eastern European states accelerated the

Figure 2: US Military Personnel in Europe



Notes: The figure shows the number of active duty military personnel in Germany and other European countries from 1950 to 2005.

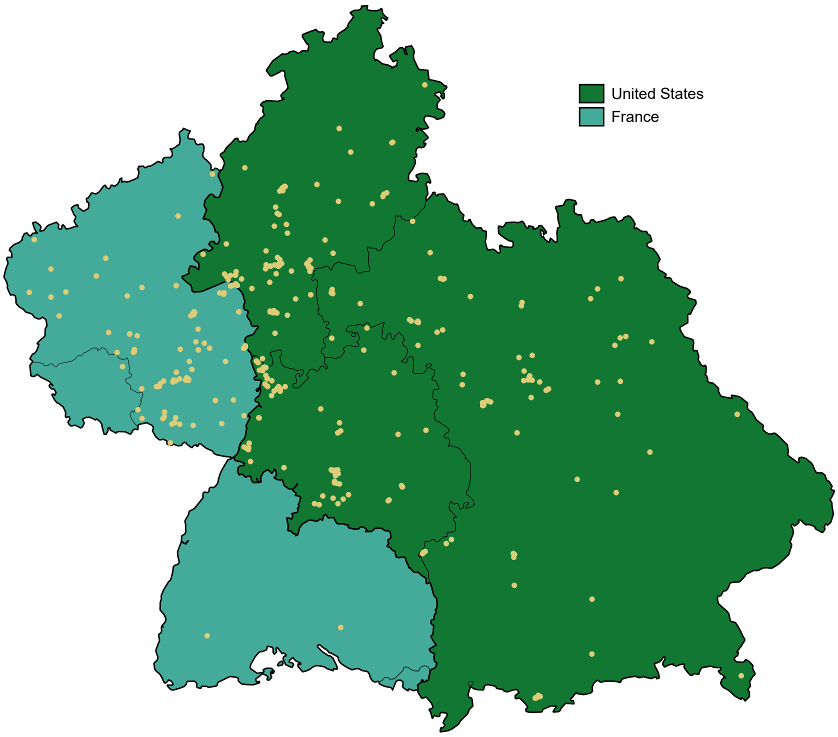
process, culminating in the signing of the Conventional Armed Forces in Europe (CFE) treaty in November 1990, just a month after the formal reunification of Germany.

Using troop ceilings established in the CFE negotiations, USAREUR devised a plan to reconfigure force levels, identify units for withdrawal, and determine bases and communities for closure. The selection criteria for closure sites were shaped by military considerations rather than local economic conditions.[5] This translated into a simplified withdrawal philosophy: " '[G]et out of the worst installations' and 'retain the best quality-of-life installations' " (p.25 Cunningham and Klemmer, 1995).[6]

[5]According to United States General Accounting Office (1991b), the key criteria included ensuring military and operational requirements, decreasing support costs, increasing operational efficiency, minimizing personnel moves, reducing environmental impact, and factors such as proximity to training areas, housing quality, facilities, local political and military environment, concerns of host nations, and proximity to road and rail networks.

[6]For thorough information, details of the planning and execution of the US drawdown process are available in several official US government reports, including United States General Accounting Office (1991a,b) and a comprehensive study by Cunningham and Klemmer (1995) published by the Bonn International Centre for Conversion (BICC).

Figure 3: Spatial Distribution of US Installations



Notes: The figure shows the location of US installations with any personnel in 1990.

The Pentagon initiated the first round of the drawdown by publicly announcing the closure of approximately 100 sites in Germany during September 1990. The majority of subsequent rounds of base closures were successively announced and carried out until the mid-1990s. They resulted in a substantial reduction in US military personnel of approximately 80% or 200,000 soldiers compared to 1989 levels (see Figure 2). The reduction in troop size was accompanied by a (non-quantifiable) large-scale out-migration among the 227,000 family dependents (Cunningham and Klemmer, 1995). The Department of Defense did not communicate their withdrawal decisions with (local) German authorities. This meant that local German politicians and communities were taken by surprise when they learned about imminent closures through US forces' public announcements. Moreover, due to the high pace of withdrawals, German public policy stakeholders could not affect the American withdrawal decisions in any meaningful way (Cunningham and Klemmer, 1995).

Three characteristics of the historic drawdown process are key to our study. First, the geopolitical shift was abrupt and unforeseeable. Second, the pace of withdrawals was high, leaving no room for public policy stakeholders to intervene. Third, the overall size of the withdrawal was large, enabling a clean identification of effects.

2.2 Institutional Context

The four states under investigation consist of around 5,900 municipalities, which are, on average, small both in population and geographic size. These municipalities have considerable autonomy in setting their own tax rates through so-called tax multipliers (*Hebesätze*), which effectively determine the local business and property tax rates. The business tax is levied on the profits of local businesses.[7] Property tax rates are applied to a property's so-called basic value (*Einheitswert*). The basic values were last assessed in the 1960s and have not changed over the period analyzed in this project. Accordingly, unlike business taxes, property taxes are characterized by a stable tax base.

Municipal revenues mainly stem from three sources:[8] (1) Intergovernmental transfers make up about 34% of total municipal revenues.[9] Intergovernmental transfers are split into two categories. On the one hand, *Schlüsselzuweisungen* are rule-based transfers designed to balance municipalities' fiscal capacity and needs. The fiscal capacity (*Steuerkraftmesszahl*) of a municipality depends on its potential to raise tax revenues.[10] The fiscal need (*Bedarfsmesszahl*) of a municipality is defined by the number of residents in a municipality. On the other hand, there are discretionary transfers called *Bedarfszuweisungen*, allocated to municipalities facing an extraordinary financial burden. (2) Municipalities directly raise revenues from local taxes–primarily the business and property taxes–which account for about 21.7% of total revenues. (3) Municipalities receive a share of federal taxes. Most notably, 15.8% of income tax revenue is allocated to a municipality based on the taxable incomes earned by its residents.

The subsequent analyses focus on the three revenue sources mentioned above. Taken together, they constitute the substantial share of 71.5% of total municipal revenues.

[7] Prior to 1998, the local business tax was also levied on a firm's business capital (*Gewerbekapital*), in addition to its profits (*Gewerbeertrag*). The capital component (*Gewerbekapitalsteuer*) played a secondary role in municipal tax revenues, contributing a relatively small share compared to the profit-based component.

[8] Table B.2 contains a detailed breakdown.

[9] This highlights the large role of fiscal federalism in Germany. While local governments (state level and lower) in Germany constituted 39.93% of total government expenditure in 2019, this figure was only 27.67% in Italy, 22.76% in the United Kingdom, and 19.83% in France (OECD, 2023).

[10] The *Steuerkraftmesszahl* is a standardized measure of municipal tax capacity in Germany. It is calculated from the main local tax bases, including business tax, property taxes, and the municipality's shares of income and value-added tax, using standardized multipliers rather than the municipality's actual tax multipliers. It therefore captures the revenue potential of a municipality.

2.3 Effect Channels

Through which channels did the US troop withdrawal affect German municipalities economically? To determine the mechanisms for interpreting our empirical results, the following section provides more background information about the legal status of US forces stationed in Germany.

The troop statute (North Atlantic Treaty Organization, 1951) and SOFA (North Atlantic Treaty Organization, 1959) specify the legal conditions under which NATO troops could be stationed in Germany. NATO members' troops are granted exemption from German taxation on their economic activities while serving in their capacity as military personnel (troop statute, article X, 1.). However, profitable private economic activities of members of the troops are subject to German taxation (troop statute, article X, 2.). Similarly, personnel with German residency serving on NATO nations' bases are not exempt from taxation (article X, 4.). Moreover, NATO nations' troops are "[...] exempt from German regulations in the field of registration of residence" (SOFA, article 6, 1.) and they are not subject to the social security system (SOFA, article 13, 1.). However, in most German states, a specified share of foreign troops (and their family dependents) enter intergovernmental transfers by increasing the fiscal need of a municipality.[11]

The latter regulations give rise to which public finance channels effects can and cannot emerge. First, official population counts are not directly affected by the withdrawal of US soldiers. Second, municipal tax revenues are not directly affected, since the troops were exempt from all sorts of taxation. This differs from domestic withdrawal processes and underscores the importance of distinguishing between the effects of domestic and foreign troop withdrawals. Third, rule-based transfers likely decrease to a small extent according to the fiscal need adjustment discussed above. However, the overall effect on intergovernmental transfers is ambiguous due to discretionary transfers, which were likely increased to support the struggling municipalities.

Accordingly, the troop withdrawals primarily affect German municipal finances through decreased local economic activity. That is, they mainly represent a local demand shock. US military bases employed a large number of civilians and purchased goods and services from local companies. Official sources state 71,000 West Germans directly employed by the US forces (Sharp, 1990). Moreover, US troops consumed goods and services in the local economy, thereby increasing local labor demand. Research shows that companies contracting with the US military exhibit excess profitability which reinforces this mechanism (McGowan and Vendrzyk, 2002; Wang and Miguel, 2012). Although a large part of

[11] A more detailed discussion can be found in Section 6 and footnote 29.

consumption took place on-base, the share of off-base consumption was substantial (28%; see Sharp, 1990).

The withdrawal could, in principle, also imply a labor supply shock via family dependents working in the local economy. However, this channel is negligible according to the relevant historical sources (Sharp, 1990): while concerns about the adverse demand effect are ubiquitous in reports about the withdrawal, labor supply responses are rarely mentioned (Cunningham and Klemmer, 1995). This is due to family dependents' common inability to speak German as well as conservative gender roles which usually disabled female labor market participation. The argument for a strong net labor demand shock is further corroborated by the large amount of US military spending flowing into the German economy. According to Sharp (1990), the US military spent 17.48 billion (2024) USD on the German (local) economy in 1989 alone.[12]

According to the reduced labor demand, we suspect the number of employed to go down in treated municipalities. The declining profits of businesses result in reduced local business tax revenues. The municipal share of income tax depends on the incomes earned by municipal residents and should go down in response to the shock as well. As long as property remains in private hands, property tax revenues will, ceteris paribus, remain flat due to the stable tax base. In theory, deteriorating economic conditions should reduce local population levels. Yet empirical studies on population responses to economic distress generally find little to no adjustment along this margin (Autor et al., 2013; Dauth et al., 2014). This is especially plausible in Germany, where high population density and short commuting distances to nearby cities lower the need for households to relocate. Accordingly, we suspect no detectable effects on population.

The effect on intergovernmental transfers is ambiguous due to a withdrawal's counteracting effects on fiscal capacity and fiscal need, but more importantly due to the discretionary nature of *Bedarfszuweisungen*. However, sources discussing mechanisms of the fiscal equalization scheme predict that intergovernmental transfers will go down in the wake of withdrawals (Landtag Rheinland-Pfalz, 1996).[13] Therefore, as the three most important revenue categories are expected to be negatively affected by the withdrawal, total revenues should fall.

From a welfare perspective, it would be interesting to analyze the effects on the housing

[12]The breakdown of this number in Table B.1 shows on which part of the German (local) economy the money was spent.

[13]The main reason for this is that although US troops and family dependents were officially exempt from German registration rules, most German states incorporated rules to attribute a share of this population to a municipality's fiscal need. Thus, withdrawals of US troops are likely to reduce the fiscal need in treated municipalities more than in proportion to their fiscal capacity (Thöne et al., 2015; Broer, 2001). See footnote 29 for more detailed explanations.

market as well. However, data to investigate responses on this margin is not available for the analyzed period.

Municipalities exposed to the withdrawal shock may react along several margins. First, to restore fiscal balance after the negative revenue shock, they are likely to reduce expenditures. Second, they can increase revenues by adjusting municipal tax multipliers. Raising the local business tax multiplier, however, would impose an additional burden on firms that are already negatively affected by the withdrawal shock. By contrast, the property tax base is comparatively inelastic, making the property tax multiplier a more stable instrument for revenue generation in times of economic distress. In line with the findings in Helm and Stuhler (2024) and Thunecke et al. (2026), we therefore expect the property tax B multiplier to increase more strongly than the business tax multiplier in response to the shock.

3 Data

We gather data on three broad categories. These are micro data on individual US installations used to quantify the US troop withdrawal at the municipal level (Section 3.1), labor market data (Section 3.2) and municipal public finance data (Section 3.3). We supplement the municipal level data with a worker-level dataset of directly affected workers (Section 3.4). The following sections describe these data sources. Other data used throughout the study are described in Section 3.5.

3.1 Quantification of Troop Withdrawals

To quantify the troop withdrawals, we mainly rely on two administrative data sources. (1) The Base Structure Reports (BSRs) and (2) the Historic Closure Report (HCR) both published by the US Department of Defense. The BSRs contain the name of each military installation, the number of military personnel, the number of civilian personnel, and the total number of personnel (sum of both). We use the installation name contained in the BSRs to hand-collect the geolocation of every installation with a positive total personnel count in any year. This enables us to aggregate troop strengths at the municipal level. The BSRs are available for selected years between 1981-2017, of which the years around the first announcement year of troop withdrawals in Germany (1990, 1991, 1998, 2000) are most important for our purposes. The BSR containing information for the year 1990 lists 783 US installations in Germany, of which 345 have any personnel assigned (total number of personnel greater than zero). BSRs for the period 1992-1997 are not available (see subsection C.1 for more information). To fill this gap, we use the HCR, which contains the exact closure date of 590 installations. This enables inferring for almost all bases with any personnel in 1990, at which date the personnel numbers must have been zero.[14]

Our empirical strategy exploits the staggered nature of the withdrawal process. We therefore compile base-closure announcement dates from the *Stars and Stripes* newspaper archive, a Department of Defense publication targeted at US soldiers stationed abroad. We scrape all volumes from 1988–1998, flag issues containing keywords related to US bases in Germany, and then manually review each pre-selected PDF to identify and record base-closure announcement dates where applicable. Because some municipalities host multiple installations and may therefore have conflicting announcement dates, we conservatively assign the earliest year reported in *Stars and Stripes*. We use these announcement dates as the treatment year in our specifications, which credibly rules out anticipation concerns while keeping treatment timing close to the actual withdrawal.[15]

Finally, later sections discuss why deployments of German and other nations' troops in Germany matter for identification. Quantifying troop strengths for countries other than the US is not feasible due to data limitations. To nonetheless rule out confounding effects from other military withdrawals, we hand-collect the geolocations of nearly all military installations in the western German states. Since 1945, in addition to the US, Germany, Belgium, Canada, Denmark, France, Luxembourg, the Netherlands, Norway, and the United Kingdom have operated at least one installation in Germany (see Figure A.1). Because installations hosted by these countries and present in 1989/90 may have closed during the same period as US bases, we conduct a robustness check excluding municipalities that hosted non-US installations in 1989/90 (see Section 5.3).

3.2 Labor Market Data

To measure labor market outcomes, we have access to the universe of IAB labor market records aggregated at the municipal level. The BHP contains all establishments with at least one employee liable to social security. The dataset provides establishment-level information on location, sector, and the number of employees liable to social security, as well as marginal part-time employees. Additionally, the dataset contains information on the average daily wages paid within an establishment. The data is provided by the German Institute for Employment Research (IAB) and has been available annually since

[14]We identify the closure dates of the very few remaining cases via web search. The list of other sources used to identify the remaining base closures can be obtained by the authors upon request.

[15]Using the closure date as treatment time would not be useful as some withdrawals start years before the final closure date.

1975 to the present. Employment numbers correspond to employment on June 30th of a given year. In our estimations, we include the period from 1975 to 2020. The labor market data is available for all four states under analysis.

For calculating labor market spillovers, it is necessary to know which part of the total effect on employment is a direct layoff effect and which part is a second-round/indirect effect (see Gathmann et al., 2020). The IAB data contains information on the number of workers employed by stationed foreign forces (*Stationierungsstreitkräfte*). Using this information, we can identify the loss of workers directly employed by US forces.

3.3 Public Finance Data

Regarding public finance outcome variables, we collect aggregate municipal revenues and expenditures, revenues from intergovernmental transfers, business tax revenues, the municipal share of income taxes, as well as tax multipliers (*Hebesätze*) for property and business taxes. Moreover, we collect the municipal population size, as well as numbers for immigration and emigration. This data is assembled from various sources. For the period 1992-2006, we retrieve the *Rechnungsergebnisse der Gemeinden/Gemeindeverbände* from the State Statistical Offices (*Statistische Ämter des Bundes und der Länder*, FDZ (2022)). They contain detailed information on municipal revenues and expenditures, which are split into a multitude of subcategories. For the period 2006-2020, as well as earlier years up until 1980, we consulted each State Statistical Office separately and digitized the retrieved data whenever necessary. Similarly, we gathered the local tax multipliers for local business tax (LBT, *Gewerbesteuerhebesatz*) as well as property taxes (*Grundsteuer*) bilaterally from each State Statistical Office. The resulting time period for the public finance variables mostly spans from 1983 to 2022. The data of three states (Bavaria, Baden-Wuerttemberg, Hesse) is fully included in our analysis.[16]

3.4 Worker-Level Data

For our worker-level analysis, we draw on the administrative Integrated Labor Market Biographies (IEB) provided by the IAB.[17] The dataset is based on mandatory notifications to social security insurance. The dataset encompasses the entire population of individuals in the German labor market who were either employed in a job subject to social security

[16] We are thankful to Helm and Stuhler (2024) who early on kindly shared their data with us.

[17] For the worker-level data, we use an extract from the IEB database provided by IAB. Access to this database is regulated by Section 75 of the German Social Code (Book X). Oberschachtsiek et al. (2009) provide an exhaustive introduction to this dataset. We follow the guidelines of Dauth et al. (2021) and Stüber et al. (2023) for general data preparation and cleaning routines.

contributions or received unemployment benefits. We restrict our sample to individuals who have at least one employment spell liable to social security contributions between the years 1989 and 2002 in one of the four states that hosted US troops. The constructed dataset provides us with information on the industry, size, and location of a worker's workplace, as well as individual characteristics such as gender, age, nationality, occupation, education, and daily wage. We measure employment status and daily wages on June 30th of a given year.

3.5 Other Data

To calculate easy-to-interpret multipliers in Section 5.6, we draw on US military spending data published by the US Department of Defense (Department of Defense, 1990, 1998), which reports aggregate expenditures in Germany. The difference between the two years is used as a proxy for the reduction in military spending during the relevant period of the withdrawal.[18] As the data are not disaggregated below the federal level, we allocate total spending across German municipalities based on their relative troop strengths. While this allocation is necessarily coarse, it is appropriate given the aggregate nature of the multipliers we estimate.

4 Empirical Strategy

Throughout this section, we first describe the treatment group and donor pool definition in Section 4.1. The descriptive statistics provided in this section highlight why a naïve estimation approach is not suitable for identifying causal effects in our context. We then describe how SDID overcomes these issues and the exact estimation specifications in Section 4.2. In that section, potential identification concerns in our setting are discussed in more detail.

4.1 Sample Definitions

Since local labor markets are integrated, the effects of the troop withdrawal are likely to spill over into nearby municipalities. In later sections, we explicitly focus on these spatial spillovers. In the baseline specification, we rule them out by defining the treatment

[18] Ideally, we would compare 2000 and 1990 to be fully in line with the definitions used throughout the paper, but the series "Atlas/Data Abstract for the United States and Selected Areas" was discontinued after FY 1997, and no comparable source exists. However, if at all, military spending rather decreased further from 1997 to 2000. Accordingly, our choice is conservative in terms of biasing our estimates.

group and donor pool properly. This definition is visually depicted in Figure 4. First, to ensure a clean identification of effects, we focus on municipalities for which the US troop withdrawal was relatively large.[19] Thus, we define municipalities as treated for which the reduction in total US personnel between 1990 and 2000 was larger than 1% relative to the 1989 municipal population (blue). All municipalities with a weaker withdrawal are not considered treated and are instead dropped from the sample (dark gray). We show that our results are highly robust to including all units with any withdrawal in a robustness check (see Section 5.3). We include all non-treated municipalities within the four states under analysis in the donor pool, with one restriction: since we expect spillover effects from the withdrawals to occur across municipal borders, we exclude all municipalities within a 15 km radius around the treated municipalities (light gray) following the theoretical arguments in Butts (2023).[20] The resulting donor pool is highlighted in light purple.

We set treatment time to the year in which a withdrawal of US troops was first announced in a municipality. Municipalities with later announcements might have reacted precautiously when the first withdrawals were announced elsewhere. In a robustness check, we thus show that setting the first global announcement year, 1990, as the event time for all municipalities yields similar results with the expected delayed impact.

As large cities may not generally have a comparable counterpart in the donor pool and ATEs would likely not be valid when translating our results into easy-to-interpret aggregate multipliers, we further restrict our sample to municipalities with a total population of less than 300,000 during the observation period.[21] We show in the robustness checks that this restriction does not drive our results.

What are the characteristics of the treatment group? Panel A of Table 1 summarizes some key statistics. It is noteworthy that among the 62 treated municipalities 83% of total personnel is withdrawn. Moreover, 71% of treated municipalities are subject to a full withdrawal of US troops, implying that no personnel remain in these municipalities. After the announcement of a troop withdrawal, it takes on average 2.03 years until the last military installation within the municipality is closed. This highlights that the impacts of a withdrawal should take a couple of years to materialize after an announcement.

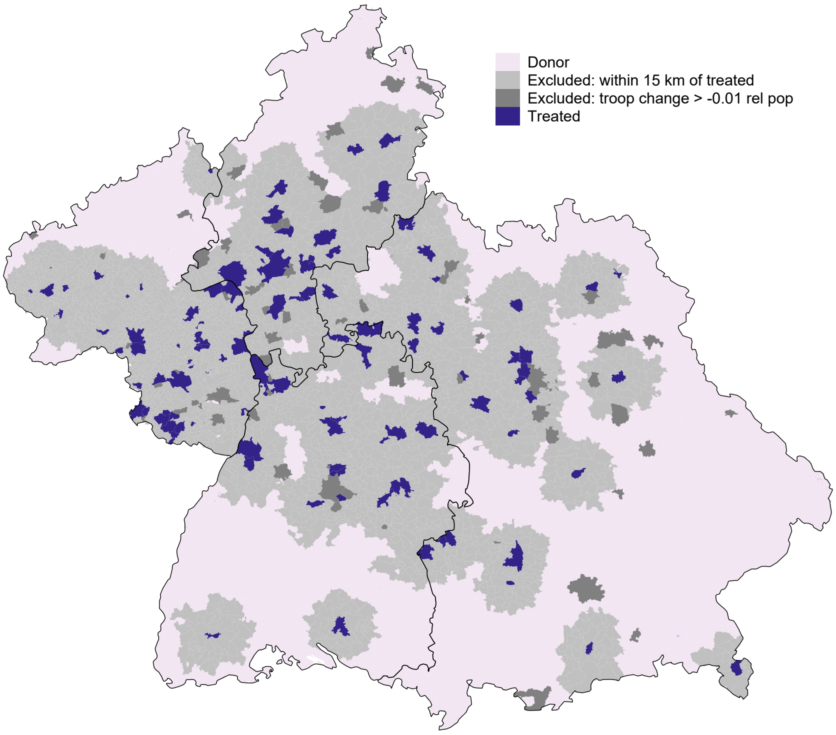
Panel B compares the main outcome variables between treated municipalities and the

[19]Including large municipalities like Munich with negligible withdrawals would blur our estimates by mainly capturing noise.

[20]Municipal fiscal data is unavailable for Rhineland-Palatinate. The municipality-level analysis therefore covers Hesse, Baden-Württemberg, and Bavaria. The individual-level analysis in Section 5.7 includes Rhineland-Palatinate.

[21]We exclude these municipalities as they are exceptional in size and do not have a counterpart in the donor pool. This restriction excludes the six following municipalities: Frankfurt am Main, Karlsruhe, Mannheim, Munich, Nuremberg, and Stuttgart.

Figure 4: Treatment and Donor Pool Definition



Notes: The figure shows the treatment group and the donor pool as well as municipalities excluded from the analysis. Municipalities for which the reduction in total US personnel between 1990 and 2000 was larger than 1% relative to the 1989 municipal population are defined as treated (blue). Municipalities with weaker withdrawals are dropped from the sample (dark gray). Furthermore, municipalities within a 15 km range of treated municipalities are excluded (light gray). The resulting donor pool is highlighted in light purple.

donor pool. Treated municipalities are, on average, larger than those in the donor pool. These differences are sizeable and almost consistently highly statistically significant. This difference motivates our choice of the SDID estimator, which selects a useful comparison group from the donor pool.

4.2 Specification

As discussed previously, the average treated municipality differs from the average donor pool municipality. Moreover, a naïve dynamic difference-in-differences estimation violates the parallel trends assumption for most outcome variables in our setting. We thus select a comparable control group via the synthetic difference-in-differences (SDID) estimator proposed by Arkhangelsky et al. (2021), which has been widely adopted in the recent empirical literature (Bijnens et al., 2025; Anukriti et al., 2025; Brockmeyer and Sáenz So-

Table 1: Summary Statistics

Panel A: Treatment Group Characteristics	
Number of treated municipalities	62
Average number of installations per treated municipality	2.53
Average number of total personnel in treated municipalities in 1990	2848
Average reduction in total personnel in treated municipalities until 2000	2015
Average relative reduction in treated municipalities (of 1990 personnel size)	83%
Number of treated municipalities with full withdrawal	44 (71%)
Average date first withdrawal announcement	1991M5
Earliest withdrawal announcement year	1990
Latest withdrawal announcement year	1995
Average years from first withdrawal announcement to last closure	2.03

Panel B: Treatment Group and Donor Pool

	Treatment		Donor	Difference	
Outcome	Mean	SD	Mean	Mean	p
Labor Market Outcomes					
# Full-time Employed	17,705	23,080	1,292	16,414	0.0000
Public Finance Outcomes					
Total Revenues	91,209	128,020	7,928	83,281	0.0000
Total Expenditures	91,745	129,944	7,498	84,246	0.0000
Income Tax Revenues	12,529	16,543	1,232	11,296	0.0000
Business Tax Base	4,767	6,285	376	4,391	0.0000
Intergovernmental Transfers	6,935	12,415	734	6,202	0.0000
Property Tax B Multiplier	275	53	283	-8	0.1577
Business Tax Multiplier	331	36	313	18	0.0000
Other Outcomes					
Population	42,407	53,259	5,046	37,361	0.0000

Notes: The table provides summary statistics for the treatment group and the donor pool. All values are from 1990 if applicable. Panel A lists various withdrawal-related statistics for the treatment group. Panel B compares the treatment group and the donor pool on the outcome variables analyzed throughout this paper. All monetary values are in thousands of Euros deflated to 2022. Column "p" lists the p-value from a two-sided t-test of the means between treatment group and donor pool.

marriba, 2025; Dench et al., 2024; Bhalotra et al., 2023; Lang et al., 2023).[22] Compared

[22]The computational implementation of the SDID estimator in Stata is based on the *sdid* command provided by Clarke et al. (2024). The event study extension *sdid_event* is outlined and implemented in Stata by Ciccia (2024). We follow the notation of Arkhangelsky et al. (2021) while outlining the SDID estimator.

to the classic DID estimator, SDID is particularly suited for settings where the standard parallel trends assumption is violated and has advantageous robustness properties compared to the standard synthetic control estimator (SC). The SDID estimator estimates the average treatment effect of the treated (ATT) by solving the following optimization problem:

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\gamma}\right) = \underset{\tau,\mu,\alpha,\gamma}{arg\ min} \left\{\sum_{i=1}^{N}\sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \gamma_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid}\right\} \tag{1}$$

where Y_{it} denotes the log outcome variable of interest in municipality $i \in \{1, ..., N\}$ in year $t \in \{1, ..., T\}$. $W_{it} \in \{0, 1\}$ denotes the binary treatment indicator. In our specific case, W_{it} equals zero for all untreated municipalities. W_{it} equals zero for treated municipalities before the first base closure in their jurisdiction took place and one afterward.[23] Similar to standard synthetic control (SC) estimators (Abadie et al., 2010), the SDID estimator uses unit weights ω_i^{sdid} to match pre-treatment outcomes of the dependent variable Y_{it} between the untreated municipalities and the treated. Arkhangelsky et al. (2021) allow for an intercept term when computing the SDID unit weights. Thus, the SDID estimator allows for more flexibility by matching the pre-treatment outcomes of the treated and untreated units in terms of trends, compared to the SC estimator of Abadie et al. (2010), which matches the two groups in levels. Besides unit weights, the SDID estimator utilizes time weights λ_t^{sdid} to balance pre- and post-treatment periods for the untreated municipalities. α_i and γ_t capture municipality and year fixed effects, respectively.

In our setting, treatment adoption time is staggered, as the announcement of US base closures happened at different points in time. We therefore employ the algorithm outlined in Clarke et al. (2024), which computes the SDID estimator for each treatment timing group and stacks the results in each relative time period. A detailed decomposition of the estimator can be found in Appendix E.

SDID matches pre-treatment trends for each outcome variable separately and requires only non-missing cells in treatment and donor units. Similar to SC, SDID hinges on a sufficient number of pre-treatment periods for matching. In our setting, the availability of pre-treatment observations differs across outcome variables. Therefore, to maximize the number of pre-treatment periods on the outcome variable level, we create panelized datasets for each outcome before running SDID. To do so, we pick the longest available

[23] SDID imposes two general conditions on the population of treated and untreated units: First, the estimator requires that being treated is an absorbing state, i.e., there are no treated units which switch back to being untreated. Second, units that are always treated cannot be incorporated into the SDID estimator. The second condition is the same for all synthetic control methods, which explicitly impose that there is at least one pre-treatment period for each and every treated unit.

time horizon on the outcome variable level. At least seven pre-periods are available for every outcome variable. For easy comparison of outcomes, we present 5 pre-periods and 10 post-periods in the results section and aggregate the later post-periods into one estimate to gain insight into the long-run effects. When binning, only relative periods for which we observe all treated units are considered. Given our observation window extending through 2022, this long-run estimate captures effects for up to 30 years after a withdrawal event took place. While the robustness of the estimates for the first 10 post-periods is supposedly high, the results for later post-periods may become unstable, particularly when only a few pre-periods are available for matching. The long-run results should thus be interpreted with caution. Inference is based on 500 bootstrap draws. Direct neighbors and municipalities within a 15 km radius are excluded from the donor pool in their respective spillover specifications to avoid bias from spatially contaminated controls (Butts, 2023).

The most salient potential confound in our setting is German reunification in 1989 shortly before the start of the withdrawal period. We address this concern through three complementary pieces of evidence. First, SDID trivially nets out shocks that are common across municipalities within a given year through its time fixed-effect component. Thus, any aggregate reunification shock on the states under analysis is absorbed, and identification relies on differential exposure of treated municipalities relative to a weighted donor pool (in our case, within states). Second, Figure A.2 shows no significant impact of the withdrawal on municipal population size, immigration, or emigration, suggesting that selective migration flows associated with reunification are unlikely to be driving our estimates. Third, we exclude the *Zonenrandgebiet*–the region most directly exposed to reunification-specific fiscal shocks–in a robustness check (column *W/o ZRG* in Table 3), with results remaining unchanged.

5 Results

The discussion of our baseline results mainly relies on figures 5 and 6 as well as columns 1 and 2 of Table 2. All outcome variables are in logs. The figures display dynamic effects for the first ten post-periods, while the long-run effects are binned in one estimate. For comparability between graphs, the y-axis has the same scale across all outcome variables. Column 1 (*Period 0-9*) of the table contains the baseline SDID results from estimating Equation 1 where the first 10 post-periods are binned. As shown in the figures, column 2 of the table bins the 10+ post-periods. The other columns in the table show the results from spillover analyses, which are discussed at a later point. Throughout this section, p-values are presented in square brackets.

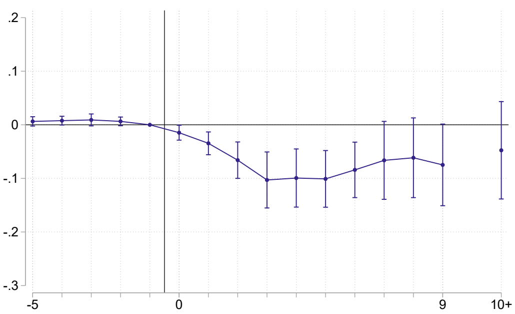
Table 2: Baseline and Spillovers Results

	Baseline		Direct Neighbors	15 km
	0-9	10+	0-9	0-9
Labor Market Outcomes				
# Full-time Employed	-0.0705***	-0.0476	0.0127	0.0020
	(0.0230)	(0.0464)	(0.0125)	(0.0078)
Public Finance Outcomes				
Total Revenues	-0.0903***	-0.0663**	-0.0412***	-0.0206***
	(0.0171)	(0.0266)	(0.0106)	(0.0046)
Total Expenditures	-0.0934***	-0.1006***	-0.0313***	-0.0126*
	(0.0177)	(0.0260)	(0.0075)	(0.0073)
Income Tax Revenues	-0.0311***	-0.0310	-0.0135***	-0.0032
	(0.0099)	(0.0262)	(0.0036)	(0.0029)
Business Tax Base	-0.1029***	-0.2410***	0.0012	-0.0223
	(0.0297)	(0.0701)	(0.0229)	(0.0153)
Intergovernmental Transfers	-0.0484	0.1533**	-0.0502***	-0.0048
	(0.0605)	(0.0599)	(0.0160)	(0.0158)
Property Tax B Multiplier	0.0443***	0.1069***	0.0252***	0.0156***
	(0.0159)	(0.0242)	(0.0053)	(0.0025)
Business Tax Multiplier	0.0262***	0.0311***	0.0119***	0.0057***
	(0.0062)	(0.0092)	(0.0027)	(0.0010)
Other Outcomes				
Population	0.0078	0.0197*	0.0058***	0.0028*
	(0.0059)	(0.0111)	(0.0021)	(0.0015)

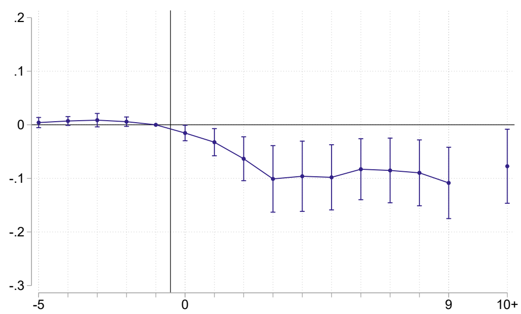
Notes: The table shows the SDID results from estimating equation 1 for various outcome variables. All outcomes are in logs. Inference is based on 500 bootstrap draws.

Figure 5: Number Full-Time Employed

(a) Overall



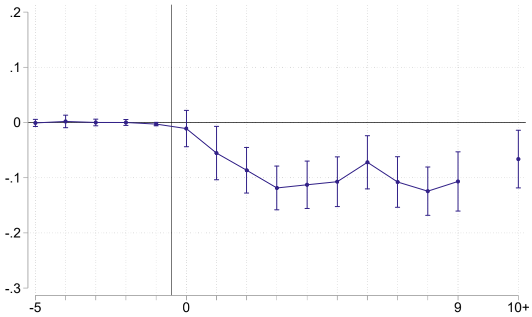
(b) W/o workers at military bases



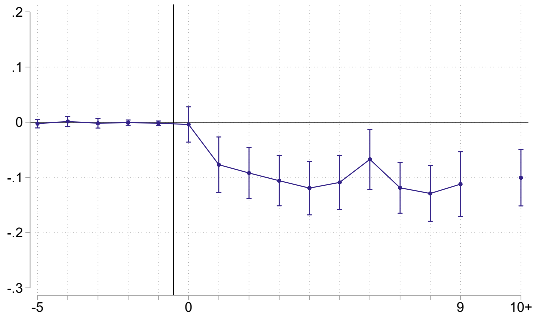
Notes: The figure depicts SDID results from estimating Equation 1 for the number of full-time employed at the municipal level. The left panel uses the overall number of full-time employed as the dependent variable. The right panel uses the overall number of full-time employed, excluding individuals directly employed at foreign military bases. All outcomes are in logs. Inference is based on 500 bootstrap draws.

Figure 6: Baseline Public Finance

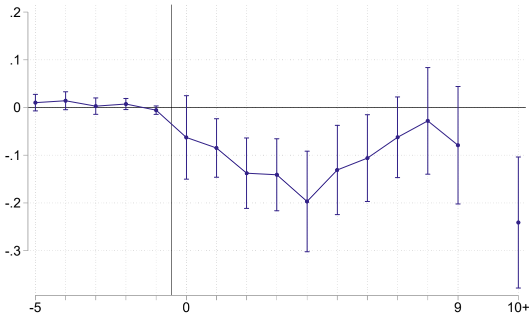
(a) Total Revenues



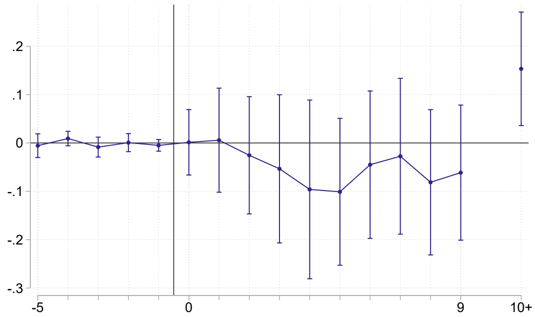
(b) Total Expenditures



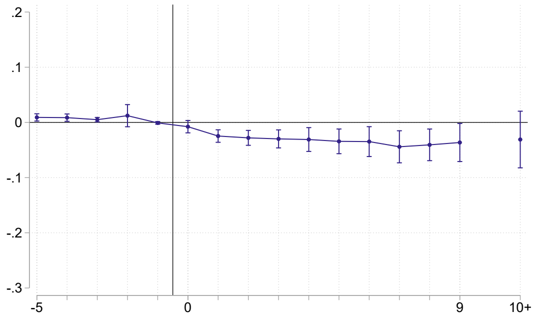
(c) Business Tax Base



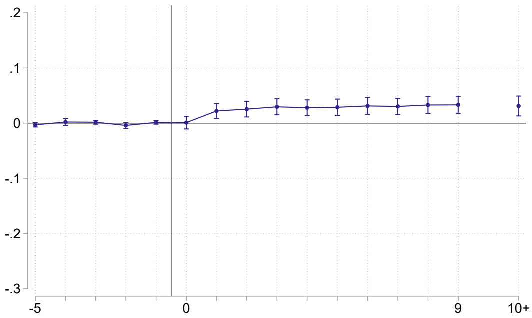
(d) Intergovernmental Transfers



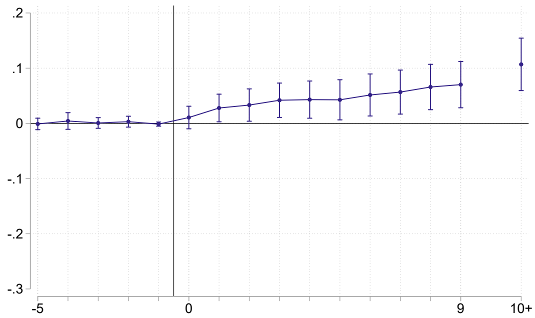
(e) Income Tax Revenues



(f) Business Tax Multiplier



(g) Property Tax B Multiplier



Notes: The figure shows SDID results from estimating Equation 1 for various local public finance variables. All outcomes are in logs.

5.1 Labor Market

Row *# Full-time Employed* shows that the log number of full-time employed in treated municipalities decreased substantially during the first ten post-years (-0.0705 [0.0022]). The dynamics of adjustment are visible in Figure 5a. The number of full-time employed first declines quickly to then slowly return to a less significant but still clearly negative long-run effect (-0.0476 [0.3050]).[24] The initial effect takes approximately four years to fully materialize due to two main reasons. First, as shown in Panel A of Table 1, it takes on average approximately 2.03 years from first announcement until final closure of all installations within a municipality. Second, indirect labor market effects from the direct layoff of German civilians employed by the US military take time to materialize. Both mechanisms delay the onset of effects.

Figure 5b depicts the dynamic effect on full-time employed, excluding workers directly employed at a US military base. Thus, this specification only captures the indirect effect of troop withdrawal on local employment. The indirect effect is very similar to the overall effect, indicating that the layoff of workers directly employed at US military bases does not drive our overall results. The last row of Table B.3 documents that in 1990, for a municipality that hosted US troops, the average share of individuals working directly for the US military base was just above one percent of the overall municipal employment.

5.2 Public Finance

How did the shock on local labor markets translate into public finances? As expected, the negative effect on employment reduces log revenues by -0.0903 [0.0000] in the first ten post-periods. Municipalities reduce their expenditures by an almost identical relative amount (-0.0934 [0.0000]). This movement in tandem of revenues and expenditures is similar to findings in related literature (Helm and Stuhler, 2024). While revenues recover slightly to -0.0663 [0.0129] in the long-run, expenditures remain on their initial low level (-0.1006 [0.0001]). This speaks in favor of consolidation efforts of the treated municipalities.

Each of the three main revenue sources discussed above contributes to the overall negative effect on total revenues. First, the municipal share of income tax revenues is negatively affected. Over the first ten post-years, it decreases by -0.0311 [0.0017]. Second, the business tax base shrinks (-0.1029 [0.0005]). Most notably, this effect is persistent

[24] The long-run estimate aggregates all post-periods beyond year nine into a single coefficient. This estimate demands that the parallel trends assumption holds over an increasingly long horizon. The long-run coefficients should therefore be interpreted as suggestive of persistence rather than as precise causal estimates.

and aggravated in the long run: from the 11^{th} post-period onward, the business tax base exhibits a -0.2410 [0.0006] reduction. This finding is consistent with research showing that contracts with the US military are associated with excess profits for contracting companies (e.g. McGowan and Vendrzyk, 2002; Wang and Miguel, 2012). Consequently, it seems likely that the withdrawal of the US military led to a persistent reduction in these contractors' taxable profits, thereby reducing the local business tax base. Part of these negative effects are offset by a long-run increase in the third main revenue source–intergovernmental transfers (0.1533 [0.0105]). This is likely due to the increased fiscal need from deteriorating economic conditions in the affected municipalities. However, in the short run, intergovernmental transfers also decrease (-0.0484 [0.4237]). This is fully consistent with a short-run mechanical decline in rule-based transfers, since a share of US troops was counted toward a municipality's fiscal need before the withdrawal, as discussed in Section 2.3.

Municipalities can directly affect their revenues via the property and business tax multipliers. Property tax B has a stable tax base, whereas the local business tax is levied on the profits of businesses in a municipality and thus exhibits a more flexible tax base. Our results show that municipalities utilize their discretionary tax-setting power to mitigate the negative fiscal shock. In the first ten years after the withdrawal municipalities raise their property tax multiplier by 0.0443 [0.0053] log points. In contrast to findings in Helm and Stuhler (2024), the tax-setting response is immediate. In line with the authors' discussion about rational inattention, these differences are possibly due to the higher public salience of the US troop withdrawal compared to the more opaque population count shifts in their setting. Notably, it even increases to 0.1069 [0.0000] log points in the long run. Municipalities also raise the business tax multiplier but to a lesser extent (0.0262 [0.0000]) and–different to the property tax B multiplier–the effect on this multiplier does not increase in the long-run (0.0311 [0.0007]). The smaller increase of the business tax multiplier is in line with findings in other literature (Helm and Stuhler, 2024; Thunecke et al., 2026). In economically challenging times, municipalities decide not to overburden struggling businesses and instead use the tax multiplier on the stable property tax base to raise revenues.

5.3 Robustness

We support the robustness of our identification and our estimation strategy by running a multitude of robustness checks. Table 3 lists various specifications. Column *Baseline* replicates the results from our baseline SDID specification in Equation 1 for the first ten post-periods.

First, we want to ensure that the announcement dates we extract from the *Stars and Stripes* newspaper archive represent an adequate treatment year definition. Therefore, we supplement the staggered baseline with a non-staggered specification in which we set the event time to the year in which a withdrawal was announced for the first time (1990–column *T-year 1990*). This ensures we do not pick up anticipation and precautionary effects for municipalities with a withdrawal announcement after 1990 in our baseline. The results remain qualitatively the same and quantitatively very similar. Moreover, Figure A.3a exemplifies for our main outcome variable, total revenues, that, consistent with expectations, the non-staggered specification exhibits a longer lag before effects materialize.

As discussed before, we restrict our treatment group to municipalities in which the size of the withdrawal was larger than 1 percent of 1989 population and in which the population size was below 300,000 across the observation period. In column *No Treat. Restriction* we show that results are very similar when these restrictions on the treatment group are dropped. Reassuringly, the absolute effect sizes on almost all outcome variables decrease as municipalities with smaller relative withdrawal sizes are included.

One concern could be that other nations' military installations were closed during the period of observation. Ex ante, it is unclear whether this would bias our results upward or downward, as such installations are located in both the treatment group and the donor pool. In column *W/o Oth. Nations*, we thus exclude all municipalities with other nations' active military installations during 1989/90. Figure A.1 shows that almost exclusively German and French bases were located in the four states under consideration. When excluding other nations' bases, the results are very similar to the baseline.

Similarly, in column *W/o ZRG*, we exclude all municipalities belonging to the former *Zonenrandgebiet*, as these municipalities lost place-based funding due to being located in an economically disadvantaged area when Germany was reunited (Ehrlich and Seidel, 2018). The *Zonenrandgebiet* is depicted in Figure A.9. The results remain qualitatively identical and quantitatively largely unaltered. Moreover, the similarity of results suggests that reunification-related confounding is unlikely to drive our findings, since municipalities in the *Zonenrandgebiet* were the most exposed to potential reunification-induced effects given their proximity to the former GDR.

To rule out potentially remaining concerns about the troop withdrawal coinciding with reunification, we analyze migration flows as a proxy for differential reunification effects between treated and matched control units. Figure A.2 in the Appendix addresses this directly, showing SDID results for municipal population size, immigration, and emigration.

We find no statistically significant effect on any of the three outcomes. This null result indicates that treated and control municipalities did not experience differential population dynamics following the withdrawal, supporting the interpretation that our labor market and public finance estimates capture the economic consequences of the troop withdrawal rather than concurrent reunification-related impacts.

Even if pre-trends are matched well via SDID, if a state is overrepresented in the donor pool, particular economic shocks in the post-period in that state could drive our results. To ensure that this is not the case, we include another German state–Northrhine-Westphalia–in a robustness check. The results are listed in column *Including NRW*. Reassuringly, the results remain very similar.

A subtler concern arises from the fact that the SDID estimator constructs outcome-variable-specific unit weights $\hat{\omega}_i^{sdid}$: for each dependent variable, a distinct synthetic comparison group is selected by matching pre-treatment trends in that variable separately. While this flexibility is a key strength of SDID, it implies that the effective comparison group differs across outcomes, which could complicate cross-outcome comparisons and, in principle, allow overfitting of donor pool weights to individual variables. Column *Unit Weight Revenues* addresses this directly. We fix the unit weights to those estimated for the total revenues specification – our main outcome variable of interest – and apply them uniformly to all other outcome variables. Reassuringly, the estimates remain quantitatively very similar to the baseline across all outcomes. This demonstrates that our findings are not an artifact of outcome-specific weight selection.

One concern could be that large municipalities drive our results. Therefore, in column *Pop < 200,000* we exclude all municipalities with a population greater than or equal to 200,000 in any of the years under observation. Again, there are no meaningful deviations from the baseline estimates.

Column *Small Withdrawal* treats municipalities experiencing a withdrawal of between 0 and 0.2% of their 1989 population as the treated group. Effects are close to zero and statistically indistinguishable from it across most outcome variables. This confirms that the effects documented in the baseline are driven by shocks of meaningful magnitude rather than being a generic feature of any US military presence.

Finally, we want to ensure that our results are not driven by the chosen SDID estimator. Therefore, we compare it to a dynamic TWFE specification in column *TWFE*. Similar to the reasoning for using SDID, we augment the TWFE specification as otherwise the parallel trends assumption is violated for various outcome variables (see discussion in subsection 4.2). To do so, we include five-year pre-treatment outcome variable growth decile group fixed effects, allowing for comparisons between units with similar pre-treatment trends in the outcome variable. Reassuringly, the SDID and TWFE estimates are very similar.

Overall, the robustness checks strongly support the chosen estimation strategy and the cleanness of identification in our setting.

Time placebos To test whether SDID picks a valid control group, we run placebo in time tests. These checks ensure that the effects we estimate are not an artifact of the matching strategy. That is, we ensure that no systematic trend breaks appear after reassigning event time to an earlier placebo year. Given data availability, we can only test this for some outcomes, as the pre-period would become too short for most other variables. We therefore rely on two main outcome variables. (1) IAB labor market data is available since 1975 for all states under observation. Therefore, we have sufficient pre-periods to run time placebos for the number of full-time employed. (2) For Bavaria, our time series for the business tax base spans the period 1970-2022. Thus, the second time placebo analysis focuses on this variable, restricting the sample to Bavaria. To do so, we set the placebo treatment five years prior to the earliest announcement year, 1990.

Figure 7 shows SDID results for both variables. The red line depicts the placebo specification where the treatment year is set to 1985, while the blue line shows the specification where the treatment year is set to the first announcement year, 1990. Reassuringly, the effect on full-time workers remains close to zero in the placebo post-period and the actual pre-period and exhibits a sharp drop only after the true first announcement year. While the business tax base is overall more erratic due to its inherent volatile nature, and due to restricting our sample to only Bavaria, no systematic trend break is visible in the placebo post-period. Again, the sharp drop at the start of the actual post-period and the similar size of measured effects in the actual post-period across both specifications are reassuring.

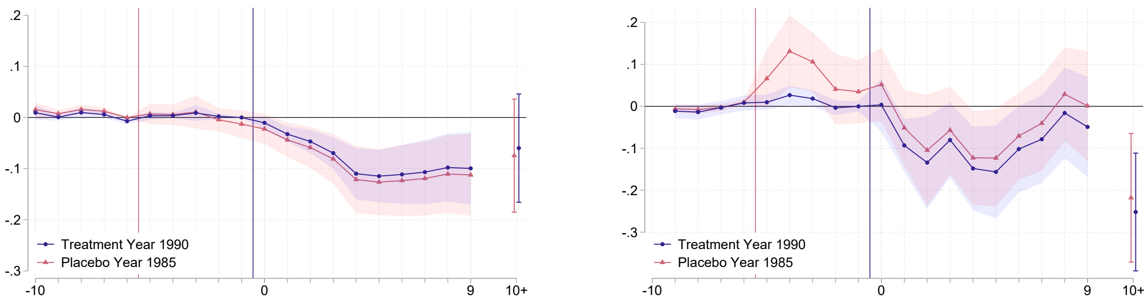
Table 3: Robustness

Outcome (log)	Baseline 0-9	T-year 1990	No Treat. Restriction	W/o Oth. Nations	W/o ZRG	Including NRW	Unit Weight Revenues	Pop < 200,000	Small Withdrawal	TWFE
Labor Market Outcomes										
# Full-time Employed	-0.0705***	-0.0799***	-0.0619***	-0.0886***	-0.0967***	-0.0641***	-0.0974***	-0.0856***	-0.0337	-0.0716***
	(0.0230)	(0.0190)	(0.0171)	(0.0321)	(0.0190)	(0.0249)	(0.0213)	(0.0204)	(0.0525)	(0.0251)
Public Finance Outcomes										
Total Revenues	-0.0903***	-0.0852***	-0.0807***	-0.0893***	-0.0916***	-0.0762***	-0.0852***	-0.0913***	-0.0218	-0.0802***
	(0.0171)	(0.0189)	(0.0133)	(0.0220)	(0.0133)	(0.0127)	(0.0189)	(0.0202)	(0.0355)	(0.0185)
Total Expenditures	-0.0934***	-0.0833***	-0.0788***	-0.0929***	-0.0927***	-0.0828***	-0.0781***	-0.0948***	-0.0100	-0.0896***
	(0.0177)	(0.0183)	(0.0145)	(0.0254)	(0.0149)	(0.0120)	(0.0185)	(0.0196)	(0.0408)	(0.0215)
Income Tax Revenues	-0.0311***	-0.0373***	-0.0295***	-0.0337***	-0.0293***	-0.0291***	-0.0963***	-0.0312***	-0.0082	-0.0480***
	(0.0099)	(0.0087)	(0.0079)	(0.0111)	(0.0092)	(0.0078)	(0.0134)	(0.0102)	(0.0238)	(0.0086)
Business Tax Base	-0.1029***	-0.0854**	-0.1028***	-0.0961***	-0.1076***	-0.0910**	-0.0892**	-0.0984**	-0.0750	-0.0802
	(0.0297)	(0.0355)	(0.0269)	(0.0360)	(0.0383)	(0.0365)	(0.0413)	(0.0410)	(0.0856)	(0.0639)
Intergovernmental Transfers	-0.0484	-0.0017	0.0151	-0.0921*	-0.0412	-0.0187	0.0104	-0.0502	0.1032	-0.0332
	(0.0605)	(0.0513)	(0.0396)	(0.0556)	(0.0574)	(0.0504)	(0.0526)	(0.0582)	(0.0814)	(0.0643)
Property Tax B Multiplier	0.0443***	0.0433***	0.0453***	0.0433***	0.0481***	0.0354**	0.0363***	0.0491**	0.0344*	0.0429***
	(0.0159)	(0.0154)	(0.0106)	(0.0128)	(0.0181)	(0.0177)	(0.0116)	(0.0224)	(0.0199)	(0.0150)
Business Tax Multiplier	0.0262***	0.0280***	0.0255***	0.0236***	0.0241***	0.0209***	0.0300***	0.0262***	0.0271**	0.0221***
	(0.0062)	(0.0065)	(0.0045)	(0.0056)	(0.0061)	(0.0064)	(0.0065)	(0.0061)	(0.0120)	(0.0059)
Other Outcomes										
Population	0.0078	0.0104*	-0.0023	0.0079	0.0014	0.0076	-0.0047	0.0088	-0.0113**	0.0027
	(0.0059)	(0.0061)	(0.0042)	(0.0064)	(0.0063)	(0.0048)	(0.0080)	(0.0062)	(0.0057)	(0.0067)

Notes: The table shows SDID results from estimating Equation 1 for various outcome variables and robustness specifications. All outcomes are in logs. Inference is based on 500 bootstrap draws. *T-year 1990* sets the treatment year to 1990 for all municipalities. *No Treat. Restriction* drops the requirement that the withdrawal exceeds 1% of 1989 population and the population cap of 300,000. *W/o Oth. Nations* excludes all municipalities in which other nations had an active military base during 1989/90. *W/o ZRG* excludes all municipalities belonging to the former *Zonenrandgebiet* analyzed in Ehrlich and Seidel (2018). *Including NRW* adds Northrhine-Westphalia to the donor pool. *Unit Weight Revenues* applies the unit weights estimated for total revenues uniformly across all outcome variables. *Pop < 200,000* excludes municipalities with a population of 200,000 or more in any observation year. *Small Withdrawal* treats municipalities with a withdrawal between 0 and 0.2% of 1989 population as the treated group. *TWFE* refers to a dynamic TWFE specification augmented with five-year pre-treatment outcome variable growth decile group fixed effects to account for pre-trends.

Figure 7: Time Placebos

(a) # Full-Time Workers (b) Business Tax Base



Notes: The figure shows time placebos for two outcome variables. The red line depicts a time placebo where the treatment year is set to 1985 for all. The blue line depicts a specification where the treatment year is set to the first announcement year, 1990, for all treated municipalities. The shaded areas are 95% confidence intervals from 500 bootstrap draws. For the number of full-time workers all states under analysis are included in the sample. For the business tax base, only Bavarian municipalities are included, as the data for Baden-Wuerttemberg and Hesse are not available for a sufficiently long pre-period.

5.4 Spillovers

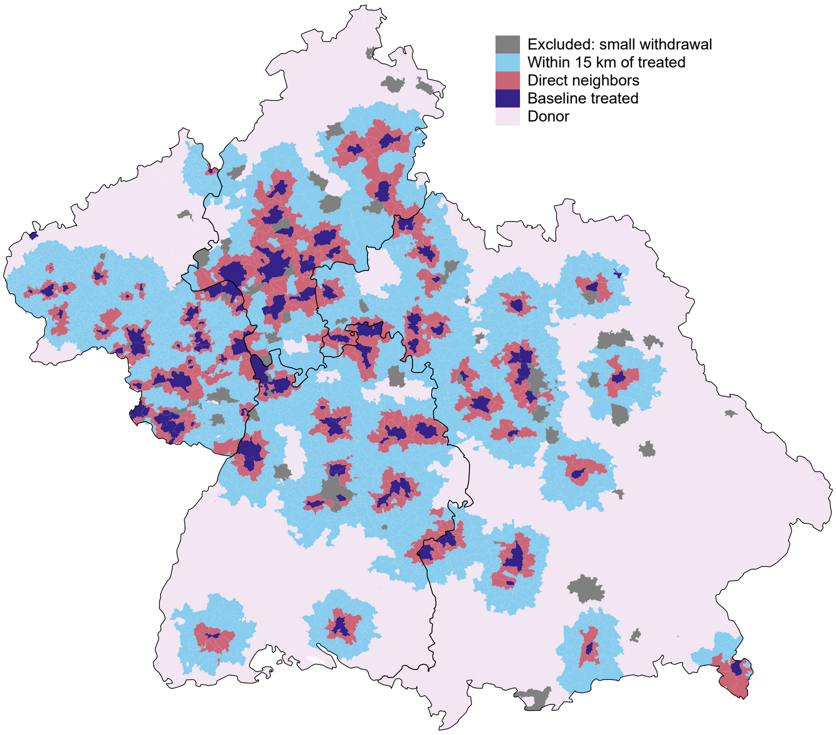
Since local labor markets are integrated, it is likely that the troop withdrawal not only affected the treated municipalities themselves but also had an impact on surrounding municipalities. These spillovers must be taken into account when evaluating the overall impact of the withdrawal. Following Butts (2023), including potentially contaminated nearby units in the control group biases DiD estimates toward zero. We therefore model spatial spillover zones explicitly and exclude more centrally located units from the control group in each respective specification. Therefore, in addition to our baseline estimates, we define two spillover groups for which we run separate sets of regressions.

For the first group, we recode treatment status to 1 if a municipality is a direct neighbor of a baseline treated municipality (*direct neighbors*). For the second spillover group, we redefine a municipality as treated if it lies within a 15 km distance of a treated municipality (*15 km*).[25] For the analysis of spillover effects, municipalities that are more centrally located than the investigated spillover group are excluded from the analysis. For example, when analyzing the 15 km group, both direct neighbors and baseline-treated individuals are dropped from the sample. The sample definition is visually depicted in Figure 8.

[25]The 15 km threshold is conservative. In 1989, the mean one-way commuting distance in West Germany was 9.6 km (Verron et al., 2005). Interpolating from Bundesregierung (1995), more than 75% of commuters travelled less than 15 km. Moreover, as Figure 8 illustrates, the nearest donor pool municipality is typically located well beyond 15 km from a treated municipality. This suggests that the radius captures the vast majority of potential spillover effects without reducing the size of the donor pool too excessively.

Since there is no clearly appropriate base closure announcement year for these spillover municipalities, we rely on a non-staggered treatment definition and set the treatment year to 1990 for all spillover municipalities. The results are listed in Table 2, columns *Direct Neighbors* and *15 km*. To investigate dynamics, we supplement the results in the table with Figure 9, which shows SDID results for our two main labor and public finance outcome variables: the number of full-time workers and total revenues.

Figure 8: Spillover and Donor Pool Definition

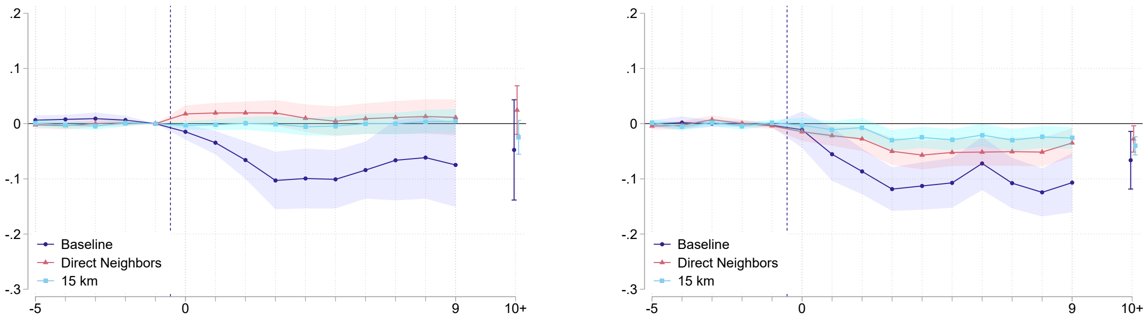


Notes: The figure shows spatial spillover group definitions. The baseline treated municipalities are depicted in blue. Direct neighbors are depicted in red. Municipalities within a 15 km radius of the baseline-treated municipalities are depicted in light blue. Municipalities with a small withdrawal are always dropped from the donor pool (dark gray). For the analysis of spillover effects, municipalities that are more centrally located than the spillover group under consideration are excluded.

The results indicate that neighboring municipalities are also affected by the withdrawal shock, albeit in different ways. Contrary to treated municipalities, the log number of full-time employed does not significantly react in period 0-9 among direct neighbors (0.0127 [0.3072]) nor within a distance of 15 km around treated municipalities (0.0020 [0.7988]). IAB data identifies workers at the place of work. Thus, the marked difference in the effect on baseline treated vs. spillover units underpins the strong direct US employment effects in baseline treated municipalities. Moreover, the dynamics in Figure 9a even hint at initially slightly positive labor market effects in directly neighboring municipalities.

Figure 9: Spatial Spillovers

(a) # Full-Time Workers (b) Total Revenues



Notes: The figure shows SDID results for the baseline treated (blue), direct neighbors (red), and municipalities within a 15 km radius around baseline treated municipalities (cyan). The shaded areas are 95% confidence intervals from 500 bootstrap draws.

Most likely, some of the workers losing their jobs in the baseline treated quickly find a new job in nearby municipalities. In total, 59613 full-time jobs or 0.0046 full-time jobs per capita are lost in an average year during the first ten post periods. In the long run, the coefficients converge to zero, and none of them remains statistically significant.

The effects on total revenues and expenditures in neighboring municipalities during the first 10 post-periods go in the same direction as in the baseline treated. Figure 9b highlights that the effects in spillover municipalities take more time to materialize and are attenuated in the short run. While there is an immediate effect on revenues in directly neighboring municipalities, it is weaker than in the baseline treated (-0.0412 [0.0001]). In farther away municipalities, negative effects only start to materialize after the fourth post-period and are further attenuated compared to the direct neighbors (-0.0206 [0.0000]). Per year, baseline treated, direct neighbors, and 15 km municipalities lose a substantial amount of 1.82 bn. USD of total revenues or USD 142.46 per capita. In the long run, the negative effect on total revenues across the three groups converges and levels at around -3%. This highlights that economic conditions in the whole region deteriorated lastingly. Table 2 shows that, same as in baseline treated municipalities, in spillover municipalities the revenue effect is again closely mirrored by the effect on expenditures (direct neighbors: -0.0313 [0.0000], 15 km: -0.0126 [0.0829]).

An attenuation pattern in geographic distance similar to the one in revenues is evident for municipal tax multipliers in Table 2. Spillover municipalities also adjust their tax multipliers, but they do it to a lesser extent as the distance to the place of withdrawal grows. Same as baseline treated municipalities, they increase the property tax B multiplier (direct neighbors: 0.0252 [0.0000], 15 km: 0.0156 [0.0000]) by more than the business tax multiplier (direct neighbors: 0.0119 [0.0000], 15 km: 0.0057 [0.0000]).

5.5 Heterogeneity

In addition to our baseline estimates and the spillover specification, we conduct heterogeneity analyses. To do so, we median-split the treated sample by 1989 population size (high vs. low population), population density (urban vs. rural), and debt per capita (good vs. poor initial fiscal condition). For ease of comparability, we visually depict the results in Figures A.4, A.5, and A.6.

Population Size Figure A.4 shows that especially small municipalities are hit hard by the withdrawal. The number of full-time employed decreases sharply by a highly significant −0.0906 [0.0000] while larger municipalities react less strongly (−0.0546 [0.1297]). Similarly, total revenues and expenditures decline more strongly in smaller municipalities (−0.1158 [0.0000], −0.1017 [0.0000]) than in municipalities with higher population (−0.0641 [0.0121], −0.0856 [0.0026]). The long-run effects on full-time employed and total revenues/expenditures are particularly interesting: while larger municipalities bounce back to pre-withdrawal conditions, smaller municipalities remain at low levels until today. Different from the results in Helm and Stuhler (2024), smaller municipalities react more strongly using their discretionary tax-setting power. The relative difference is particularly strong on the business tax multiplier where small municipalities increase by 0.0411 [0.0001] whereas larger municipalities only raise by 0.0112 [0.0848]. A salience argument might explain this: imminent troop withdrawals were likely more salient in small municipalities since they depended more on the positive demand effects induced by US military presence. On the other hand, large municipalities are likely to have been more aware of Census population count adjustments in the context of Helm and Stuhler (2024) as they are more reliant on intergovernmental transfers from heightened population counts.

Urban vs. Rural When differentiating between urban and rural municipalities in Figure A.5, we find marked differences between the two groups. Overall, municipalities with a lower population density (*rural*) are harder hit than those with a higher population density (*urban*). In the first ten post-periods revenues and expenditures react more strongly in rural municipalities (−0.1076 [0.0000], −0.0936 [0.0000]) than in urban municipalities (−0.0699 [0.0025], −0.0930 [0.0006]). In the long-run, revenues and expenditures converge such that rural municipalities exhibit even tentatively higher revenues and expenditures. This effect seems to be mostly driven by intergovernmental transfers: rural municipalities initially lose intergovernmental transfers while urban municipalities exhibit an effect close to zero. However, in the long-run rural municipalities receive substantially higher transfers compared to the pre-withdrawal period. As intergovernmental transfers represent a large share of overall municipal revenues, this can explain the convergence in revenues and expenditures in the long-run. The number of full-time employed bounces back to previous levels in urban municipalities in the long-run whereas rural regions remain at persistently lower levels. The business tax base (−0.1514 [0.0000]) decreases more strongly in rural municipalities than in urban regions (−0.0541 [0.3143]). The pattern in income tax revenues is striking as urban municipalities are nearly not affected by the withdrawal on this dimension (0.0040 [0.7555]) whereas rural municipalities exhibit substantial negative reactions (−0.0692 [0.0000]) which are again highly persistent in the long run. Similarly, rural municipalities react more strongly in raising tax multipliers than do urban municipalities.

Initial Fiscal Condition Analyzing heterogeneity by debt per capita shows that municipalities with worse initial fiscal conditions are more negatively affected by the shock. The revenue effect during the first ten post-periods is −0.0673 [0.0144] in those with better fiscal conditions compared to −0.1121 [0.0000] in those with worse. The picture is mirrored for expenditures. Strikingly, this difference widens further in the long-run. While the number of full-time employed reacts to a similar extent in the medium-run, levels remain low in economically struggling municipalities while municipalities with better initial fiscal conditions return to their pre-withdrawal levels.

5.6 Policy Implications

To link our findings to the existing literature and to make our results intuitive for policy-makers, we translate our estimates into easy-to-interpret multipliers. In doing so, we use our results for the first ten post periods, unless stated otherwise. To calculate multipliers, we (repeatedly) rely on the following statistics:[26]

(1) Aggregate Number of Jobs Lost We take our estimates from section 5 on the relative reduction in the number of full-time workers in treated municipalities, direct neighbors, and municipalities within a 15 km radius around the baseline treated municipalities. We multiply each of the three estimates by the aggregate pre-withdrawal number of full-time workers in the corresponding regions. We arrive at our estimate for (1) by summing up the three resulting numbers.

[26] Detailed explanations of the calculation steps can be found in Appendix D.

(2) Aggregate Military Spending Reduction We use US aggregate military spending data for Germany from the US Department of Defense (1990, 1998) and allocate it to treated municipalities according to their share of total personnel. We sum up the resulting shares of military spending for all municipalities hosting US troops within a 15 km radius around our baseline treated municipalities.

(3) Aggregate Revenues Lost We take our estimates from section 5 on the relative reduction in total revenues in treated municipalities, direct neighbors, and municipalities within a 15 km radius around the baseline treated municipalities. We multiply each of the three estimates by the aggregate pre-withdrawal level of total revenues in the corresponding regions. We arrive at our estimate for (3) by summing up the three resulting numbers.

(4) Aggregate Number of Workers Directly Laid Off by the US Military We use the information that 62,000 Germans were employed by the US military at the end of the cold war (Cunningham and Klemmer, 1995) and that 15,436 foreign nationals were employed by the US military in 2000 (Department of Defense, 2000). We distribute this reduction among all municipalities with positive troop strengths according to their share of overall military and civilian personnel. We sum up the resulting numbers for municipalities within a radius of 15 km around our baseline treated municipalities.

(5) Aggregate Number of Withdrawn Soldiers We calculate the aggregate number of withdrawn US soldiers from all municipalities within a 15 km radius around the baseline treated municipalities using information from the Department of Defense Base Structure Reports.

Military Spending per Created Job The US troop withdrawal can be understood as the abolishment of a particular type of place-based policy. To link our results to this literature, we calculate a statistic frequently reported in place-based policy analyses: the public spending needed to produce one additional job. To do so, we divide (1) the aggregate number of jobs lost during the withdrawal by (2) the aggregate military spending reduction.

When compared to targeted place-based policies, e.g., in Siegloch et al. (2025), who find a cost per job of at most \$19,000, it is evident that foreign troop deployments are a rather ineffective way for creating local jobs: the cost per created job in our setting is almost six times higher at $\$110,400$. Similarly, windfall revenues accruing to local governments can be much more effectively spent in terms of creating jobs (Chodorow-Reich et al., 2012; Serrato and Wingender, 2016). Figure A.7a provides an intuitive visual comparison of our estimate with estimates found in other contexts.

Revenues Lost per USD Military Spending We quantify how much revenue a municipality loses when US military spending is decreased by one USD. This multiplier can help guide local policymakers' expectations about the strength of the impact of a foreign troop withdrawal. Moreover, when total municipal revenues are viewed as a proxy for local GDP, this measure can be loosely interpreted as a local fiscal multiplier.[27] To calculate the multiplier, we divide (3) aggregate revenues lost by (2) the aggregate military spending reduction.

We find that municipal revenues decrease by $0.28 per US Dollar of reduced US military spending. Nakamura and Steinsson (2014) estimate a fiscal multiplier of 1.5 in the context of domestic US military spending. This highlights that foreign military spending is comparably ineffective in improving local economic conditions.

Labor Market Spillovers (Direct vs. Indirect Effect) To link our results to the literature on labor market spillovers from, e.g., mass layoffs (Gathmann et al., 2020), we calculate a multiplier quantifying indirect effects. In line with the existing literature, we calculate the share of indirect job losses of the total number of jobs lost due to the analyzed shock. To do so, we divide (4) the aggregate number of workers directly laid off by the US military by (1) the aggregate number of jobs lost during the withdrawal.

We find that 61% of the reduction in full-time employment is driven by indirect effects on local labor markets. This figure is substantially higher than the 35% estimate from Gathmann et al. (2020) in their setting of mass layoffs. Although the direct shock in our context is mainly on the non-tradable sector with supposedly lower indirect effects, we are dealing with a strong simultaneous demand shock from reduced US military spending in the respective regions. Moreover, the US military not only hired German civilians directly but also contracted with German firms, creating an additional channel through which reduced military spending affected local labor markets. This can explain why labor market spillovers are substantially larger.

[27] No GDP measure exists for German municipalities. Some variables are candidates to proxy local GDP. For example, the municipal share of income taxes or wages from the IAB data could be used. However, both variables are capped at determined thresholds, which would likely severely downward bias our results. Total municipal revenues, while an imperfect proxy themselves, arguably most adequately represent local economic conditions and are thus used here.

Jobs Lost per US Soldier To give an intuitive estimate of the labor market impacts of the withdrawal, we calculate the number of jobs lost per withdrawn US soldier. To do so, we divide (1) the aggregate number of jobs lost by the (5) aggregate number of withdrawn US soldiers.

We find that per withdrawn soldier, the number of full-time jobs within a 15 km radius around our baseline treated municipalities decreases by 0.53. In Figure A.7b we depict the statistic together with estimates in the related literature. The most straightforward and arguably most informative comparison is between Zou (2018) and our estimate. Evidently, Zou's estimate is much higher at 1.2. This suggests that domestic military installations are economically much more integrated than the foreign bases in our setting and thus represent a more relevant economic factor. While other comparisons in the graph are not perfectly clean as the statistics are not fully equivalent, they are still informative. For instance, Becker et al. (2021) find that creating one job in the public sector creates 0.84 additional jobs in the private sector. When compared to the average job increase from a one-job exogenous increase in the tradable sector, as, e.g., in Moretti (2010), our withdrawal-related estimate appears small. Specifically, the local employment multiplier for high-tech job creation can be almost an order of magnitude larger than the multiplier we estimate for US troop deployments.

5.7 Worker-Level Evidence

In the following, we supplement our municipal-level results with worker-level evidence. To explore this, we examine the dynamic impact of base closures on the labor market outcomes of displaced workers up to fifteen years after the initial layoff. Given the nature of this individual-level analysis, we follow the worker definitions and propensity score matching approach of Schmieder et al. (2023) rather than SDID.

Identifying Displaced Workers We identify workers who are directly displaced by the large US troop withdrawals in four steps. First, drawing on social security records, we identify foreign military bases as establishments with an industry code that classifies them as *foreign stationed armed forces*.[28] Figure A.8 illustrates the development of employment and establishment numbers of foreign stationed armed forces in Germany. The depicted movement pattern closely tracks the time series of US military personnel in Germany

[28]This classification corresponds to the three-digit industry code *921* of the 73 version (*WZ73*) and the five-digit code *99002* of the 93 and 03 versions (*WZ93* and *WZ03*). Note that stationed armed forces are no longer identified in the industry codes starting from the 08 version (*WZ08*), which is years after the considerable reduction in US troops' deployments in Germany took place.

(see Figure 2). Second, we define the set of workers potentially affected by the US troop withdrawals as individuals employed by a foreign military base located in a German municipality that hosted US military bases but no military bases of other nations. Third, we define a potentially affected worker i as being displaced if he was employed by a military base in year $c_i - 1$ but not in year c_i. Additionally, define A_m as the year containing the first announcement date of US troop withdrawals within municipality m. We restrict the sample to workers whose displacement year is within five years after the announcement date, i.e., $c_i \in [A_m, A_m + 5]$. Fourth, in line with other papers in the literature (e.g., Schmieder et al., 2023), we restrict the sample of displaced workers to individuals who have been employed for at least four consecutive years at the same military base prior to displacement and who are between 25 and 50 years old in year $c_i - 1$. Finally, define C as the set of all unique values of the set $\{c_1 - 1, c_2 - 1, \ldots, c_I - 1\}$.

Propensity Score Matching For every pre-displacement year $c - 1$ contained in C, we match displaced workers to suitable control workers in four steps. First, we restrict the set of workers to individuals who are employed in year $c - 1$. Second, we only keep workers whose workplace is in a municipality that is not directly affected by US military troop withdrawals and that is not within a 10 km radius of a municipality that is directly affected. Third, we again restrict the sample to individuals who have been employed for at least four consecutive years at the same establishment prior to year c and who are between 25 and 50 years old in year $c - 1$. We append the constructed set of potential control workers to our set of displaced workers. Fourth, within each gender, age and part-time cell (all in year $c - 1$), we estimate the propensity of a worker being displaced using tenure and establishment size (in year $c - 1$), as well as the worker's log annual earnings in years $c - 2$ and $c - 3$. For each displaced worker i, we match the control worker with the closest propensity score (without replacement).

Table 4 depicts the descriptive statistics of the matched estimation sample. Applying the outlined conditions, we identify $11,210$ workers who are directly displaced by the closure of US military bases. All characteristics are measured one year before displacement ($s = -1$). The average age of the estimation sample is around 36 years, while less than a third of the displaced workers are women. We observe that, already in the year preceding displacement, displaced workers work around thirty days less than their matched partners, resulting in lower labor earnings. This finding suggests that some workers have already lost their jobs by the second half of this year, as we measure employment status as of June 30th. This pre-displacement dip is a well-documented feature of mass layoff settings, reflecting anticipatory labor supply reductions and measurement conventions, and does not indicate a failure of the matching procedure (Schmieder et al., 2023).

Table 4: Descriptive statistics matched sample

	Displaced workers		Matched control workers	
	Mean	SD	Mean	SD
Exact matching				
Age	36.24	6.90	36.24	6.90
Female	0.29	0.45	0.29	0.45
Part-time	0.03	0.16	0.03	0.16
Propensity score matching				
Log annual earnings lag2	10.03	0.27	10.05	0.31
Log annual earnings lag3	9.99	0.26	10.00	0.31
Tenure at establishment (days)	3023.90	1241.94	3008.17	1365.66
Firm size (employees)	1226.35	1000.93	1099.62	3321.71
Other characteristics				
German	0.84	0.37	0.92	0.27
Annual earnings	21611.84	7830.32	25414.13	7936.23
Log annual earnings	9.92	0.42	10.09	0.34
Daily wage	59.13	18.81	63.97	17.19
Log daily wage	4.07	0.32	4.12	0.30
Employed this year (days)	325.77	60.85	357.98	27.70
Benefits received this year (days)	22.57	50.55	3.76	18.85
Firm age (years)	16.33	3.17	15.53	3.96
AGR-Agricultural occupations	0.01	0.08	0.01	0.11
EMB-Unskilled manual occupations	0.06	0.24	0.25	0.43
QMB-Skilled manual occupations	0.23	0.42	0.25	0.43
TEC-Technicians	0.05	0.21	0.05	0.22
ING-Engineers	0.03	0.16	0.01	0.11
EDI-Unskilled services	0.24	0.43	0.11	0.31
QDI-Skilled services	0.03	0.17	0.03	0.16
SEMI-Semiprofessions	0.03	0.18	0.05	0.23
PROF-Professions	0.00	0.06	0.01	0.09
EVB-Unskilled com. and admin. occupations	0.04	0.21	0.05	0.22
QVB-Skilled com. and admin. occupations	0.27	0.45	0.16	0.37
MAN-Managers	0.01	0.09	0.01	0.11
Year-individual-observations	11210		11210	

Notes: The table depicts the descriptive characteristics of the estimation sample one year before displacement. The first and second columns depict the mean and standard deviation of the characteristics of the displaced workers. The third and fourth columns depict the mean and standard deviation of the characteristics of the matched sample.

Empirical Approach Equation 2 presents the estimation equation for evaluating the dynamic effect of displacement on various worker-level outcomes. The empirical approach is identical to the one used in Schmieder et al. (2023):

$$Y_{itc} = \sum_{s=-4}^{15} \tau_s \, \mathbf{1}\{t = c + s\} \, Disp_i + \sum_{s=-5}^{15} \phi_s \, \mathbf{1}\{t = c + s\} + \alpha_i + \gamma_t + \mathbf{X}_{it}\beta + \epsilon_{itc}. \tag{2}$$

Y_{itc} denotes the dependent variable of worker i who is displaced in year c and who is observed in year t. $Disp_i$ is a binary variable, which is equal to one for individuals who got displaced as part of the US troop withdrawals and zero for workers contained in the control group. Individual and year fixed effects are captured by α_i and γ_t, respectively. ϕ_k captures observation year relative to baseline year fixed effects. Finally, $\mathbf{X}_{it}$ contains individual, time-varying characteristics, e.g., the worker's age.

The main coefficient of interest is τ_s, which depicts the difference in the displaced worker's outcome around displacement relative to the evolution in the created control group. This difference is estimated relative to the baseline difference in period $t = c - 5$, which is omitted due to collinearity and therefore captured by the intercept ϕ_0. In the following we will explore the dynamic effect of displacement on workers' (1) annual earnings, (2) probability of being employed, (3) probability of receiving unemployment benefits, (4) probability of being employed in the same municipality as in year $c - 1$, and (5) probability of being employed in the same district as in year $c - 1$.

Estimation Results Figure 10 presents the estimation results. We observe that the probability of employment for displaced workers drops by around fifty percentage points in the year of displacement. Roughly three-fifths of this reduction in employment is attributed to an increase in the probability of displaced workers receiving unemployment benefits. The remaining two-fifths can be explained by an increase in the number of workers leaving the German social security system. Not being captured by the social security system in Germany can have several reasons; most importantly, people can leave the labor force, become self-employed, migrate to another country, or work as civil servants. Note that we observe a decline in the number of days being employed already in the year before workers' displacement. This suggests that some of the displacements may have already occurred in the second half of the year, i.e., after June 30th, or that there was a reduction at the intensive margin in the year preceding the displacement year. Focusing on the dynamic effects, we document a slow but steady recovery in the employment probability of displaced workers. This movement is closely tracked by a reduction in the probability of receiving benefit payments. After fifteen years, the employment probability of displaced workers remains around twenty percentage points lower than that of control workers. This difference is entirely explained by the difference between the two groups in their likelihood of being captured by the social security system.

The lower four panels of Figure 10 focus on the effect on annual labor earnings and daily wages. Both variables are measured in 2022 prices. We document a stark drop in annual earnings of around 5,000 Euros in the year of displacement, which corresponds to an earnings drop of around 25 percent. The earnings difference between displaced and matched workers is declining over the years, stabilizing at around 2500 Euros or 12.5 percent. Finally, daily wages (measured on June 30th), decline by around 15 percent in the year of displacement. Similar to earnings, we observe a shrinkage of the difference over the years, stagnating at just below 10 percent fifteen years after displacement. Similar to Schmieder et al. (2023), we observe a decline in annual earnings and daily wages in the year preceding displacement.

Figure A.10 displays further outcomes, examining the impact of displacement on changes in broad occupation and region relative to the year preceding the year of displacement. The probability of working in another occupation increases by around 30 percentage points in the year of layoff, decreasing to around 20 percentage points in the long run. Similarly, the results show that the probability of workers finding employment in another municipality (+60.3 p.p.), county (+51.5 p.p.), or state (+14.7 p.p.) increases in the year of displacement. The effect is only slightly decreasing over the years and persists until fifteen years after the layoff. This suggests that the direct impact of base closure on local employment is persistent.

Heterogeneity We explore the effect of base closures on displaced workers' labor market outcomes for different subgroups based on workers' gender (Figure A.11), age (Figure A.12), and the initial labor market conditions (Figure A.13). Initial labor market conditions are approximated by the ratio of the number of unemployment benefits recipients to the number of employed in the year before displacement at the county level.

Figure A.11 documents that the reduction in the probability of employment is markedly larger for women (−61 p.p.) than for men (−43 p.p.). Fifteen years after the displacement, this difference remains sizable, with a −27 percentage point difference for women and a −17 percentage point difference for men. Different exit rates from the social security system can largely explain this gender difference in the long run. Even when conditioning on being employed, the initial drops in labor earnings and daily wages are substantially higher for women. Women's earnings and wages drop by roughly 40 and 30 percent, respectively.

Figure A.12 depicts that the initial reduction in employment is larger for older (−57 p.p.) than for younger (−39 p.p.) workers. This difference is decreasing over time. There is no notable age difference in employment probability fifteen years after displacement. Different from employment, we find that the age difference in the effect on earnings and daily wages is not only present at the initial drop but also fifteen years after displacement. Fifteen years after the event year, the wages of displaced younger workers are around 5.4 percent lower than those in the matched sample, while the difference is around 13.5 percent for older workers.

Finally, Figure A.13 documents that the negative effect of displacement is more severe in regions with worse initial labor market conditions. We find that the difference in employment probability is diminishing over time, while the effect on earnings and wages persists even fifteen years after the displacement.

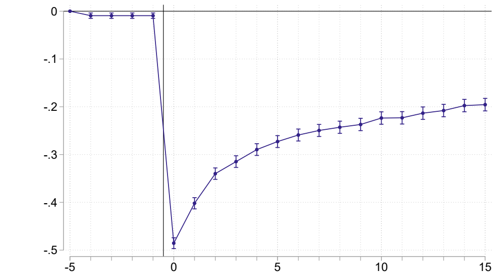
Comparison to the Literature Our worker-level findings align closely with the broader job displacement literature. The initial earnings drop of around 25 percent and the long-run decline of approximately 12.5 percent fall within the 10 to 25 percent (e.g., Athey et al., 2026; Schmieder et al., 2010; Couch and Placzek, 2010) range of persistent losses documented since Jacobson et al. (1993), and closely mirror the estimates for Germany in Schmieder et al. (2023). The 20 percentage point employment gap that persists fifteen years after displacement is consistent with Gathmann et al. (2020). Similar to us, they also document a stark decline in the probability of receiving unemployment benefits and a persistent increase in the probability of laid off workers moving out of the social security system. The considerably higher impact of base closures on women's labor market trajectories is in line with previous work by Illing et al. (2024), who explore the gender differences on the effect of mass layoffs in more detail. The authors demonstrate that part of the gender difference can be attributed to variations in part-time employment and job-search behavior, while parenthood generally enlarges the documented gender gap. Finally, the more severe effects in weaker labor markets are consistent with Bertheau et al. (2023), who find that displacement costs are substantially larger where re-employment prospects are poorer. Overall, our results confirm that US military base closures produce individual-level labor market scars comparable to those in the mass-layoff literature.

6 Discussion

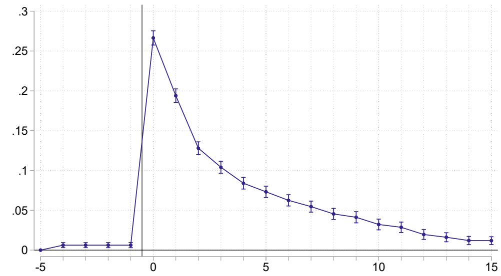
In this section, we discuss the implications of our findings for regional adjustment, fiscal policy, and the local consequences of foreign troop deployments. The results show that US troop withdrawals have a persistently negative effect on the local labor market and public finances, with especially pronounced effects in more vulnerable regions. These

Figure 10: Effect of displacement

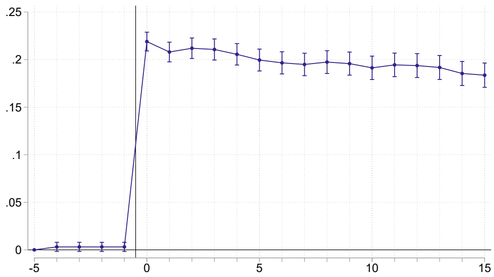
(a) Employment



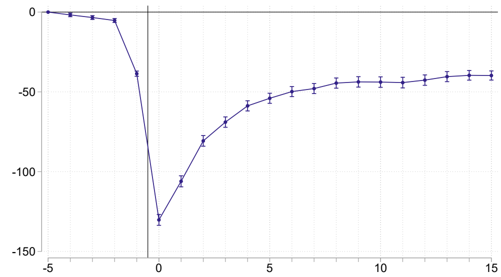
(b) Unemployment benefit recipient



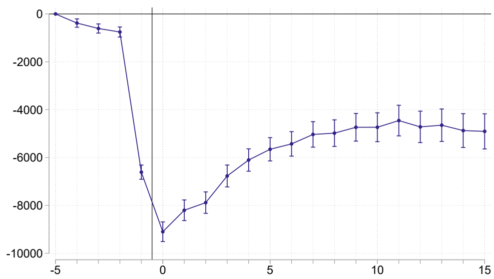
(c) Out of social security system



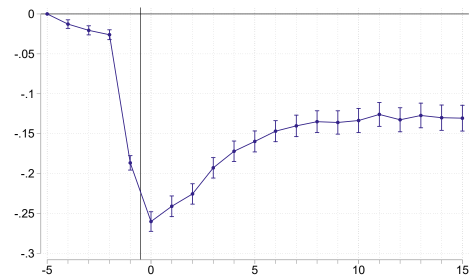
(d) Number of days employed



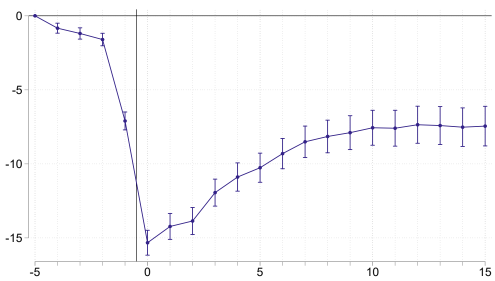
(e) Annual labor earnings



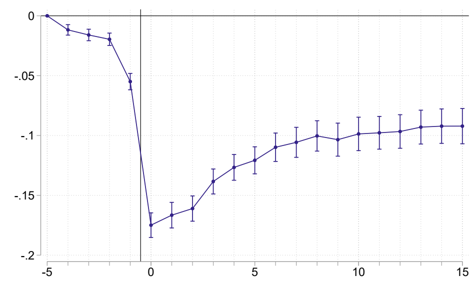
(f) Log of annual labor earnings



(g) Daily wage



(h) Log of daily wage



Notes: This figure illustrates the estimation results of Equation 2. The first panel depicts the dynamic effect on the probability of employment. The second panel depicts the dynamic effect on the probability of receiving unemployment benefits. The third panel depicts the dynamic effect on the probability of dropping out of the dataset, i.e., being outside of the German social security system. The fourth panel depicts the dynamic effect on the number of days employed in a given year. The fifth and sixth panels depict the dynamic effect on annual labor earnings and the logarithm of annual labor earnings, respectively. Both are conditional on being employed in a given year. The seventh and eighth panels depict the dynamic effect on daily wages and the logarithm of daily wages, respectively. Both are conditional on being employed in a given year. All monetary variables are measured in 2022 prices.

findings highlight the importance of reconversion policies, transfer design, and place-based compensation in shaping local adjustment. Finally, we discuss the external validity of our findings.

Reconversion Efforts The results of this paper show that the withdrawal of US soldiers had a strong negative impact on employment and public finances in the treated regions. These negative consequences persist over time. One limitation of this paper is that, due to a lack of data, we were unable to explore the effects on properties and terrain occupied by the US Army after leaving Germany. Local governments could use the freed-up space to create commercial or residential areas. Anecdotal evidence suggests that, in many cities and municipalities, left-behind properties have remained unused for years, potentially limiting the economic capacity of these jurisdictions. Nevertheless, there are also counterexamples of successful reconversion. For example, in 1951, the stationed troops built a large military airport, the *Hahn Air Base*, in a relatively remote area in Rhineland-Palatinate. Following the end of the Cold War, German authorities recognized the high potential and converted it into a commercial airport. The reconversion led to a substantial economic boost, resulting in the creation of many jobs. Figure A.14 shows the employment development in the municipality in which the airport is located. Future studies could systematically explore the reconversion efforts of affected regions, demonstrating how different, endogenous responses of municipalities may help to mitigate the observed shock.

German Intergovernmental Transfers The exact design of the intergovernmental transfer scheme in Germany is often debated among public policy stakeholders. To account for a hypothesized additional burden from hosting foreign troops, most states counted and often still count parts of the troops (and their family dependents) towards a municipality's fiscal need.[29] This implies that the more US soldiers a municipality hosts, the more intergovernmental transfers it receives. Related literature discusses whether this approach is adequate. For example, Broer (2001) raises the question of whether foreign troops are an economic burden or rather a welfare-inducing force. Our study provides clear empirical evidence on this discussion demonstrating that the presence of foreign

[29] In Baden-Wuerttemberg, the so-called *Bedarfsmesszahl* was increased by 15% for each soldier living within barracks. Moreover, 75% of foreign forces living outside barracks were counted towards the municipal population (Landtag Baden-Württemberg, 1986). In Bavaria, 75% of the foreign forces living outside barracks were counted towards the so-called *Ausgangsmesszahl* or basic measure (Landtag Bayern, 1994). In Hesse, the population counts were increased by 100% for foreign troops as well as family dependents living in the respective municipalities (Broer, 2001). In Rhineland-Palatinate the respective increase was 50% (Kirchhoff, 1996; Landtag Rheinland-Pfalz, 1996).

troops enhances prosperity in host regions. Therefore, *Schlüsselzuweisungen* should not account for foreign troop deployments as having a net negative fiscal impact. In line with claims made in the related literature, abandoning this approach would (1) foster fairness in the intergovernmental transfer scheme and, more importantly, (2) avoid the double-burden of simultaneously decreasing tax revenues and intergovernmental transfers in the event of foreign troop withdrawals. For example, in Hesse, foreign troops are no longer counted towards population counts (Landtag Hessen, 2025). Our analyses suggest that other states should follow this example.

Regional Disparities Our heterogeneity analyses indicate that the adverse effects of troop withdrawals are concentrated in particular types of regions. Rural municipalities and those with weak initial fiscal conditions are especially negatively affected. The same pattern emerges at the individual level: workers employed by the military in these more vulnerable regions experience persistently worse labor market outcomes after troop reductions. In light of ongoing concerns about regional disparities, our results suggest that troop withdrawals may exacerbate initial differences between regions, whereas troop deployments may mitigate them. These distributional implications should be considered when determining troop stationing and the location of military bases. Because local authorities often have limited influence over foreign withdrawal decisions, targeted compensatory measures–e.g. through *Bedarfszuweisungen*–could help alleviate the resulting adverse effects.

External Validity Germany's economic strength, institutional stability, and unique Cold War legacy mean that findings may not directly apply to smaller host nations or more recent basing arrangements in Asia or the Middle East. At the same time, Germany represents a major host nation with decades of military presence, extensive infrastructure, and strong integration into NATO, making our results a valuable benchmark for similar long-standing basing arrangements.

7 Conclusion

This paper provides the first comprehensive empirical assessment of the local economic effects of foreign troop withdrawals. Using the historic large-scale withdrawal of US forces from Germany in the early 1990s as a natural experiment, we combine newly digitized data from the US Department of Defense with detailed administrative information on local labor markets and municipal public finances. Employing a synthetic difference-in-differences approach, we isolate plausibly exogenous variation in exposure to troop reductions to estimate both short- and long-term local effects.

Our results reveal that the withdrawal triggered substantial and persistent local economic consequences. Employment falls substantially per withdrawn soldier, driven primarily by indirect demand effects rather than direct layoffs. At the worker level, displaced employees face long-run declines in employment probabilities and earnings, with losses especially pronounced for women, older workers, and those in initially weaker labor markets.

The withdrawal shock translates directly into local fiscal stress. Municipal revenues fall significantly, driven by a shrinking business tax base and declining intergovernmental transfers. Municipalities respond by consolidating budgets and raising tax multipliers, particularly on the relatively inelastic property tax base.

Both effects are highly persistent, with employment and many fiscal outcomes remaining on a depressed level until today. Spatially, employment effects are localized while fiscal spillovers extend into neighboring municipalities. Viewed as a place-based policy, foreign military spending is relatively inefficient in creating local demand: we find a cost per job of $\$110,400$, around six times higher than for targeted place-based policies in Germany.

Our findings contribute to the broader literature on local labor market adjustments and public finance responses to large, geographically concentrated demand shocks. By documenting both fiscal and micro-level labor market consequences, this study highlights that the economic footprint of foreign military deployments extends well beyond direct employment effects. Moreover, the persistence of these effects implies limited local resilience to abrupt external demand contractions.

From a policy perspective, our results carry several implications. First, troop withdrawals impose lasting costs on host regions, especially where economic diversification is limited. For the optimal design of fiscal equalization schemes, this finding suggests that the presence of foreign troops should not be treated as a fiscal burden. Instead, automatic fiscal stabilizers should be in place for withdrawals while the opposite has often been done historically. On the individual worker level, our results highlight that troop withdrawals closely resemble the effects of mass layoffs and can be countered with similar policy measures. From an international perspective, the findings underscore that foreign military spending can have meaningful local multiplier effects. However, they are modest relative to domestic multipliers. While our analysis focuses on economic impacts and abstracts from the broader political and strategic dimensions of troop deployments, it provides critical empirical evidence on the economic side of foreign military presence.

Overall, the evidence demonstrates that the large-scale US troop withdrawal from Germany constituted a major local economic shock with long-lasting repercussions for employment, fiscal stability, and individual workers' careers. These insights are essential for policymakers evaluating the costs and benefits of hosting military forces and for understanding how local economies adjust to the sudden withdrawal of a long-standing external economic anchor.

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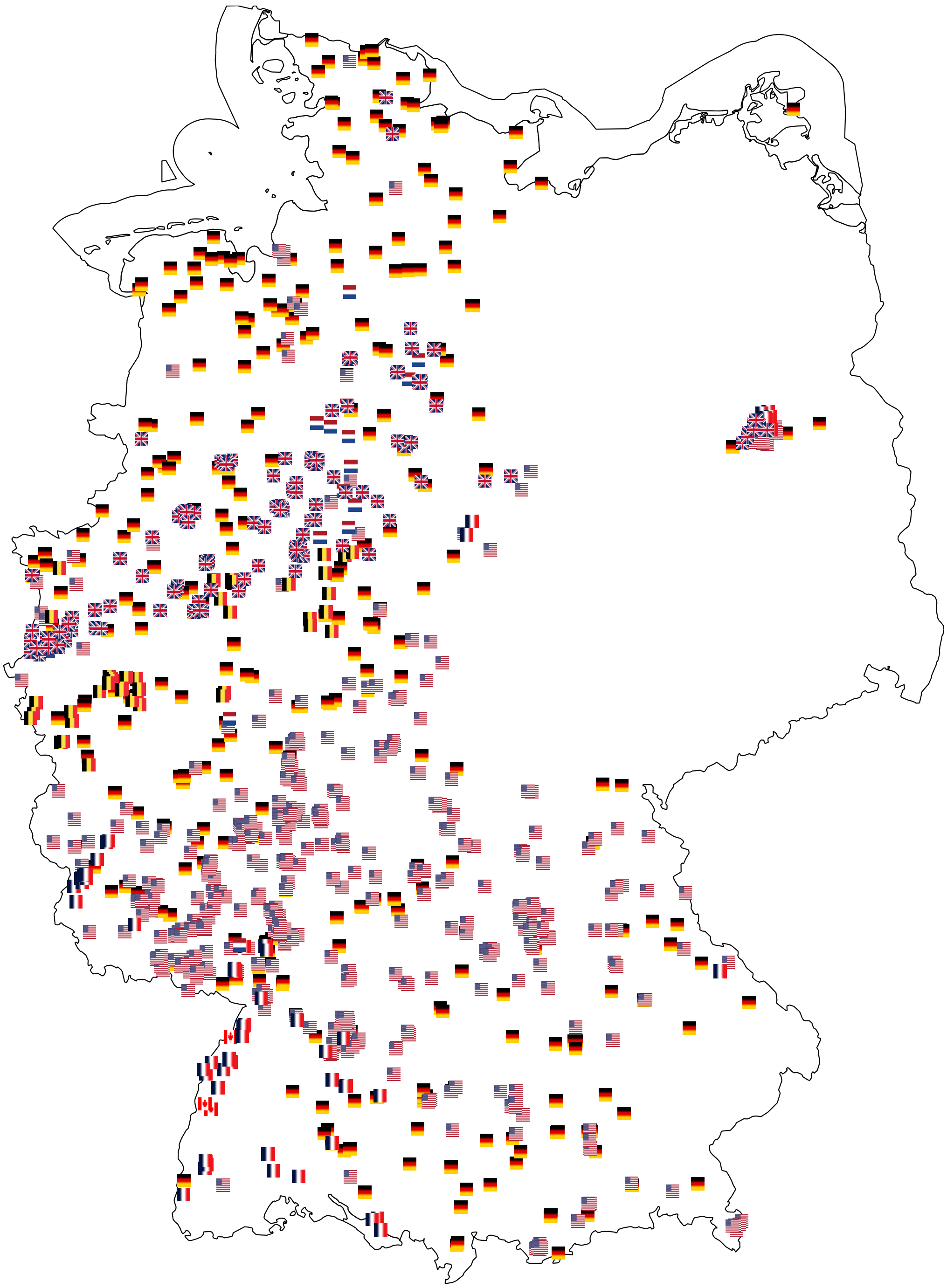
A Figures

Figure A.1: Military Bases in Germany



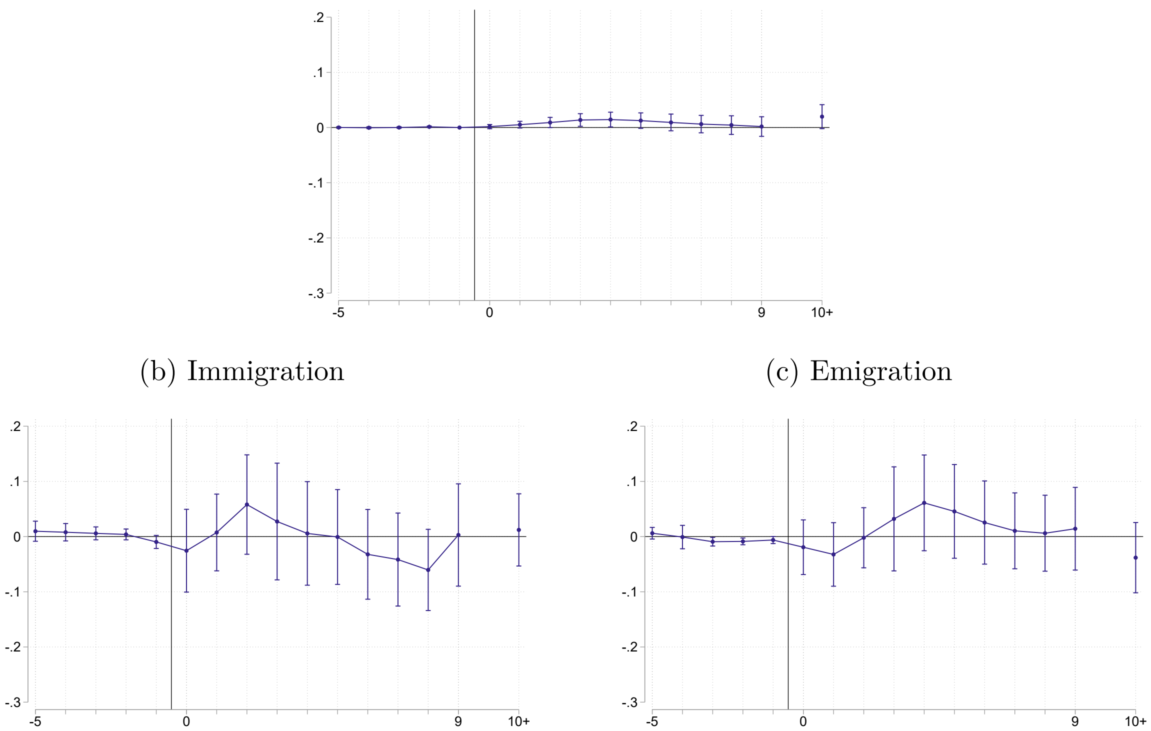
Notes: The figure shows the locations of all military bases in Germany by nation from 1945 to today excluding Soviet installations.

Figure A.2: Population, Immigration, and Emigration

(a) Population

(b) Immigration

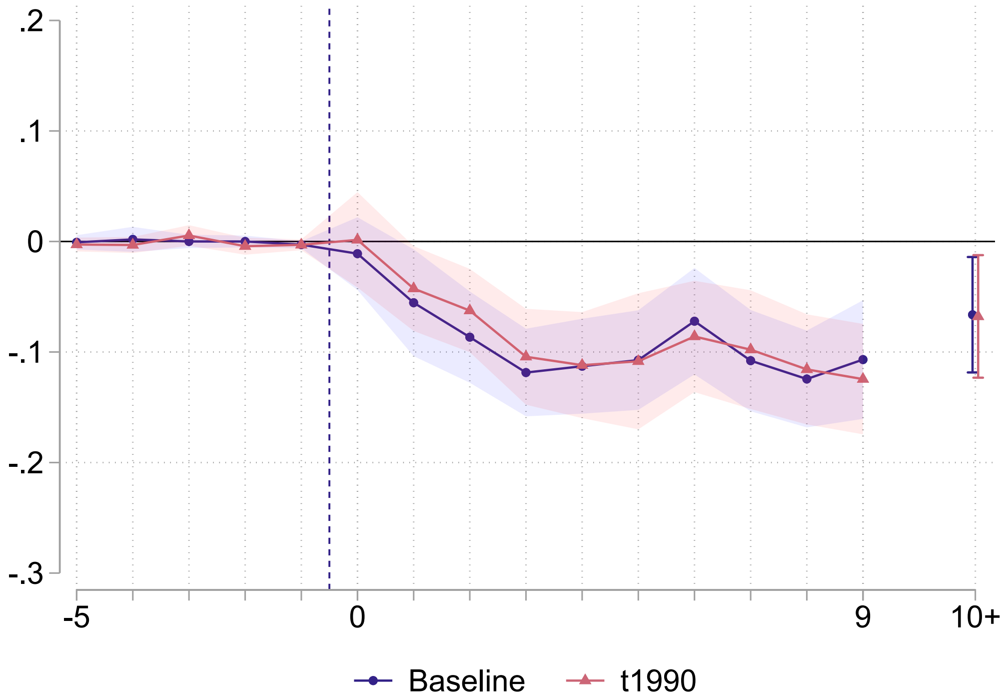
(c) Emigration



Notes: The figure shows SDID results from estimating Equation 1 for municipal population size, immigration, and emigration. All outcomes are in logs. The absence of significant differential trends in population and migration outcomes supports that the results in the main analysis are not confounded by selective migration flows associated with German reunification.

Figure A.3: Other Results

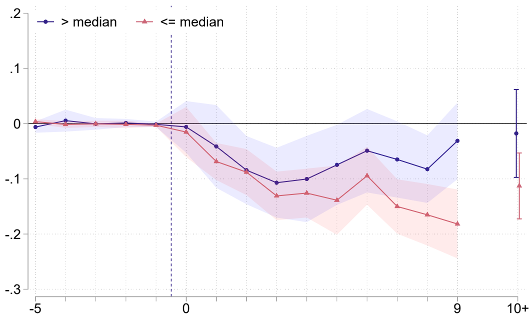
(a) Total Revenues: Baseline and Treatment Year 1990



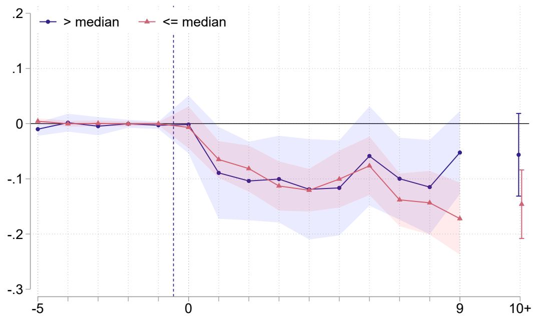
Notes: Figure A.3a shows SDID results from estimating Equation 1 comparing it to a specification in which the treatment year is set to 1990 for variable total revenues. All outcomes are in logs.

Figure A.4: Population Size

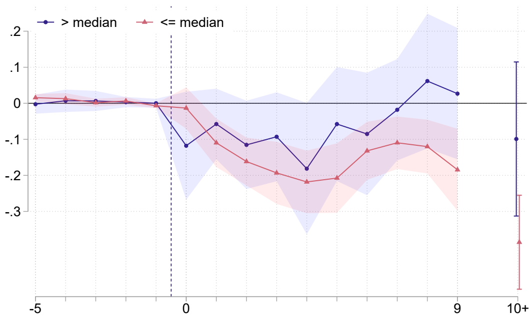
(a) Total Revenues



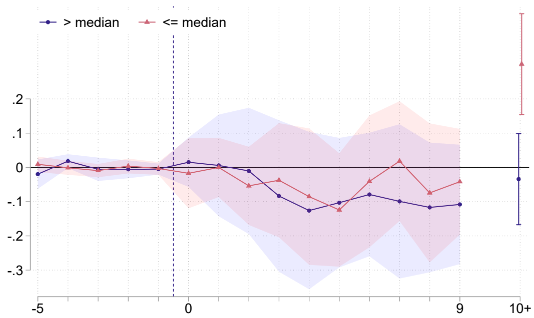
(b) Total Expenditures



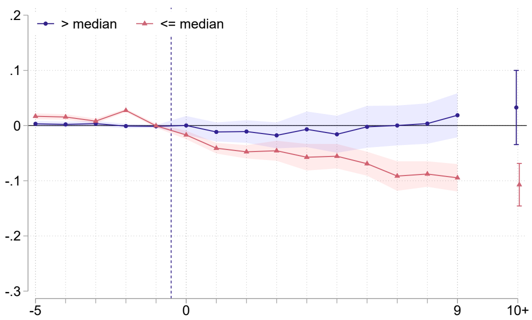
(c) Business Tax Base



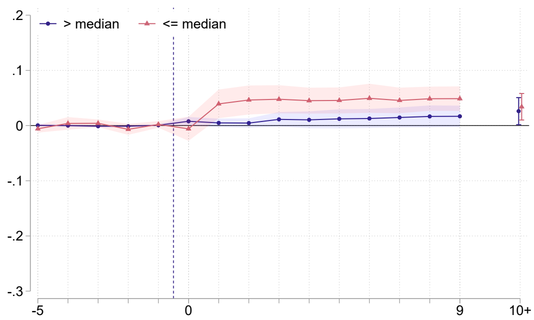
(d) Intergovernmental Transfers



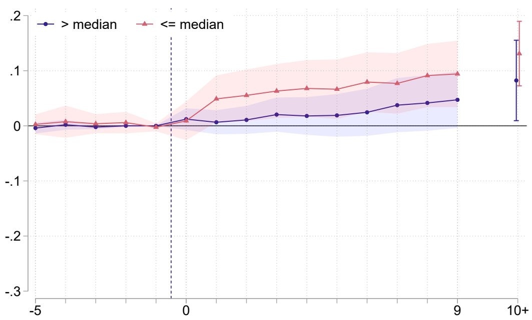
(e) Income Tax Revenues



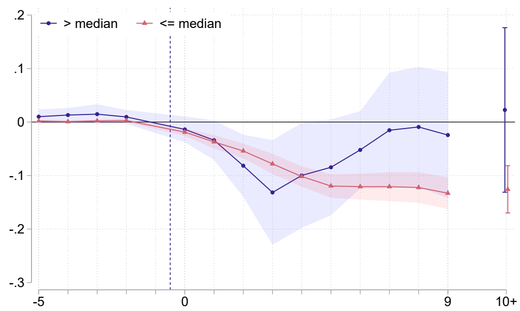
(f) Business Tax Multiplier



(g) Property Tax B Multiplier



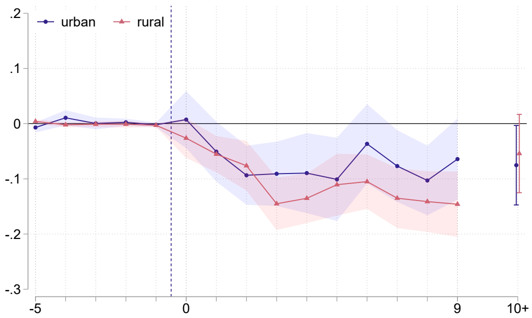
(h) # Full-Time Employed



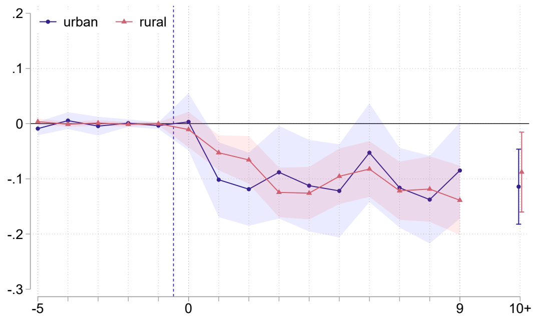
Notes: The figure shows SDID results from estimating Equation 1 for various outcome variables median-splitting the treated sample by population size in regions with higher population (blue) and lower population (red). All outcomes are in logs.

Figure A.5: Urban vs. Rural

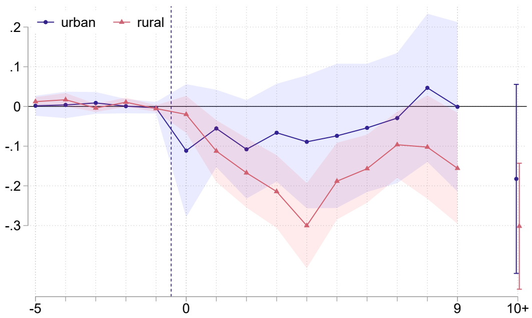
(a) Total Revenues



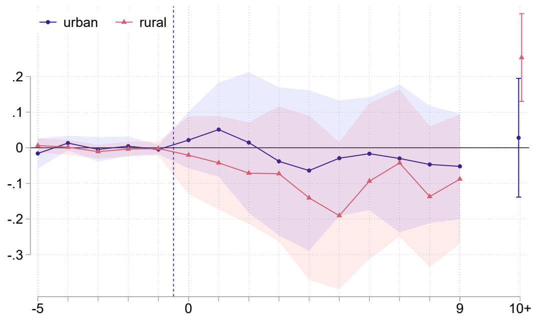
(b) Total Expenditures



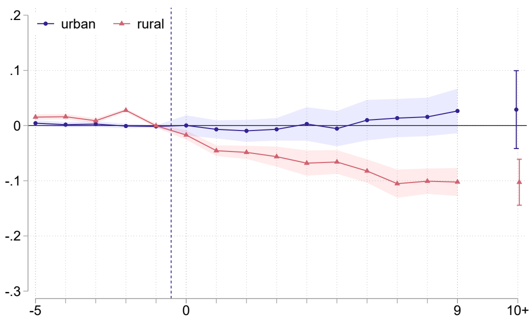
(c) Business Tax Base



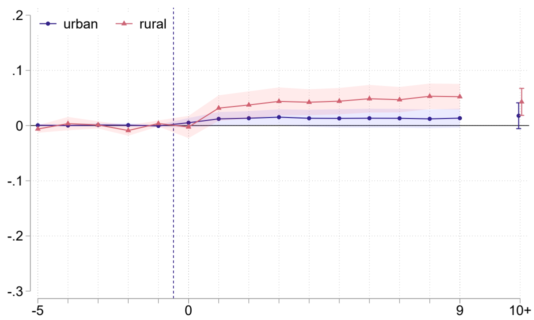
(d) Intergovernmental Transfers



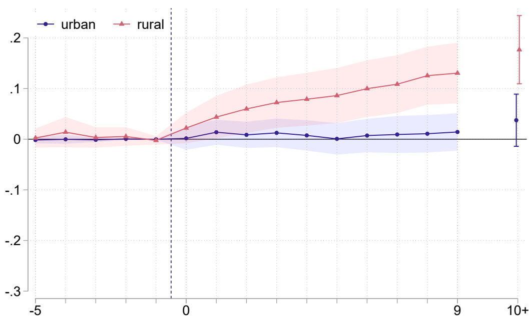
(e) Income Tax Revenues



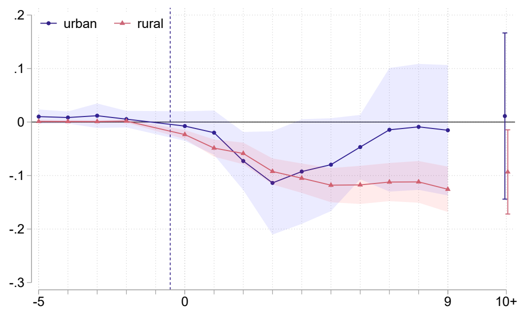
(f) Business Tax Multiplier



(g) Property Tax B Multiplier



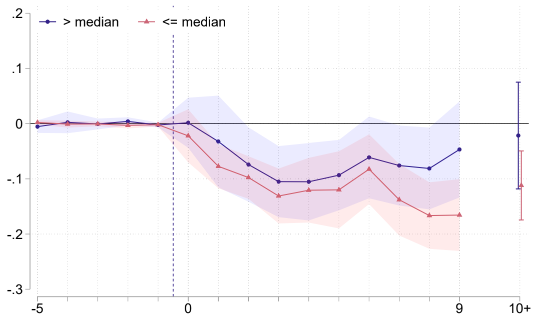
(h) # Full-Time Employed



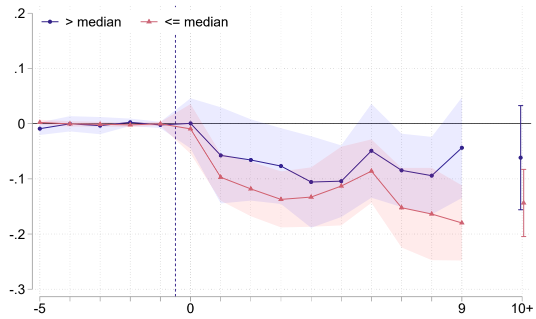
Notes: The figure shows SDID results from estimating Equation 1 for various outcome variables median-splitting the treated sample by population density in urban (blue) and rural (red) regions. All outcomes are in logs.

Figure A.6: Initial Fiscal Condition

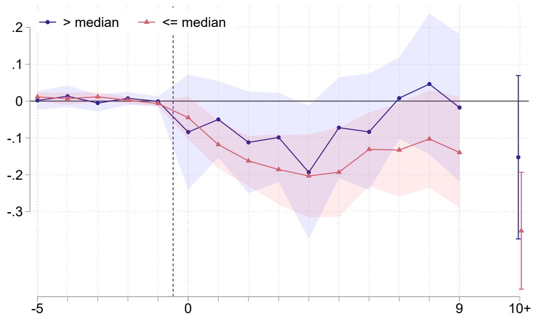
(a) Total Revenues



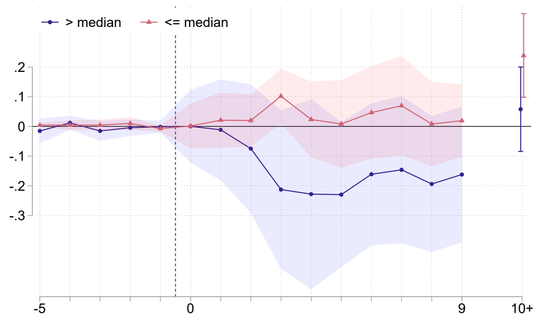
(b) Total Expenditures



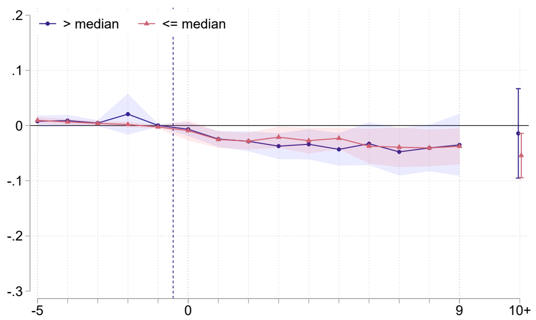
(c) Business Tax Base



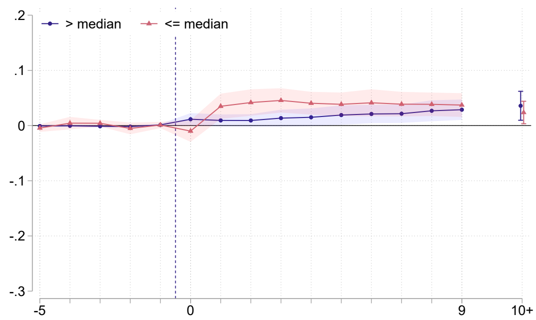
(d) Intergovernmental Transfers



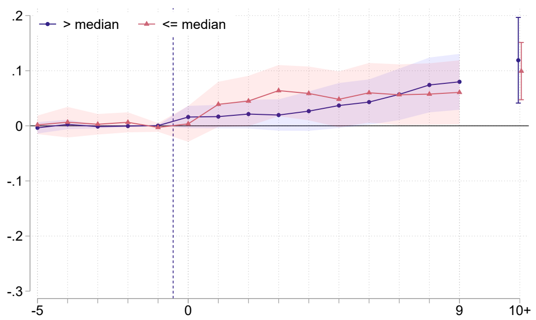
(e) Income Tax Revenues



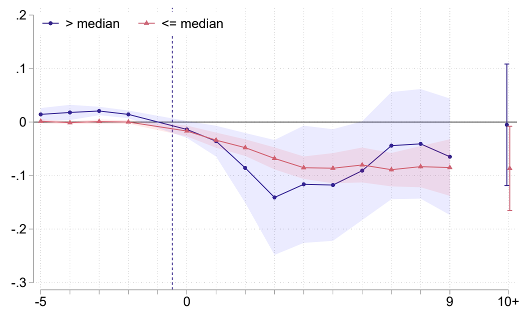
(f) Business Tax Multiplier



(g) Property Tax B Multiplier



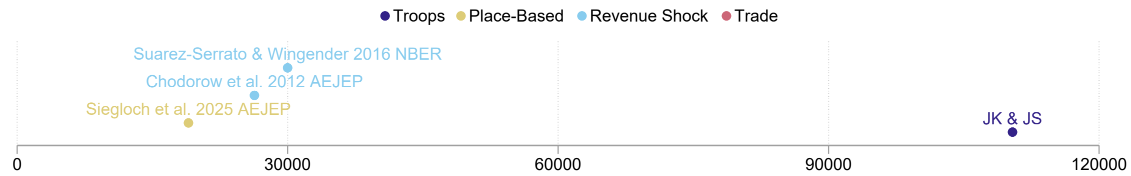
(h) # Full-Time Employed



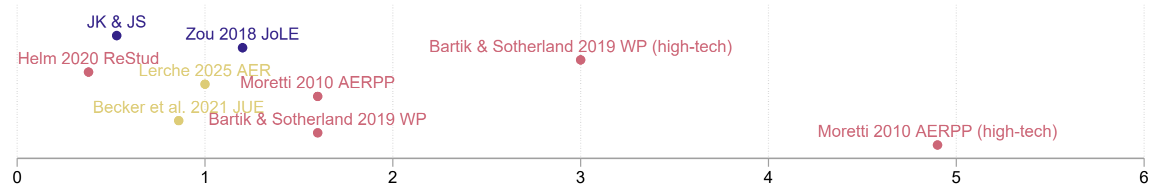
Notes: The figure shows SDID results from estimating Equation 1 for various outcome variables median-splitting the treated sample by debt per capita in regions with better initial fiscal condition (blue) and worse initial fiscal condition (red). All outcomes are in logs.

Figure A.7: Literature Comparisons

(a) Spending per Job



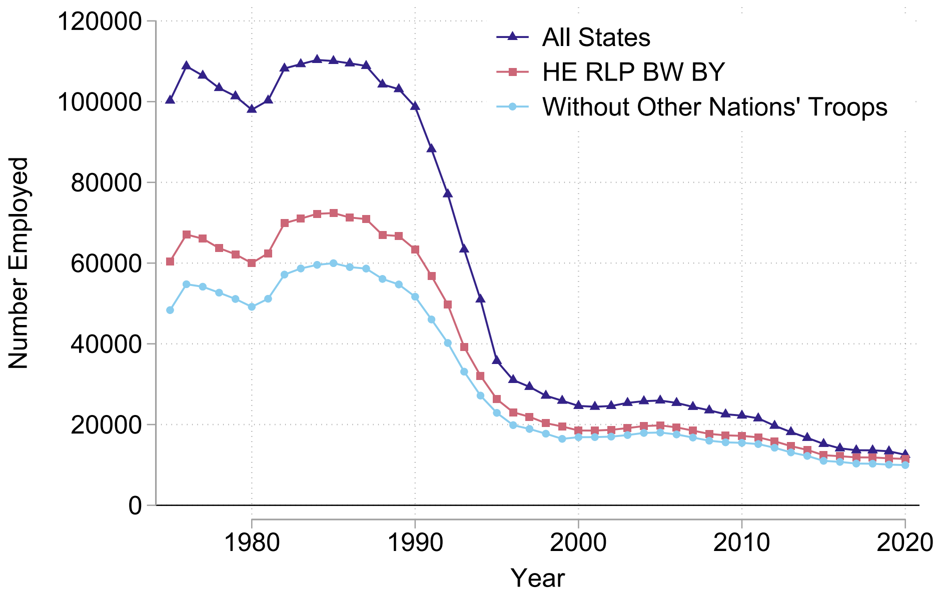
(b) Jobs per Job



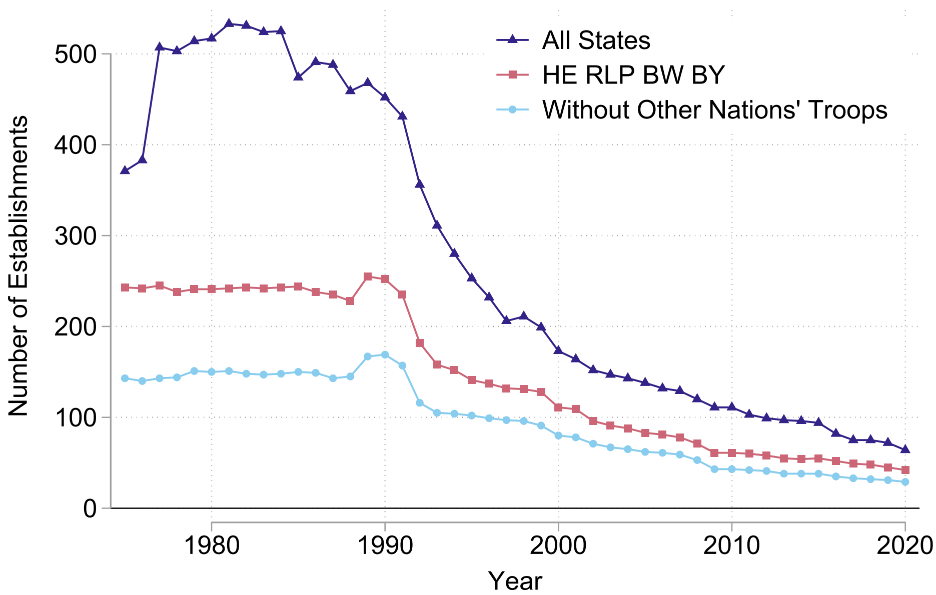
Notes: The figure collects a non-conclusive list of estimates similar to the statistics we calculate for our setting to allow for intuitive comparisons. Panel (a) shows spending per job estimates. Panel (b) depicts job creations (losses) per created (lost) job. The estimates are allocated to four different types of settings (see legend). *JK & JS* refers to this project. The following papers are included: Siegloch et al. (2025); Becker et al. (2021); Helm (2020); Bartik and Sotherland (2019); Zou (2018); Serrato and Wingender (2016); Chodorow-Reich et al. (2012); Moretti (2010).

Figure A.8: Development of Stationed Troops' Military Bases (Sector 921)

(a) Employment

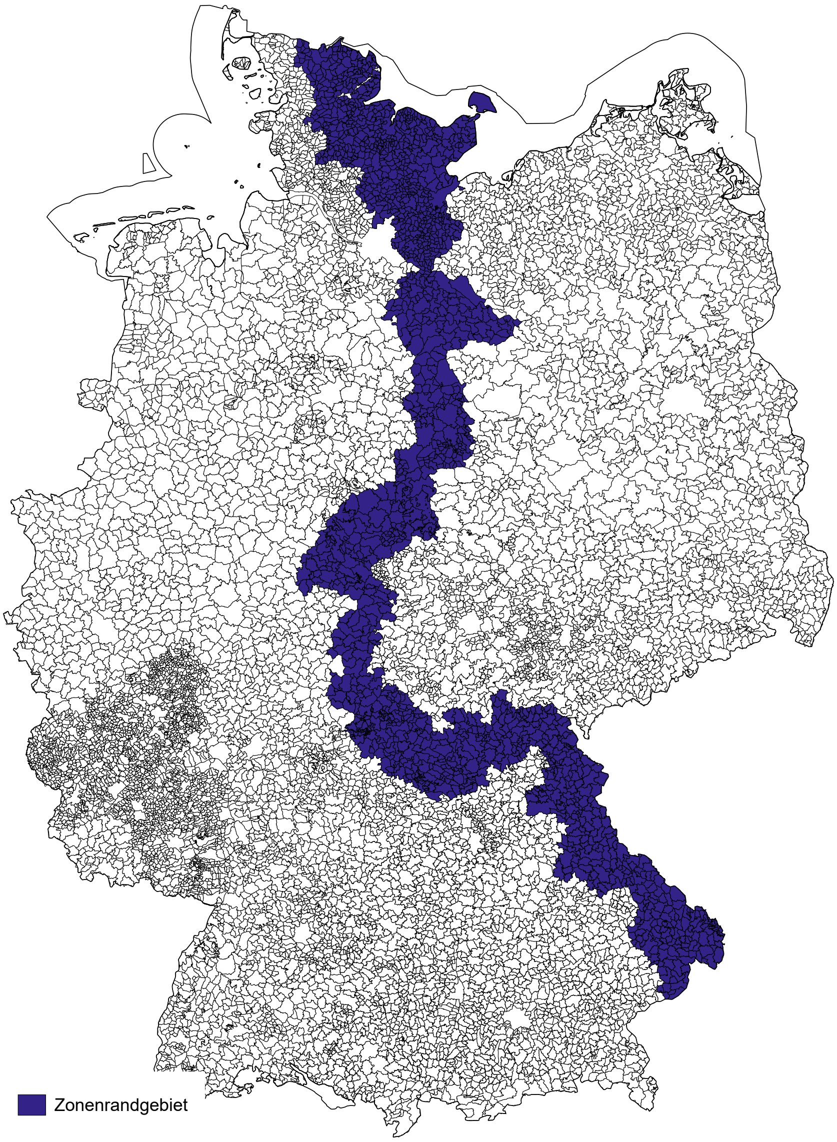


(b) Establishments



Notes: Figure A.8a illustrates the development of employment at military bases of stationed troops between 1975 and 2020. Figure A.8b illustrates the number of military bases with stationed troops, as listed in German social security records. Military bases of stationed troops are identified by sector 921 of the WZ73 classification. The blue line depicts all military bases. The red line depicts bases in the states of *Baden-Württemberg*, *Bavaria*, *Hesse*, and *Rhineland-Palatinate*, i.e., states used as part of our estimation sample. The light blue line uses the same set as the red line but excludes municipalities that also hosted troops from other nations, in addition to the US military.

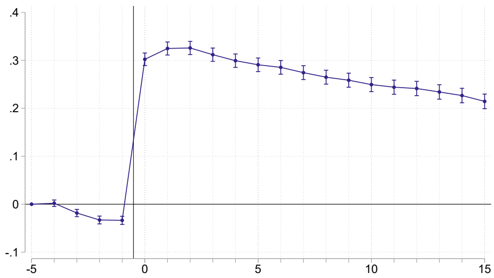
Figure A.9: *Zonenrandgebiet* (Ehrlich and Seidel, 2018)



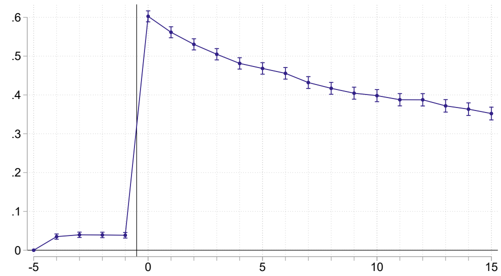
Notes: The figure shows the so-called *Zonenrandgebiet* highlighted in blue. Since this region was subject to place-based policies during the German divide, we exclude it in a robustness check (Ehrlich and Seidel, 2018).

Figure A.10: Effect of Displacement: Further Outcomes

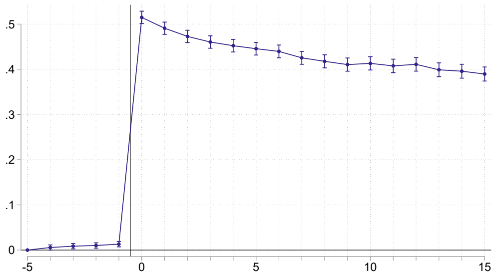
(a) Employment in Other Occupation



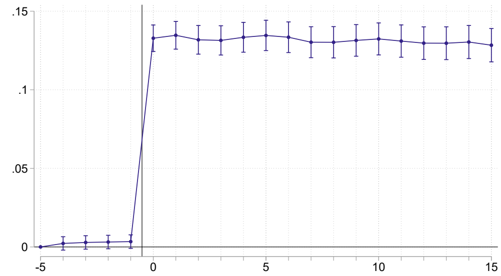
(b) Other Municipality



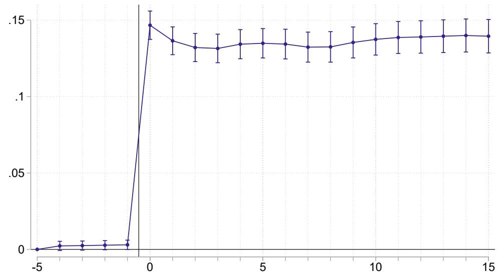
(c) Other County



(d) Other *Regierungsbezirk*



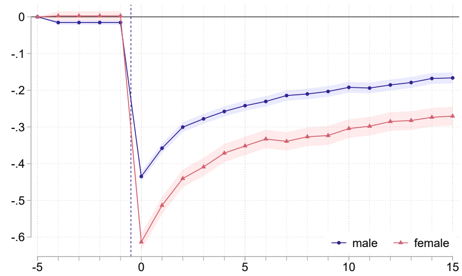
(e) Other State



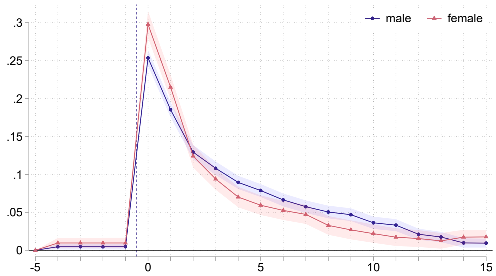
Notes: This figure illustrates the estimation results of Equation 2. The first panel depicts the dynamic effect on the probability of being employed in a different occupation than in the year preceding displacement. The second to fifth panels depict the dynamic effects on the probability of being employed in a different municipality, country, *Regierungsbezirk*, or state than in the year preceding displacement.

Figure A.11: Effect of displacement by gender

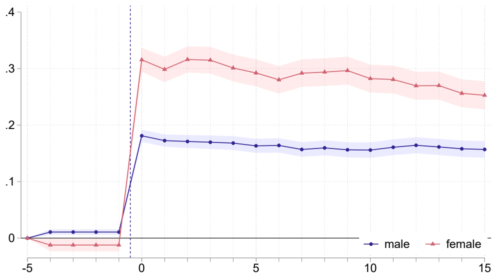
(a) Employment



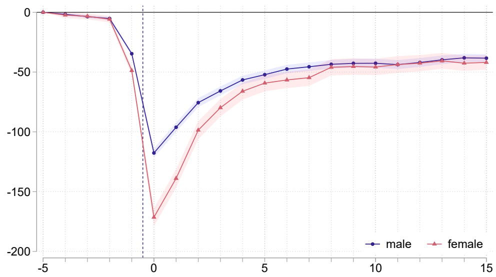
(b) Unemployment benefit recipient



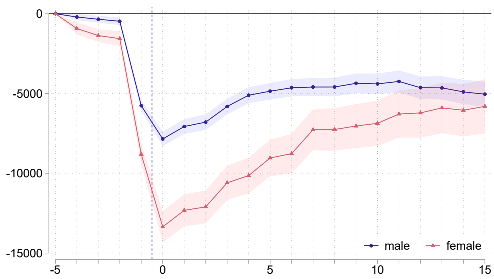
(c) Out of social security system



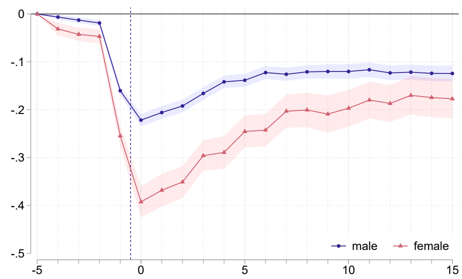
(d) Number of days employed



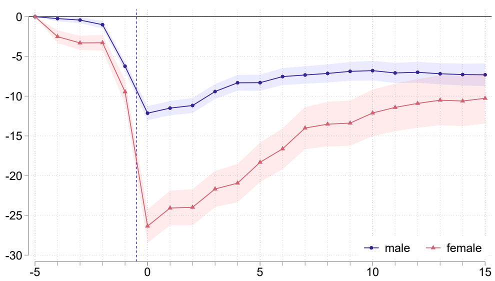
(e) Annual labor earnings



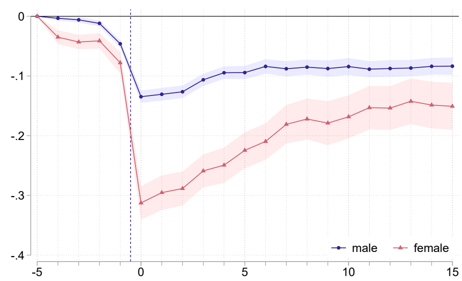
(f) Log of annual labor earnings



(g) Daily wage



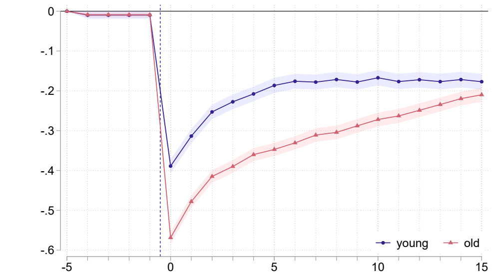
(h) Log of daily wage



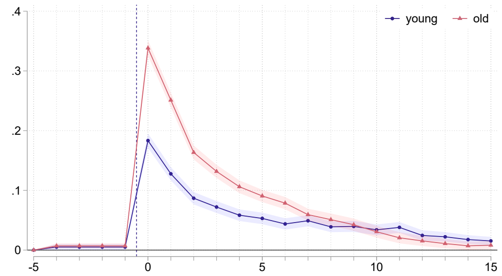
Notes: This figure illustrates the estimation results of Equation 2. The first panel depicts the dynamic effect on the probability of employment. The second panel depicts the dynamic effect on the probability of receiving unemployment benefits. The third panel depicts the dynamic effect on the probability of dropping out of the dataset, i.e., being outside of the German social security system. The fourth panel depicts the dynamic effect on the number of days employed in a given year. The fifth and sixth panels depict the dynamic effect on annual labor earnings and the logarithm of annual labor earnings, respectively. Both are conditional on being employed in a given year. The seventh and eighth panels depict the dynamic effect on daily wages and the logarithm of daily wages, respectively. Both are conditional on being employed in a given year. All monetary variables are measured in 2022 prices.

Figure A.12: Effect of displacement by age

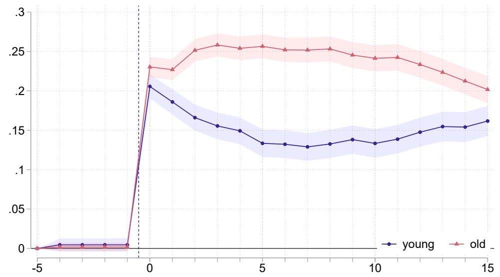
(a) Employment



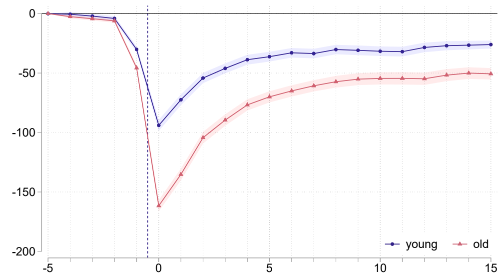
(b) Unemployment benefit recipient



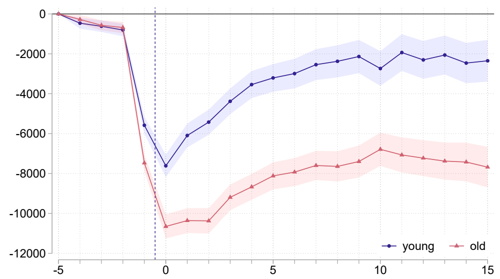
(c) Out of social security system



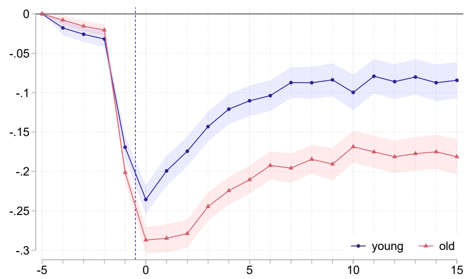
(d) Number of days employed



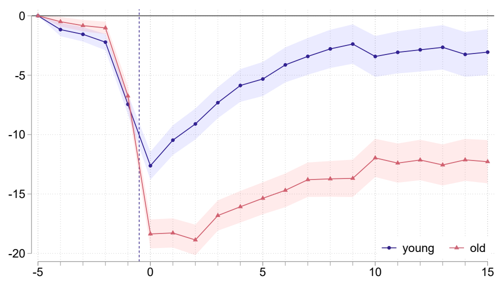
(e) Annual labor earnings



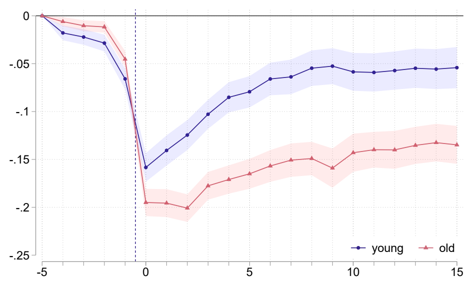
(f) Log of annual labor earnings



(g) Daily wage



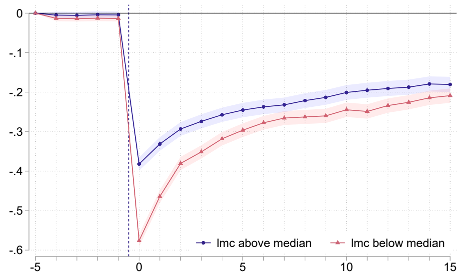
(h) Log of daily wage



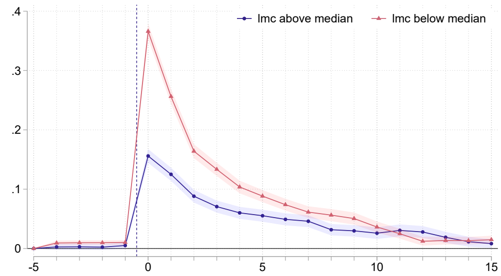
Notes: This figure illustrates the estimation results of Equation 2. The first panel depicts the dynamic effect on the probability of employment. The second panel depicts the dynamic effect on the probability of receiving unemployment benefits. The third panel depicts the dynamic effect on the probability of dropping out of the dataset, i.e., being outside of the German social security system. The fourth panel depicts the dynamic effect on the number of days employed in a given year. The fifth and sixth panels depict the dynamic effect on annual labor earnings and the logarithm of annual labor earnings, respectively. Both are conditional on being employed in a given year. The seventh and eighth panels depict the dynamic effect on daily wages and the logarithm of daily wages, respectively. Both are conditional on being employed in a given year. All monetary variables are measured in 2022 prices.

Figure A.13: Effect of displacement by initial labor market conditions

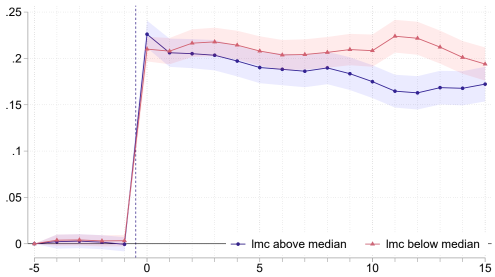
(a) Employment



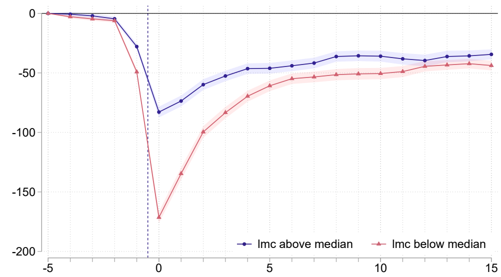
(b) Unemployment benefit recipient



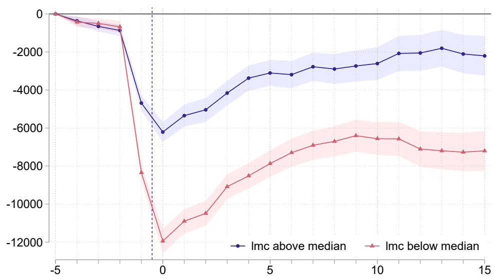
(c) Out of social security system



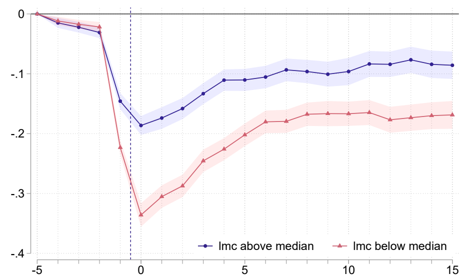
(d) Number of days employed



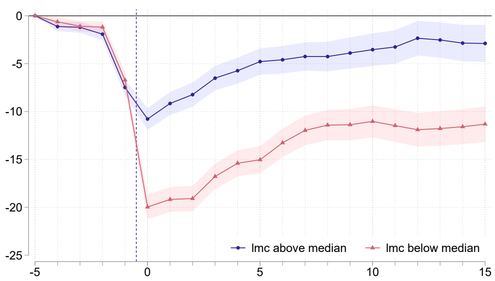
(e) Annual labor earnings



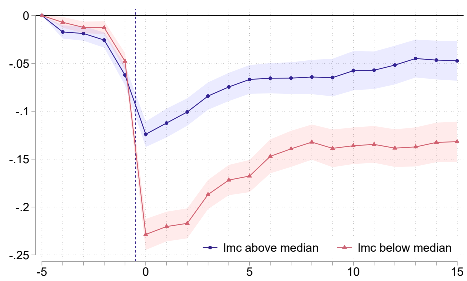
(f) Log of annual labor earnings



(g) Daily wage

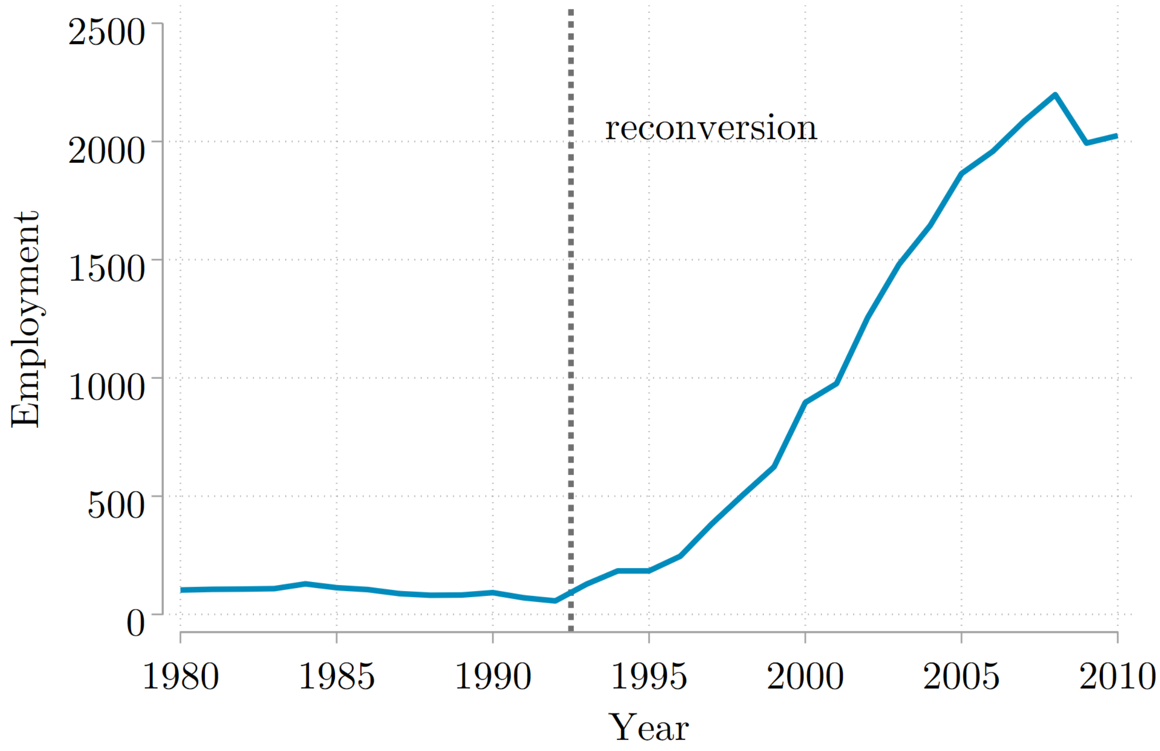


(h) Log of daily wage



Notes: This figure illustrates the estimation results of Equation 2. The first panel depicts the dynamic effect on the probability of employment. The second panel depicts the dynamic effect on the probability of receiving unemployment benefits. The third panel depicts the dynamic effect on the probability of dropping out of the dataset, i.e., being outside of the German social security system. The fourth panel depicts the dynamic effect on the number of days employed in a given year. The fifth and sixth panels depict the dynamic effect on annual labor earnings and the logarithm of annual labor earnings, respectively. Both are conditional on being employed in a given year. The seventh and eighth panels depict the dynamic effect on daily wages and the logarithm of daily wages, respectively. Both are conditional on being employed in a given year. All monetary variables are measured in 2022 prices. Initial labor market conditions are approximated by the ratio of the number of unemployment benefits recipients to the number of employed in the year before displacement at the county level.

Figure A.14: Employment Hahn Airport



Notes: The figure depicts employment in *Lautzenhausen*, i.e., the municipality in which most of the Hahn Airport is located. The dashed line indicates when the airbase was converted to a commercial airport in 1993. The data is based on employment liable to social security contributions between the years 1980 and 2010.

B Tables

Table B.1: US Military Spending in Germany

	2024 bn. USD
Salaries to West German blue and white collar personnel	3.36
Private consumption of US soldiers, civilians and family dependents in the German local economy	4.39
Purchases made by US contracting agencies	2.02
Goods and services purchased by US installations	3.77
Construction work	1.23
For general orders (e.g. Bundespost, Bundesbahn and for private consumption and rents)	2.71
Total	**17.48**

Notes: The table contains sources of US military spending (in)directly impacting German (local) labor demand deflated to 2024 USD. *Source:* Sharp (1990), own calculations.

Table B.2: Descriptives: Municipal Finances

	2019 Old States	
	Sum bn. EUR	Share %
Administrative Budget (*Verwaltungshaushalt*)		
Taxes and Similar Revenues	93.1	41.1
Business tax	37.9	16.7
Muni share of income tax	35.7	15.8
Muni share of VAT	6.6	2.9
Property taxes	11.4	5.0
State Transfers	77.0	34.0
Rule-based transfers	42.6	18.8
Discretionary transfers	34.4	15.2
Fees and Other Revenues	41.6	18.4
Capital Budget (*Vermögenshaushalt*)		
All Revenue Categories	14.7	6.5

Notes: The table shows aggregate municipal revenues from administrative budget (*Verwaltungshaushalt*) and capital budget (*Vermögenshaushalt*) split into various sources for the western states of Germany in year 2019. *Source:* Bundesministerium der Finanzen (2023).

Table B.3: Troop Descriptives

	1990	2000
Installations		
Sum # personnel	360164	115802
Average # personnel at installations with personnel > 0	1043.95	732.92
# installations	783	209
# installations with personnel > 0	345	158
Municipalities		
# municipalities with personnel > 0	181	62
Average # personnel in municipalities with personnel > 0	1989.86	1867.1
Average # of workers employed in sector 921 (as % of overall full-time employment) in municipalities with personnel > 0	1.151%	2.305%

Notes: The table shows selected descriptive statistics of US installations in Germany for the years 1990 and 2000. # personnel refers to the total personnel, including military and civilian personnel. This is why the numbers are different from figure 2 where only active duty military personnel is considered. Personnel > 0 includes only installations with positive total personnel numbers in the respective year. The last row uses social security records to compute the number of individuals full-time employed at a foreign military base (sector 921) as a share of overall municipal full-time employment. *Source:* DOD Base Structure Reports (Department of Defense, 1989) and Integrated Employment Bigraphies (IAB).

C Data

C.1 Base Structure Reports

The Base Structure Reports (BSRs) are available for the years 1982, 1986, 1987, 1989, 1991, 1993, 1999, 2001-2007, 2009, 2010, 2012, 2015, 2018. For most of the BSRs, the data contained in a BSR for fiscal year y is a snapshot of troop strengths as of September in year $y-1$. This implies that BSR 1991 contains information from year 1990, BSR 1999 from year 1998, and so on. One exception is BSR 1993 which contains information as of September 1991, that is, $y-2$. Currently, years 1990, 1991, 2000, 2001, 2002, 2003, 2004, 2009 (BSRs 1991, 1993, 2001, 2002, 2003, 2004, 2005, 2010) are used for quantifying the withdrawals.

As discussed in the main text, the HCR is used to fill gaps stemming from missing BSR years. Moreover, closed installations do not appear in later BSRs which makes the HCR a valuable source of information for the closure date of these bases, too. If a base was finally closed, personnel numbers are inferred to be zero from that point onwards. In principle, the personnel numbers for years in which information is missing could be interpolated in the year range of BSRs (currently: 1990-2010) and extrapolated to the first and last year for the period under analysis outside the BSR observation window (1985-1989, 2003-2020). This would allow computing withdrawal intensities for each year and municipality. However, this measure would be noisy. Thus, we stick to a more conservative withdrawal intensity calculation for the whole period: $\frac{totalpersonnel2000 - totalpersonnel1990}{municipalpopulation1989}$.

We manually collect the geolocations of each installation via web search using the installation name provided in the BSRs. We focus on installations with positive numbers of total personnel in any of the years under analysis. Only 754 (0.01127%) of total personnel summed up across all years cannot be attributed to a municipality because finding the geolocation of the respective installations was impossible.

D Calculating Multipliers

(1) Aggregate Number of Jobs Lost The pre-withdrawal number of jobs is calculated for year 1989.

(2) Aggregate Military Spending Reduction We use aggregate military spending data for Germany from Department of Defense (1990, 1998) and calculate its aggregate reduction between 1989 and 1997.[30] We then calculate the share of spending accruing to all municipalities with any positive troop numbers according to their share of total military and civilian personnel in the respective years (1989, 1997). We sum up this reduction in military spending for all municipalities within a radius of 15 km around our baseline treated municipalities. That is, we also take into account those municipalities which are excluded in the baseline analysis for their small withdrawal size relative to their 1989 population. This is adequate as the military spending reduction in these regions also contributes to the effect within the three treatment regions under analysis. Note, however, that this inclusion is innocuous: the "small withdrawal" municipalities within a radius of 15 km around the baseline treated only make up a share of 4.12% of the military and civilian personnel withdrawal size within this region (9,426 soldiers of 219,370+9,426 soldiers within the radius of 15 km around baseline treated municipalities). Excluding them would only slightly change results. Given the aggregate nature of our multiplier, we view this measure of spending reduction in treated municipalities as precise enough. The shares of military and civilian personnel used to allocate the aggregate spending figure to municipalities is calculated using information from the Base Structure Reports for year 1990 and 2000.

(3) Aggregate Revenues Lost The pre-withdrawal total revenue level is calculated for year 1989.

(4) Aggregate Number of Workers Directly Laid Off by the US Military IAB data does not allow to disentangle workers laid off by the US military from those laid off by other nations. We therefore rely on the overall reduction figures in Cunningham and Klemmer (1995); Department of Defense (2000) for 1990 and 2000 mentioned in the main text. The shares of military and civilian personnel used

[30] Unfortunately this publication series was discontinued after 1998 (Fiscal Year 1997). Therefore, we do not have figures for 2000 which is the year for which we define most other variables for these analyses. However, as the withdrawal and military spending reduction was almost fully completed by 1996, we view the data for 1997 as a sufficiently good proxy for year 2000.

to allocate the aggregate number of German workers directly laid off by the US military to municipalities is calculated using information from the Base Structure Reports for year 1990 and 2000.

(5) Aggregate Number of Withdrawn Soldiers We calculate the number of withdrawn soldiers within a radius of 15 km around our baseline treated municipalities using the difference in military personnel between 1990 and 2000 from the military personnel figures in the US DOD Base Structure Reports. That is, we count any withdrawn US soldier in the area surrounding baseline treated municipalities also taking into account those municipalities which are excluded in the baseline analysis for their small withdrawal size relative to their 1989 population. This is adequate as withdrawn soldiers in these regions also contribute to the effect within the three treatment regions under analysis. Note, however, that this inclusion is innocuous: the "small withdrawal" municipalities within a radius of 15 km around the baseline treated only make up a share of 4.01% of the military personnel withdrawal size within this region (7,043 soldiers of 168,656+7,043 soldiers within the radius of 15 km around baseline treated municipalities). Excluding them would only slightly change results.

E SDID Decomposition

We decompose the SDID estimator as follows. In the following, we assume that the first N_{co} (control) units are never treated, while the last $N_{tr} = N - N_{co}$ (treated) units are treated from time T_{pre} onward. Let A be the adoption date vector, containing the distinct adoption years a.[31] Following Ciccia (2024), the SDID estimator for treatment adoption cohort $a \in A$ can be stated as:

$$\hat{\tau}_a^{sdid} = \frac{1}{N_{tr}^a} \sum_{i \in I^a} \left(\frac{1}{T-a+1} \sum_{t=a}^{T} Y_{it} - \sum_{t=1}^{a-1} \hat{\lambda}_t^{sdid} Y_{it} \right) - \sum_{i=1}^{N_{co}} \hat{\omega}_i^{sdid} \left(\frac{1}{T-a+1} \sum_{t=a}^{T} Y_{it} - \sum_{t=1}^{a-1} \hat{\lambda}_t^{sdid} Y_{it} \right) \tag{3}$$

$$= \frac{1}{T-a+1} \sum_{t=a}^{T} \left(\frac{1}{N_{tr}^a} \sum_{i \in I_a} Y_{i,t} - \sum_{i=1}^{N_{co}} \omega_i Y_{i,t} \right) - \sum_{t=1}^{a-1} \left(\frac{1}{N_{tr}^a} \sum_{i \in I_a} \lambda_t Y_{i,t} - \sum_{i=1}^{N_{co}} \omega_i \lambda_t Y_{i,t} \right) \tag{4}$$

where I_a denotes the subset of treated municipalities containing the indices of municipalities in treatment adoption cohort a. N_{tr}^a denotes the number of treated municipalities in cohort a. Ciccia (2024) show that the period-by-period estimator can then be retrieved as:

$$\hat{\tau}_{a,\ell}^{sdid} = \frac{1}{N_{tr}^a} \sum_{i \in I_a} Y_{i,a-1+\ell} - \sum_{i=1}^{N_{co}} \omega_i Y_{i,a-1+\ell} - \sum_{t=1}^{a-1} \left(\frac{1}{N_{tr}^a} \sum_{i \in I_a} \lambda_t Y_{i,t} - \sum_{i=1}^{N_{co}} \omega_i \lambda_t Y_{i,t} \right) \tag{5}$$

where $\hat{\tau}_{a,\ell}^{sdid}$ depicts cohort a's estimated treatment effect ℓ periods after treatment adoption. Finally, the cohort specific dynamic treatment effects $\hat{\tau}_{a,\ell}^{sdid}$ can be aggregated to receive the dynamic SDID estimator $\hat{\tau}_{\ell}^{sdid}$, predicting the effect of US troop withdrawal on local outcomes ℓ periods after the troop withdrawal took place:

$$\hat{\tau}_{\ell}^{sdid} = \sum_{a \in A_\ell} \frac{N_{tr}^a}{N_{tr}^\ell} \hat{\tau}_{a,\ell}^{sdid} \tag{6}$$

where A_ℓ is defined as the subset of treated cohorts with at least ℓ post treatment periods, while N_{tr}^ℓ denotes the number of municipalities in treatment cohorts a such that $a \in A_\ell$. We will report the static SDID $\hat{\tau}^{sdid}$ as well as the dynamic SDID $\hat{\tau}_{\ell}^{sdid}$ estimator in our later empirical analysis.

[31] By way of example, if the adoption years are all years between 1990 and 1996, then $A = \{a_1, a_2, \ldots, a_7\} = \{1990, 1991, \ldots, 1996\}$. Note that $T_{pre} = a-1$ for all treated municipalities of adoption cohort a.