The Local Economic Impacts of US Troop Withdrawals in Germany*

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Abstract

This paper analyzes the local economic impacts of military 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 local public finances. We use administrative data provided by the US Department of Defense to precisely 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. Per a withdrawn US soldier, the number of local jobs decreases by 0.53. The decrease in economic activity results in a reduction of revenues for affected municipalities. To balance decreased revenues, affected municipalities lower their expenditures, while increasing business and property tax multipliers. Viewed as a place-based policy, the US troop deployment was rather ineffective in creating local employment. We estimate the cost per job to be USD 110,400 of US military spending. We supplement the municipal-level results with a worker-level analysis. We document that workers displaced by the closure of a US military base have persistently lower employment rates. At the same time, their daily wages are around 9.2 percent lower fifteen years after their 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.

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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, 05.08.2020, Fox News Videos

What are the local economic impacts of foreign military troop deployments? The heated debate on this geopolitically relevant issue reveals that there is no empirical answer to the question. This scarcity is due to various identification challenges when it comes to causally estimating the effects of troop withdrawals. (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 1990s. 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 ($\sim$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 could influence the process. We utilize this ideal historical setup to investigate the local economic impact of US troop withdrawals in Germany. The analysis proceeds along five research questions: *(1) What are the effects on local labor markets?* We find that the number of full-time workers decreases due to the withdrawal of military troops. This effect is not driven by workers directly employed at US military bases. *(2) What are the effects on local public finances?* Municipal revenues decrease substantially. Local governments react by reducing public expenditures and increasing local tax rates. *(3) What are the long-run consequences?* The adverse effects on local labor markets, municipal public finances, and local tax rates are highly persistent. *(4) How do the ef-*

fects spill over spatially? Troop withdrawal only affects the local employment numbers in treated municipalities. Focusing on local public finances, we document negative spillover effects on public revenues of nearby municipalities. *(5) What is the effect on directly affected workers?* Displaced workers' employment probability and daily wages are lower than those of a comparable control group not only in the short run, but also fifteen years after their displacement.

In this paper, we combine detailed information on US military deployments with state-of-the-art econometric methods to examine the effect of military troop withdrawal on local labor markets and municipal public finances. We digitize the administrative Department of Defense Base Structure Reports, which contain detailed information on troop strengths at the military base level. We hand-collect the geolocations of all 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, thereby attenuating selection concerns to a certain extent. For the empirical analysis, we alleviate possibly remaining concerns by employing the synthetic difference-in-differences (SDID) estimator, which selects a comparable control group and has various advantageous properties in our setting (Arkhangelsky et al., 2021; Clarke et al., 2023). We treat the first announcement of a base closure in a given municipality as our treatment year (event time zero) to account for potential anticipation effects.

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 consumption and the military bases cease to purchase goods and services in the local economy. 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 public (tax) revenues.

The empirical analysis is organized around the five research questions mentioned above: First, we find that 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 15km 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 -8.8%. This overall effect is not driven by individuals who were directly employed by US military bases, but by workers employed 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.

Second, 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. By way of example, local business tax (LBT) revenues (-10.0%), as well as intergovernmental transfers (-4.8%) decrease. Additionally, we exploit a particularly interesting feature of the German setting: the approximately 11,000 municipalities have discretionary tax-setting power for business and property 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. In line with findings in the related literature (Helm and Stuhler, 2024; Merlo et al., 2023), the tax multiplier on the more elastic business tax base does not react as strongly to the shock (2.6%).

Third, 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 -7.0% 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.

Fourth, 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 surrounding as treated units. We find that the effects on the labor market are

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

highly localized. We do not find a statistically significant impact on full-time employment in direct neighbors of treated municipalities. Focusing on local public finances, our results indicate that municipalities in the surrounding areas of treated units are also negatively affected. 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 in a radius of 15 km around treated municipalities, leveling at approximately -3%.

Fifth, we supplement the municipal-level results with worker-level estimations that explore the impact of base closure-induced displacement on workers' labor market outcomes in the short and long term. We draw on microdata provided by the IAB to identify workers who were working at a US military base and got displaced shortly after the announcement of this base's closure. We follow the previous literature on workers' displacement (e.g. Schmieder et al., 2023; Illing et al., 2024) and deploy a propensity-score matching approach to evaluate the effect of displacement on treated workers' labor market outcomes. 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 workplace is usually located in a different municipality than their previous workplace. The negative impact on labor market outcomes is especially pronounced for women, older workers, and in regions with initially unfavorable labor market conditions.

We translate our reduced form estimates into easy-to-interpret multipliers to link our findings to the existing literature and to make the results intuitive for policymakers. 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 USD 0.28. In comparison to targeted place-based policies, military spending is rather ineffective in creating local jobs. We find the cost per created job to be USD 110,400 of military spending.

Methodologically, our setting is advantageous for various reasons. (1) The identifying variation is credibly exogenous due to the abruptness of the geopolitical shift and the high pace of the withdrawal. Corroborating the exogeneity argument, no public policy stakeholders were involved in the withdrawal process. Instead, the US Department of Defense unilaterally decided on base closures primarily on military grounds. (2) Unlike usual relocation processes, the troop reductions in our setting were permanent, enabling the study of long-run dynamics. (3) The German federalist landscape allows for detailed spa-

tial analyses of public finance reactions and spillovers due to municipalities' discretionary tax-setting power. (4) Via the administrative Base Structure Reports, the withdrawal can be cleanly quantified at the fine-grained level of the ∼11,000 German municipalities. (5) 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 German demand (Sharp, 1990). Approximately 200,000 soldiers and 223,000 family dependents left Germany during this period. (6) Finally, the fall of the Iron Curtain marks a pivotal historic moment, lending the context inherent relevance.

Deployments of foreign military troops 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. To the best of our knowledge, we are the first to quantify the public finance impacts of a foreign withdrawal.[2] We provide a comprehensive economic analysis, summarized in easy-to-interpret metrics, which can inform policymakers dealing with foreign troop deployments.[3]

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 rarely studied type of shock: a sizable reduction of local military spending. Our focus on long-run consequences is particularly informative as evidence in this field is scarce (Helm and Stuhler, 2024). We also provide insights on tax responses (Merlo et al., 2023; Fremerey et al., 2022), contributing to the limited literature on the dynamics of reactions in affected jurisdictions (Helm and Stuhler, 2024). Second, the troop withdrawal can be understood as a special type of mass layoff and/or the abandonment of a particular place-based policy. By providing key metrics, such as the cost per created job, as well as fiscal multipliers, we directly connect to these strands of literature (e.g. Gathmann et al., 2020; 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

[2] A working paper related to our setting is aus dem Moore and Spitz-Oener (2012), who only look at county-level labor market outcomes of the 1990 US troop withdrawal from Germany on the district level.

[3] Other related studies are either pure case studies, focus on different outcomes, are much smaller in temporal scope, and/or do not overcome the endogeneity issues discussed above and are thus less relevant for this paper (Dahlberg et al., 2021; Calia et al., 2021; Nickelsburg, 2020; Paloyo et al., 2010; Andersson et al., 2007).

military spending (Auerbach and Gorodnichenko, 2012). Finally, our worker-level analysis of the long-run labor market outcomes of displaced workers closely connects to previous papers that explore the impact of mass layoffs on workers' labor market trajectories (e.g. Schmieder et al., 2023; Illing et al., 2024).

We contribute to the scarce literature on the economic effects of troop withdrawals. The earliest paper in this context is Hooker and Knetter (2001), who analyze US domestic troop withdrawals between 1971 and 1994 but lack a convincing identification strategy from a current econometric viewpoint. Zou (2018) analyzes the same setting between 1990 and 2010 on the county level. To overcome the endogeneity issues present in the domestic relocation process, the author combines instrumental variable estimation and matching methods in a static DID setting. Zou (2018) shows 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 (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 migration response detected in Zou (2018), real per capita revenues and expenditures are not affected, which is not the case in our setting. These findings highlight that foreign withdrawals can have very different impacts compared to domestic withdrawals. Unlike the above papers, we conduct a rich, dynamic analysis on a spatially fine-grained level and can examine long-term effects to the present day. Finally, we supplement the municipal-level labor market results with a worker-level analysis, providing novel micro-level evidence on the long-run impact of troop withdrawals on displaced workers.

This paper examines the economic implications of foreign troop withdrawals. It does not address the various associated political benefits, which may include serving as international security guarantees, providing strategic bases for joint military operations, and generally reinforcing international relations. Moreover, we do not address societal impacts in the hosting regions (e.g. Schindler and Westcott, 2021). From a policy perspective, considering these dimensions is essential for making informed decisions about optimal deployment and hosting decisions.

The remainder of the paper is structured as follows. Section 2 introduces the historical background, discusses the German federal tax-setting structure, and describes 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 (WWII), Germany was split into four occupation zones (*Besatzungszonen*) according to the final 3rd European Advisory Commission zone protocol passed on 26 July 1945. Different from the agreements in the 1st and 2nd EAC zone protocol, France received its own occupation zone in addition to Great Britain, the Soviet Union, and the United States. The formal occupation ended when the Occupation Statute of Germany (*Besatzungsstatut*) was passed in 1949. Although the statute granted Germany conditional sovereignty, the Allies of WWII maintained rights related to control over disarmament, demilitarization, and war reparations. Most importantly, it granted the four countries the right to keep military forces in Germany. These so-called allied control rights (*Alliierte Vorbehaltsrechte*) were finally relinquished in 1991, when Germany gained full sovereignty in the Treaty on the Final Settlement with Respect to Germany (*Zwei-plus-Vier Vertrag*) following reunification. The legal foundation of foreign troop deployments are now 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 (short: SOFA) regarding foreign troops stationed in the Federal Republic of Germany' (SOFA, North Atlantic Treaty Organization, 1959).

The American occupation zone stretched over southwest Germany, including the later 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 troops 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. The figure also shows that the US troop presence in Germany was exceptional in magni-

tude. From 1970 onwards, Germany hosted between four to five times more US troops than all other European countries combined.

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. *Source:* Own depiction.

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

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. *Source:* Own depiction using data from Kane (2006).

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. 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.

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 3: Spatial Distribution of US Installations

Notes: The figure shows the location of US installations with any personnel in 1990.
Source: Own depiction using DOD Base Structure Reports (Department of Defense, 1989).

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 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.

Planning for structural changes commenced as early as July 1988, when the European command of the US Army in Europe (USAREUR) formed a planning group. Using troop ceilings established in the CFE negotiations, USAREUR swiftly 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. The key aspects included ensuring military and operational requirements, decreasing support costs, increasing operational efficiency, minimizing personnel moves, reducing environmental impact, and considering 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 (United States General Accounting Office, 1991b). 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).[4]

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% compared to 1989 levels (see Figure 2). In the withdrawal process, coordination between US and (local) German authorities barely took place. The main reason was the high pace of withdrawals. This meant that local German politicians and communities were taken by surprise when they learned about imminent closures through US forces' public announcements. Cunningham and Klemmer (1995) discuss how the speed of the withdrawals did not provide sufficient time for USAREUR to consult local authorities, such that German public policy stakeholders could not affect the American withdrawal decisions in any meaningful way.

The mid-1990s witnessed a slowdown in the drawdown process due to new safety threats in Europe, such as those arising from the dissolution of Yugoslavia in the Balkans. In recent years, the debate about base closures has gained new momentum, particularly with Donald Trump's emphasis on US troop withdrawals.

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. Three, the overall size of the withdrawal was large, enabling a clean identification of effects.

2.2 Institutional Context

Germany has around 11,000 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. Both taxes are annual taxes. The business tax is levied on the profits of local businesses. Property tax rates are applied to a property's so-called basic value (*Einheitswert*). The basic values were last assessed in the 1960s and

[4]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).

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.[5] (1) Intergovernmental transfers make up about 34% of total municipal revenues.[6] 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. 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.

2.3 Effect Channels

Through which channels did the US troop withdrawal affect German municipalities economically? To determine the mechanisms behind the effects estimated in the empirical part, more background information is needed 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,

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

[6] 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).

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.[7]

The latter regulations clarify that official population counts were not directly affected by the withdrawal of US soldiers. Municipal tax revenues were not directly affected, since the troops were exempt from all sorts of taxation. Only rule-based transfers should be directly affected to a small extent. However, the overall effect on intergovernmental transfers is ambiguous due to the discretionary nature of *Bedarfszuweisungen*, which were likely adjusted in a counteracting way.

Accordingly, the primary channel affecting German municipal finances is through local labor markets. The withdrawal mainly represents a local demand shock. 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. Although a large part of consumption took place on-base, the share of off-base consumption was substantial (28%, Sharp (1990)). The withdrawal could, in principle, also imply a labor supply shock via family dependents working in the local economy. However, this was 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). Family dependents' common inability to speak German and conservative gender ideals usually disabled their 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.[8] As discussed above, other direct economic or fiscal effect channels are muted. This implies that the effects on public finance we discuss in Section 5 work through the labor market for the most part.

From a welfare perspective, it would be interesting to analyze the effects on the housing market as well. However, data to investigate responses on this margin is not available for the analyzed period.

According to the negative withdrawal shock on local labor markets, we suspect the number of employed people 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

[7] A more detailed discussion can be found in Section 6.2 and footnote 21.

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

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.

The effect on intergovernmental transfers is in principle 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 the role of military withdrawals on the fiscal equalization scheme predict that intergovernmental transfers will go down in treated municipalities (Landtag Rheinland-Pfalz, 1996).[9] Therefore, as the three most important revenue categories are expected to be negatively affected by the withdrawal, total revenues should fall.

What are potential reactions of municipalities subject to the withdrawal shock? First, to balance their budgets, municipalities are likely to reduce expenditures in response to the negative revenue shock. Second, municipal tax multipliers are an instrument to increase revenues. Since businesses were supposedly negatively hit by the withdrawal process, increasing the local business tax multiplier would further burden them. The inelastic property tax base makes the property tax multiplier a more stable tool for raising revenues in times of economic distress. In line with the findings in Helm and Stuhler (2024) and Merlo et al. (2023), we thus suspect that the property tax B multiplier increases more strongly than the business tax multiplier.

3 Data

We gather data on three broad categories. These are data used to quantify the US troop withdrawal at the municipal level (3.1), labor market data (3.2) and municipal public finance data (3.3). We supplement the municipal level labor market data with a worker-level dataset of directly affected workers (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 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,

[9] 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 21 for more detailed explanations.

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. We identify the closure dates of the very few remaining cases via web search.[10]

Later sections will discuss the importance of considering deployments of German and other nations' troops in Germany for identification. While no sufficiently precise data is available to quantify troop strengths for other nations besides the US, we collect the geolocations of nearly every military installation in the western states of Germany. Besides the US, Germany itself, Belgium, Canada, Denmark, France, Luxembourg, the Netherlands, Norway, and the United Kingdom had at least one military installation in Germany since 1945 (see Figure A.1). Since other nations' military installations that existed in 1989/90 might have been closed down during the same period as US bases, we exclude municipalities hosting other nations' military installations in 1989/90 in a robustness check (see Section 5.3).

Finally, our empirical strategy exploits the staggered nature of the withdrawal process. To do so, we hand-collect announcement dates of base closures from the *Stars and Stripes* newspaper archive, which is specifically targeted at US soldiers stationed abroad and published within the Department of Defense. We scrape every volume of the period 1988-1998 and pre-select a newspaper whenever keywords related to German US bases appear. We then manually go through each pre-selected PDF, assessing its content, and collect the base closure announcement date in a dataset if applicable. Some municipalities are home to more than one installation and can thus have conflicting announcement dates. In such instances, we set the announcement date to the earliest year found in the *Stars and Stripes*. We use the collected announcement dates as the treatment year in our specifications. This credibly rules out anticipation concerns while at the same time minimizing the time to

[10]The list of other sources used to identify the remaining base closures can be obtained by the authors upon request.

the actual withdrawal.[11]

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 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 the numbers of emigration and immigration. 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

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

variables mostly spans from 1983 to 2022. The data of three states (Bavaria, Baden-Wuerttemberg, Hesse) is fully included in our analysis. Finally, we are thankful to Helm and Stuhler (2024) who early on kindly shared their data with us.

3.4 Worker-Level Data

For our worker-level analysis, we draw on the Integrated Labor Market Biographies (IEB) provided by the IAB.[12] The dataset is based on mandatory notifications to social security insurance. The dataset encompasses the entire population of all individuals in the German labor market who were either employed in a job subject to social security 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.[13] 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.

[12] 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.

[13] 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.

4 Empirical Strategy

Throughout this section, we first describe the treatment group and donor pool definition in subSection 4.1. The descriptive statistics provided in this section highlight why a naive 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 subSection 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. However, in the baseline specification, we rule them out by defining the treatment 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.[14] 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 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). 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. As discussed above, we collect this information from the Stars and Stripes newspaper archive. Municipalities with later announcements might have reacted precautiously when the first withdrawals were announced elsewhere. In a robustness check, we thus demonstrate 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, we further restrict our sample to municipalities with a total population of less than 300,000 during the observation period.[15] We show in the robustness checks that this restriction

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

[15] This restriction excludes the six following municipalities: Frankfurt am Main, Karlsruhe, Mannheim,

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 2010 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. *Source:* Own depiction using DOD Base Structure Reports (Department of Defense, 1989).

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 take a couple of years to materialize after an announcement.

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

Munich, Nuremberg, Stuttgart.

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. *Source:* Own calculations.

4.2 Specification

As discussed previously, the average treated municipality differs from the average donor pool municipality. Accordingly, a naive dynamic difference-in-differences estimation vio-

lates 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 Somarriba, 2025; Dench et al., 2024; Bhalotra et al., 2023; Lang et al., 2023).[16] Compared 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.[17] 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. (2023), which computes the SDID estimator for each treatment timing

[16] The computational implementation of the SDID estimator in Stata is based on the *sdid* command provided by Clarke et al. (2023). 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.

[17] 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.

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 time horizon on the outcome variable level. At least seven pre-periods are available for matching 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. 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 treated with caution. Inference is based on 1000 bootstrap draws.

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.

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 (-0.0876 [0.0001]). 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

Table 2: Baseline and Spillovers Results

	Baseline		Direct Neighbors	15 km
	0-9	10+	0-9	0-9
Labor Market Outcomes				
# Full-time Employed	-0.0876***	-0.0695	0.0127	0.0020
	(0.0221)	(0.0439)	(0.0125)	(0.0078)
Public Finance Outcomes				
Total Revenues	-0.0903***	-0.0663**	-0.0412***	-0.0206***
	(0.0162)	(0.0264)	(0.0106)	(0.0046)
Total Expenditures	-0.0934***	-0.1006***	-0.0313***	-0.0126*
	(0.0162)	(0.0266)	(0.0075)	(0.0073)
Income Tax Revenues	-0.0311***	-0.0310	-0.0135***	-0.0032
	(0.0092)	(0.0235)	(0.0036)	(0.0029)
Business Tax Base	-0.0998***	-0.2434***	0.0012	-0.0223
	(0.0355)	(0.0759)	(0.0229)	(0.0153)
Intergovernmental Transfers	-0.0484	0.1533***	-0.0502***	-0.0048
	(0.0561)	(0.0539)	(0.0160)	(0.0158)
Property Tax B Multiplier	0.0443***	0.1069***	0.0252***	0.0156***
	(0.0163)	(0.0249)	(0.0053)	(0.0025)
Business Tax Multiplier	0.0262***	0.0311***	0.0119***	0.0057***
	(0.0063)	(0.0097)	(0.0027)	(0.0010)
Other Outcomes				
Population	0.0078	0.0197*	0.0058***	0.0028*
	(0.0064)	(0.0111)	(0.0021)	(0.0015)

Notes: The figure shows the SDID results from estimating equation 1 for various outcome variables. All outcomes are in logs. Inference is based on $\geq$ 100 bootstrap draws for *Baseline* and 25 for all other specifications. *Source:* Own calculations.

Figure 5: Number full time employed

(a) Overall (b) Without people working at military bases

Notes: The figure depicts SDID results from estimating equation 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 100 bootstrap draws. *Source:* Own depiction.

long-run effect (-0.0695 [0.1132]). 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, spillover or second-round labor market effects from the direct layoff of German civilians 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 only slightly smaller than 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. This supports our finding that the overall employment effect is mostly driven by the indirect effect, i.e., a decline of full-time employment at non-military establishments.

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

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. *Source:* Own depiction.

(-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 [0.0120] in the long-run, expenditures remain on their initial low level (-0.1006 [0.0002]). 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 [0.0007]. Second, the business tax base shrinks (-0.0998 [0.0049]). Most notably, this effect is persistent and aggravated in the long run. In 10+, the business tax base exhibits a -0.2131 [0.0033] reduction. This indicates that the environment for businesses in treated municipalities has deteriorated substantially and persistently. Part of these negative effects are offset by a long-run increase in the third main revenue source – intergovernmental transfers (0.1533 [0.0045]). This is due to the increased fiscal need from deteriorating economic conditions in the affected municipalities. However, in the short run, intergovernmental transfers also decrease in line with the expectations motivated in Section 2.3 (-0.0484 [0.3879]).

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 [0.0065] 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. The very precisely estimated effect is again persistent. 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 [0.0013]). The smaller increase of the business tax multiplier is in line with findings in other literature (Helm and Stuhler, 2024; Merlo et al., 2023). 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.4a exemplifies for our main outcome variable, total revenues, that, consistent with expectations, the non-staggered specification exhibits a longer lag before effects materialize.

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 virtually identical to the baseline.

Similarly, in column *W/o ZRG*, we exclude all municipalities belonging to the former *Zonenrandgebiet*, as these municipalities lost 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.3. The results remain qualitatively identical and quantitatively largely unaltered.

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 Northrhine-Westphalia – another German state – in a robustness check. The results are listed in column *Including NRW*. Reassuringly, the results remain very similar.

One concern could be that large municipalities drive our results. Therefore, in column *Pop* $\geq$ *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.

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 section 4.2). To do so, we include five-year pre-treatment outcome variable growth quintile 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 support the chosen estimation strategy and the cleanness of identification in our setting. For almost all outcomes, results remain very similar across all specifications in the table.

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 the 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, the 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 yellow 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 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 also due to restricting our sample to only Bavaria, no systematic trend break is visible in the pre-period. Again, the sharp drop at the start of the post-period and the similar size of measured effects in the post-period across both specifications are reassuring.

Table 3: Robustness

Outcome (log)	Baseline 0-9	T-year 1990	W/o Oth. Nations	W/o ZRG	Including NRW	Pop < 200,000	Small Withdrawal	TWFE
Labor Market Outcomes								
# Full-time Employed	-0.0876***	-0.1050***	-0.0886***	-0.0967***	-0.0641***	-0.0856***	-0.0337	-0.0716***
	(0.0221)	(0.0145)	(0.0321)	(0.0190)	(0.0249)	(0.0204)	(0.0525)	(0.0251)
Public Finance Outcomes								
Total Revenues	-0.0903***	-0.0852***	-0.0893***	-0.0916***	-0.0762***	-0.0913***	-0.0218	-0.0802***
	(0.0162)	(0.0216)	(0.0220)	(0.0133)	(0.0127)	(0.0202)	(0.0355)	(0.0185)
Total Expenditures	-0.0934***	-0.0833***	-0.0929***	-0.0927***	-0.0828***	-0.0948***	-0.0100	-0.0896***
	(0.0162)	(0.0213)	(0.0254)	(0.0149)	(0.0120)	(0.0196)	(0.0408)	(0.0215)
Income Tax Revenues	-0.0311***	-0.0373***	-0.0337***	-0.0293***	-0.0291***	-0.0312***	-0.0082	-0.0480***
	(0.0092)	(0.0058)	(0.0111)	(0.0092)	(0.0078)	(0.0102)	(0.0238)	(0.0086)
Business Tax Base	-0.0998***	-0.0877**	-0.0961***	-0.1076***	-0.0910**	-0.0984**	-0.0750	-0.0802
	(0.0355)	(0.0360)	(0.0360)	(0.0383)	(0.0365)	(0.0410)	(0.0856)	(0.0639)
Intergovernmental Transfers	-0.0484	-0.0017	-0.0921*	-0.0412	-0.0187	-0.0502	0.1032	-0.0332
	(0.0561)	(0.0508)	(0.0556)	(0.0574)	(0.0504)	(0.0582)	(0.0814)	(0.0643)
Property Tax B Multiplier	0.0443***	0.0433***	0.0433***	0.0481***	0.0354**	0.0491**	0.0344*	0.0429***
	(0.0163)	(0.0123)	(0.0128)	(0.0181)	(0.0177)	(0.0224)	(0.0199)	(0.0150)
Business Tax Multiplier	0.0262***	0.0280***	0.0236***	0.0241***	0.0209***	0.0262***	0.0271**	0.0221***
	(0.0063)	(0.0069)	(0.0056)	(0.0061)	(0.0064)	(0.0061)	(0.0120)	(0.0059)
Other Outcomes								
Population	0.0078	0.0104*	0.0079	0.0014	0.0076	0.0088	-0.0113**	0.0027
	(0.0064)	(0.0062)	(0.0064)	(0.0063)	(0.0048)	(0.0062)	(0.0057)	(0.0067)

Figure 7: Time Placebos

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

Notes: The figure shows time placebos for two outcome variables. The yellow 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. 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. *Source:* Own depiction.

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. 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*). 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 9. 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 8, 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: Spatial Spillovers

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

Notes: The figure shows SDID results for the baseline treated (blue), direct neighbors (yellow), and municipalities within a 15 km radius around baseline treated municipalities (cyan). The shaded areas are 95% confidence intervals. *Source:* Own depiction.

Figure 9: 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 yellow. 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. *Source:* Own depiction using DOD Base Structure Reports (Department of Defense, 1989).

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 8a even hint at initially slightly positive labor market effects in directly neighboring municipalities. Most likely, some of the workers losing their jobs in the baseline treated will quickly find work 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 8b 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, as listed in Table 2.

Columns *High Population* and *Low Population* divide the baseline treatment group into large and small municipalities according to their 1989 population size. Although the withdrawal size is correlated with population size, the results are informative. They show that especially small municipalities are hit hard by the withdrawal. The number of full-time employed decreases sharply by a highly significant -0.1217 [0.0000]. Similarly, total revenues and expenditures decline much more strongly in smaller municipalities (-0.1158 [0.0000], -0.1017 [0.0000]). Different from the results in Helm and Stuhler (2024), it is the smaller municipalities that react more strongly using their discretionary tax-setting power. Again, a salience argument might explain this: while large municipalities were aware of Census population count adjustments in the context of Helm and Stuhler (2024), the opposite might be true in the troop withdrawal setting, where US troop presence has likely been most salient in smaller municipalities.

5.6 Policy Implications

To link our findings to the existing literature and to make our results intuitive for policymakers, 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:[18]

(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 its corresponding aggregate pre-withdrawal number of full-time workers. Finally, we sum up the three resulting estimates.

(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.

[18] Detailed explanations of the calculation steps can be found in the appendix section D.

(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 its corresponding aggregate pre-withdrawal level of total revenues. Finally, we sum up the three resulting estimates.

(4) Aggregate Number of Workers Directly Laid Off by the US Military Using IAB data, we calculate the number of German workers directly laid off by the US military (variable *Stationierungsstreiträfte*) for each municipality hosting US troops. We sum up these figures for all municipalities within a 15 km radius 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.

To compare our estimates to the ones found elsewhere, we collect comparable statistics from various fields of literature and depict them jointly in Figure 10. This enables an intuitive assessment of the size of the impacts from foreign troop withdrawals vis-à-vis other settings and types of policies.

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.

We compare our multiplier to estimates in the related literature on place-based policies in Figure 10a. When compared to targeted place-based policies, e.g., in Siegloch et al. (2025), who find a cost per job of at most USD 23,000, it is evident that troop deployments are a rather ineffective way for creating local jobs. The cost per created job in our setting is almost four times higher at USD 85,200. 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).

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

Figure 10: Literature Comparisons

(a) Spending per Job

(b) Jobs per Job

Notes: The figure collects 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). *Source:* Own depiction using various sources.

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.[19] To calculate the multiplier, we divide (3) aggregate revenues lost by (2) the aggregate military spending reduction.

We find that municipal revenues decrease by USD 0.28 per USD 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.

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

[19] 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.

around our baseline treated municipalities decreases by 0.53. In Figure 10b 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. 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.

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*.[20] Figure A.2 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 (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, 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

[20] 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.

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 10km 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 11210 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.

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_i, respectively.

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	

Note: 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.

ϕ_k captures observation year relative to baseline year fixed effects. Finally, $\mathbf{X}_{it}$ contains individual, time-varying characteristics, i.e., 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 11 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 a civil servant. 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 11 focus on the effect on annual labor earnings and daily wages. Both variables are measured in 1990 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 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.5 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.6), age (Figure A.7), and the initial labor market conditions (Figure A.8). 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.6 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 around 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. 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.

Figure A.7 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.8 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.

6 Discussion

6.1 Reconversion Efforts

The results of this paper show that the withdrawal of US soldiers had a negative impact on the employment of the affected regions and the public finances of the municipalities. These negative consequences persist over time. One limitation of this paper is that we did not explore what happened to the properties and terrain occupied by the US Army after it left 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. 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.9 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.

6.2 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 needs.[21] This implies that the

[21] 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

Figure 11: 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

Note: 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 1990 prices.

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 empirical evidence to support this discussion, clearly demonstrating that the presence of foreign troops enhances prosperity in the host regions. Therefore, *Schlüsselzuweisungen* should not be designed in a way that acknowledges a net negative fiscal impact of foreign troop deployments. 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.

6.3 External Validity

Regarding US troops, Germany represents a major host nation with decades of U.S. military presence, extensive infrastructure, and strong integration into NATO. Lessons about the economic impact on local communities may therefore generalize to other long-standing basing arrangements in allied countries. Therefore, our results can serve as a valuable benchmark for similar countries. At the same time, several limits to external validity should be acknowledged. Germany's economic strength, democratic stability, and central role in European security are unique; findings may not directly apply to smaller or less prosperous host nations. The Cold War legacy and deep institutional ties to NATO also set Germany apart from more recent basing arrangements in Asia or the Middle East. Finally, political sensitivities surrounding sovereignty, public opinion, and strategic priorities vary widely. Thus, while the German case provides valuable insights, translating results to other settings requires careful attention to contextual differences in economics, politics, and security alliances.

7 Conclusion

The local economic impacts of foreign troop deployments are a highly relevant issue from both economic and geopolitical perspectives, yet empirical evidence is scarce. Such evidence is particularly important to guide the often heated policy debate on this topic.

increase was 50% (Kirchhoff, 1996; Landtag Rheinland-Pfalz, 1996).

Our study focuses on the aftermath of the historic large-scale US troop withdrawal from Germany after the end of the Cold War. The withdrawal, prompted by sudden geopolitical shifts following the fall of the Iron Curtain, offers a credible exogenous setting to gain a deeper understanding of the economic effects associated with foreign troop presence.

Deploying synthetic control methods (Arkhangelsky et al., 2021) and detailed, administrative data on troop strengths at the municipal level, we analyze the effects on local labor markets and municipal finances. We observe a substantial reduction in employment and municipal revenues and expenditures. Specifically, we find a -8.8% effect on the number of full-time employed in affected municipalities in the first ten years after the withdrawal. The employment results stay unchanged when controlling for the direct impact of base closures on employment numbers, i.e., excluding workers who were employed at a US military base. In line with other literature, we find that wages remain largely unaffected by the withdrawal. The municipal-level estimates are supplemented by worker-level evidence. We find that the employment probability of displaced workers decreases by 50 percentage points in the year of layoff. Fifteen years after displacement, affected workers are still around 20 percentage points less likely to be employed than workers in a matched sample. Daily wages of displaced workers are 9.2 percent lower than those of control workers fifteen years after displacement.

Furthermore, our analysis uncovers profound implications for municipal finances. Treated municipalities experience a -9.0% reduction in revenues, to which they react by decreasing expenditures by -9.3%. The decline in revenues stems primarily from a reduction in business tax revenues. In the long-run, treated municipalities face a log business tax revenues reduction of -0.2131 log points which is partially offset by increased intergovernmental transfers (0.1533). Municipalities also leverage their discretionary tax-setting power, evident in a highly significant 10.7% increase in the property tax B multiplier and a more minor but highly significant hike in the business tax multiplier (3.1%).

In this paper, we quantify the local economic impacts of foreign troop deployments in a historically interesting setting. The estimated effects are valuable as ballpark figures for local policy makers facing a withdrawal of foreign troops elsewhere. Such policy guidance is necessary, as our analysis reveals that the adverse effects on employment and municipal revenues are substantial and long-lasting. Moreover, our spillover analyses show that whole regions can be affected. This suggests that troop deployments can be understood as place-based policies. Accordingly, other place-based policies could also be a helpful tool to smooth the impact of troop withdrawals. From a scientific perspective, our analysis offers a clean identification of a sizable negative economic shock at the fine-grained jurisdictional level of 11,000 German municipalities. The absorbing state of treatment in our setting

enables a particularly valuable focus on long-run dynamics of fiscal adjustments. Our results align with findings from other research on local economic shocks, while the forensic analysis of effect channels provides a nuanced understanding of the repercussions of a specific type of economic shock – troop withdrawals.

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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. *Source:* Own depiction, data hand collected using various sources.

Figure A.2: Development of stationed troops' military bases (sector 921)

(a) Employment

(b) Establishments

Note: The left panel illustrates the development of employment at military bases of stationed troops between 1975 and 2020. The left panel illustrates the development 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 *Hessia*, *Rhineland-Palatine*, *Hessia*, *Baden-Württemberg*, and *Bavaria*, i.e., states used as part of our estimation sample. The green 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.3: Ehrlich and Seidel (2018) Zonenrandgebiet

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). *Source:* Own depiction.

Figure A.4: Staggered Baseline vs. Non-Staggered

(a) Total Revenues

Notes: The figure shows SDID results from estimating equation 1 and various other specification. All outcomes are in logs. *Source:* Own depiction.

Figure A.5: Effect of displacement: further outcomes

(a) Employment in other occupation

(b) Other municipality

(c) Other county

(d) Other *Regierungsbezirk*

(e) Other state

Note: 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.6: 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

Note: 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 1990 prices.

Figure A.7: 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

Note: 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 1990 prices.

Figure A.8: 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

Note: 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 1990 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.9: 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

(2) Aggregate Military Spending Reduction We then US aggregate military spending data for Germany from Department of Defense (1990, 1998) and calculate its aggregate reduction between 1989 and 1997.[22] We then calculate the share of spending accruing to our treated municipalities according to their share of total military and civilian personnel in the respective years (1989, 1997). Given the aggregate nature of our multiplier, this measure of spending reduction in the treated municipalities is likely precise enough. Dividing (1) by (2) delivers our spending-per-job multiplier.

(3) Aggregate Revenues Lost XXX

(4) Aggregate Number of Workers Directly Laid Off by the US Military We validate this figure using information from Cunningham and Klemmer (1995) who estimate that 71,000 German workers would be laid off by the US military. We allocate the respective share of this number to our baseline treated municipalities according to their share of total civilian personnel. We calculate the latter number using information from the Department of Defense Base Structure Reports.

(5) Aggregate Number of Withdrawn Soldiers XXX

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

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.[23] 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.

[23] 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.