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Jan Peter aus dem Moore*
Alexandra Spitz-Oener*

* Humboldt-Universität zu Berlin, Germany

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Bye Bye, G.I. - The Impact of the U.S. Military Drawdown on Local German Labor Markets*

Jan Peter aus dem Moore[1] and Alexandra Spitz-Oener[2]

[1]Humboldt-Universität zu Berlin
[2]Humboldt-Universität zu Berlin, IAB, CASE, IZA

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Abstract

What is the impact of a local negative demand shock on local labor markets? We exploit the unique natural experiment provided by the drawdown of U.S. military forces in West Germany after the end of the Cold War to investigate this question. We find persistent negative effects of the reduction in the U.S. forces on private sector employment, with considerable heterogeneity in terms of age and education groups, and sectors. In addition, the U.S. forces reduction resulted in a rise in local unemployment, whereas migration patterns and wages were not affected.

Key Words: Labor demand shock; Base closure; Employment; Wages

JEL Classification: J23, R23

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1 Introduction

The impact of economic shocks on local labor markets is a subject of long-standing interest to economists, policy makers and the general public alike. In particular, the nature and magnitude of potential local consequences of economic shocks are important for the justification and design of regional economic policies. In many countries, considerable resources have been devoted to helping regions mitigate and overcome past adverse economic shocks or to attracting new investment in the hope of positive local externalities. Despite this interest, empirical research has had difficulty establishing the causal effects of local economic shocks.[1]

In this paper we identify the causal effect of a local economic shock by taking advantage of a shock that induced large exogenous shifts in labor demand in several districts of four states ("Bundesländer") in West Germany, but not in others. Specifically, we exploit the district variation in the stationing and withdrawal of U.S. military forces in Germany after German reunification and the end of the Cold War to examine the consequences of regional economic shocks on local private sector employment. In addition, we also investigate the impact of this labor demand shock on wages, unemployment and migration.

The unique natural experiment setting of the event allows us to improve on limitations that impaired previous studies analyzing the effect of regional economic shocks on local labor markets.[2] The U.S. forces were stationed in West Germany in the 1950 at strategic points along two major defense lines; local economic considerations were not important in this decision process. In addition, and in a similar fashion to the stationing decision, the withdrawal decisions for the U.S. forces in Germany were made exclusively by U.S. military officials and were neither subject nor responsive to any politicizing: the U.S. Department of Defense decided on the details of the withdrawal process purely on strategic military grounds. Both of these facts alleviate concerns regarding the validity of exogeneity assumptions.

The U.S. forces affected the German local economies through three main channels: firstly, the bases demanded goods and services from German companies; secondly, the U.S. soldiers, civilian employees and their dependents were consumers in the local economies; and thirdly, the

[1] See Moretti (2011) for a recent review.

[2] It also allows us to improve on limitations that previous studies on the effects of base closures faced. For example, the Base Realignment and Closure Process (BRAC) in the U.S. and the realignment of the German Army are both likely to be influenced by strong local or regional stakeholders lobbying to safeguard their bases against closures.

bases acted as employers of German civilian workers.

Although German civilian employees typically comprised a small fraction of local employment (the median is less than 0.5 percent of local employment in districts with U.S. military presence in 1989), many of the early studies were preoccupied with the fate of these German employees.[3] This focus reflects in part the overriding public and political interest at the time and the fact that the legal status of the local national employees was then unclear.[4]

We focus on effects that are mainly driven by changes in channels one and two: at the end of the 1980s, there were about 250,000 U.S. servicemen stationed in Germany (see Figure 1). Together with the U.S. civilians whom the bases employed and the family members they all brought along, the total U.S. presence in West Germany amounted to nearly 600,000 persons in 1989 (see Figure 2). At the district level where the U.S. bases were located, the U.S. presence was sometimes large; for the 86 districts with a U.S. presence retained in our baseline analysis, the mean of the U.S. force level in 1990 was 3,707, which represents 2.9 percent of the district population.[5,6] The U.S. forces consumed mainly local, non-traded goods and services from German sources – with respect to traded goods and services, the U.S. bases were mostly self-sufficient. Overall, the withdrawal process represented a large consumption shock to the affected regions which translated into a negative shock to local labor demand.

The results indicate that the realignment of the U.S. forces did indeed have significant negative effects on local private sector employment in Germany. On average, our coefficient estimates suggest that the complete withdrawal in a given district is associated with a 0.4-0.7 percent year-by-year drop in the number of jobs in the local private sector. An analysis of the dynamic pattern

[3] See, for example, Blien et al. (1992), Blien (1993), and Gettmann (1993).

[4] See, for example, the official information requests by members of the German parliament (Deutscher Bundestag, 1990a,b,c, 1991a,b) and the report by the U.S. General Accounting Office (1992) on the process of reducing the local national workforce.

[5] See Table 1. U.S. military deployments abroad of comparable size have only recently been built up in Afghanistan and Iraq, with the peak of the force levels totaling 42,500 (about 0.1 percent of the local national population) in Afghanistan and 251,100 (about 0.8 percent) in Iraq in 2009 (for the force level data, see Belasco (2009); the population data for the relative importance of the deployments has been sourced from the Central Intelligence Agency (2011)). A new report for the U.S. Senate Foreign Relations Committee (U.S. Congress, 2011) discusses whether Afghanistan might be infected by a "Dutch Disease", i.e. an overdependence of local employment on foreign aid connected to the foreign troop presence that might vanish into thin air in the case of a swift withdrawal. We do not argue that our estimates for the drawdown in Germany could be extrapolated to these cases as the circumstances of the deployments, the level of development of the countries, and the base structures and relationships with the local economy are vastly different.

[6] Bebermeyer and Thimann (1990) attempt to assess the aggregate economic importance of the US stationing in West Germany using a cost-benefit balance sheet accounting approach. Combining various data sources from 1986 and 1987, they calculate an annual gross benefit of 14.8 billion German DM and a net benefit (subtracting cost items that are largely borne by the German federal budget) of 12.5 billion DM, which is equal to 0.62 per cent of the West German gross national product at the time.

reveals that this adverse effect is persistent and does not fade away even several years after the withdrawal shock first hits. In line with the specifics of the consumption shock on which we are focusing, the employment effects are most pronounced for local goods and services sectors that were prone to suffer most from the reduction of local purchasing power, and were primarily borne by young and old, and by low- to medium-skilled workers. We also find evidence of a rise in local unemployment, whereas we do not find effects along the migration margin or in terms of local wages.

This study advances the literature on the consequences of economic shocks on local labor markets. The traditional approach in the literature uses deviations in regional time series of employment from national averages to investigate the consequences of economic shocks (for example, Topel, 1986, Decressin and Fatas, 1995) and Blanchard et al. (1992) in part of their analysis). Employment, however, is determined by both labor demand and supply forces, and these studies are not able to identify the effects of these two factors separately. Another prominent approach in this area of the literature is to identify local economic shocks by using national changes in industry employment interacted with measures of local industrial composition (see, for example, Bartik, 1991, Blanchard et al., 1992, Bound and Holzer, 2000, Moretti, 2010, and Notowidigdo, 2011). While this instrument is likely to be exogenous to local labor supply, it is not clear whether it captures shocks to local labor demand very well. It is possible, for example, for a region to lose employment in an industry even though that industry is growing on the national level.

Carrington (1996) was among the first studies to examine a specific shock, namely the construction of the Trans-Alaska Pipeline System (TAPS) between 1974 and 1977. He analyzes how this construction project affected the Alaskan labor market and finds that the timing of the evolution of aggregate monthly earnings and employment closely matched changes in TAPS-related activities. Overall, however, the findings suggest that this major demand shock had only short-term consequences for the Alaskan labor market.

Other studies involving specific shocks use variation in energy prices to analyze the impact of labor demand shocks on local labor markets. Black et al. (2005a), for example, analyze the impact of the coal boom and bust in the 1970s and 1980s on local labor markets and find positive effects of the boom on local non-mining sector employment and earnings, in particular in non-mining sectors producing local goods.[7] The coal bust led to negative effects that are smaller than the positive effects during the boom.[8] They also find that the regions affected by the coal bust experienced considerable population losses, whereas population growth was barely affected by the coal boom. Studies that identify shocks through price fluctuations in natural resources such as coal or oil focus on price changes of input factors that are widely used throughout the economy; it is unlikely that these price fluctuations only impact on the energy-extracting industries and do not have repercussions on both non-energy-extracting industries within the treatment regions and industries outside these regions, in particular as prices of different energy resources are highly correlated.

A common feature of the analysis by Carrington (1996) and the studies focusing on energy price fluctuations is that they analyze shocks to very isolated or mainly rural regions,[9] and some of the results might be explained by these idiosyncrasies (e.g. that population growth in the resource-rich regions was not affected by the coal boom in Black et al., 2005a). None of the districts in the four West German states under consideration are as rural or remote as the regions of main interest in these earlier studies.[10]

Our analysis also differs from these earlier studies with respect to the time frame. In the setting of all these studies, the economic actors should have been aware of the fact that the shocks were not permanent. We focus instead on a permanent and irretrievable shock that should have been perceived as such by the economic agents. In this respect, our study is similar to Greenstone and Moretti (2003) and Greenstone et al. (2010), who study the regional industry-level employment and productivy effects from the awarding of "Million Dollar Plants". While their analysis provides an original identification design for regional spill-over effects by focussing on the different evolutions in "winner" and "loser" counties in competitive biddings for large industrial plants, an important limitation of their data is that it does not provide information on the expected size of the plant, which is likely to be important for the magnitude of the potential

[7]For a similar analysis using Canadian data see Marchand (2011).

[8]In other papers, these authors investigate how the coal boom and bust affected other outcome variables such as education or participation in disability programs (Black et al., 2002, 2003, 2005b).

[9]Alaska is obviously very remote; but Kentucky, Ohio, Pennsylvania and West Virginia, the states in which treatment and control districts are located for the analysis by Black et al. (2005a), are also rural areas with population density ranging from 30 - 110 inhabitants per square kilometer.

[10]Several studies consider other arguments as to why adjustments to economic shocks might play out differently; factors discussed include relative skill supply, the enterprise ownership structure or the housing supply in the affected regions. See, for example, Bound and Holzer (2000), Glaeser and Gyourko (2005), Kolko and Neumark (2010), Notowidigdo (2011), and Larson (2011).

spill-over effects into employment and welfare. The U.S. withdrawal process on which we focus is a well-defined shock because the data at hand allow us to measure precisely the size and structure of the shock for all treatment areas.

The paper is structured as follows: the next section provides a brief account of the historical background of the stationing and withdrawal of the U.S. military forces in Germany. In Section 3 we present our estimation strategy, and Section 4 discusses the data. Section 5 reports our results, separately for regional employment, wages, unemployment and net migration, as well as several robustness checks. Section 6 provides a conclusion.

2 Historical Background

2.1 U.S. Forces in Germany 1945-1990

After the end of World War II, the Allied Forces (American, British, French and Soviet) established four occupation zones in Germany. Following negotiations that had already started during the war in 1944 within the allied *European Advisory Council* (EAC) and that had been agreed upon in principle at the Yalta conference, the final demarcations of the 4 zones were confirmed by the Potsdam Agreement on August 2, 1945.

The American zone included a large part of the southwest area of Germany (which was later to become the states of Bavaria, Hesse, and the northern part of Baden-Württemberg) plus the seaport town of Bremerhaven on the North Sea and the American Sector in Berlin. In Article 12 of the "Berlin Declaration" issued on June 5, 1945, the Allied Powers granted themselves the authority "to station forces and civil agencies in any or all parts of Germany as they may determine" (U.S. Department of State, 1985). However, there were initially no plans for a major permanent military presence. With the burgeoning confrontation of the Cold War marked by the establishment of two states on German soil in 1949, the Berlin blockade and airlift, and the war in Korea, the Western Allied Powers established NATO (which the West German Federal Republic of Germany (FRG) joined in 1955) as a common defense organization and deterrent against potential Soviet aggression. The NATO "Forward Strategy" foresaw the West German area as the central battlefield where a potential Soviet invasion would have to be halted until additional forces could be activated. Against the backdrop of this concept, the U.S. forces in Germany established bases at strategic points along two major lines of defense, expanding their presence also beyond the early boundaries of the American zone.[11]

An estimated number of 1.9 million American soldiers were stationed on German soil at the end of World War II.[12] After the temporary reduction to less than 100,000 in the first years of the occupation up to 1950, the strength of the U.S. forces was consolidated to around 250,000 in the mid 1950s. Figure 1 tracks the historical evolution of the U.S. active military personnel in Germany from 1950 to 2005. Apart from some temporary build-ups and reductions, for example after the Berlin and Cuban Missile crises in the early 1960s and later due to the Vietnam War, the level of the American military presence remained more or less stable until 1989, making it one of the largest and longest peace-time deployments of an army in a foreign country in modern history.[13] The overall U.S. presence, including the employed civilian personnel and dependents was even more significant, totaling more than 570,000 in the spring of 1989 (see Figure 2). The U.S. forces in Germany maintained over 800 bases and installations, ranging from small unmanned signal posts to training areas covering more than 20,000 hectares or airbases that employed more than 12,000 personnel. The left part of Figure 3 illustrates the regional distribution and the relative personnel size of the U.S. bases across Germany in 1990.

2.2 The Withdrawal and Realignment of U.S. Forces after 1990

The end of the Cold War created a turning point for the U.S. presence in Germany. In March 1989, the NATO countries and their counterparts from the Warsaw Pact began negotiations on reductions of conventional armed forces in Europe. The fall of the Berlin wall on November 9, 1989 and the swift political transformations in several Eastern European states further sped up the negotiations, and just one month after the formal reunification of Germany, the Conventional Armed Forces in Europe (CFE) treaty was signed in November 1990.

Several official U.S. government reports (U.S. General Accounting Office, 1991a,b) and the comprehensive study conducted in 1995 by the *Bonn International Centre for Conversion* (BICC) (Cunningham and Klemmer, 1995) provide detailed insights into the planning and exe-

[11] Several large airbases were constructed, for example, in Rhineland-Palatinate in the former French occupation zone west of the Rhine considered to be less vulnerable to a Soviet attack. For a brief account of the history of U.S. forces in Germany, see for example Duke (1989), pp. 56-148. For details on the U.S. base planning in Rhineland-Palatinate, see van Sweringen (1995).

[12] See Frederiksen (1953) and Trauschweizer (2006).

[13] The numbers in Figure 1 also reveal the distribution between the different branches of the U.S. armed forces, with the Army constituting 70-85 percent, the Air Force 10-30 percent and the Navy and Marine Corps less than 1 percent of the total deployment at any point in time.

cution of the U.S. drawdown process. In preparation for some of the structural changes that were to materialize in the future, the European command of the U.S. Army in Europe (USAREUR) had formed a planning group as early as July 1988. Based on the troop ceilings established in the CFE negotiations, the USAREUR command was quick to draw up a plan to reconfigure the required force levels and identify units for withdrawal and bases and communities for closure. The key criteria for the selection of sites by U.S. military officials were

(i) "ensuring that the forces would meet military and operational requirements;
(ii) decreasing support costs and increasing efficiency of base operations;
(iii) minimizing personnel moves;
(iv) reducing environmental impact; and
(iv) considering the proximity of training areas, the quality of housing and facilities, the local political and military environment, the concerns of host nations, and the base's proximity to road and rail networks."[14]

On September 18, 1990, the Pentagon publicly announced the closure and realignment of 110 sites in Germany, starting the first phase of the withdrawal.[15] By 1996, another 20 rounds of base closures in Germany would be gradually announced, bringing the number of U.S. military personnel at the end of the 1996 fiscal year to a low point of around 85,000, a massive 75 percent reduction compared to the 1989 level. Although the official documents and newspaper accounts from the time mention some coordination between U.S. and German authorities, they also highlight the fact that the local German politicians and communities were usually taken by surprise and learned about the imminent closures only around the time of the public announcements by the U.S. forces in the news media.[16,17] The "drawdown shock" at the local level was further

[14]U.S. General Accounting Office (1991b), p.3.

[15]Earlier, U.S. Defense Secretary Dick Cheney had already announced the closure of Zweibrücken air base in Germany on January 29, 1990, as part of a round of mostly domestic base closures within the 1991 fiscal year defense budget (Doke, 1990; Vynch, 1990).

[16]Cunningham and Klemmer (1995) describe how "the US Department of Defense maintains complete authority" which "has to a large extent de-politicized the foreign base closure process" compared to the domestic BRAC process. They report that even for Rhineland-Palatinate, where the state authorities specifically requested that "the United States close primarily installations in densely populated and highly industrialized urban areas (...), but keep open the sites located in rural and underdeveloped areas of the state", these priorities were "inconsequential" due to the increasing pace of the withdrawal. They conclude as follows: "In none of the cases reviewed were the German civil authorities able to stop or reverse the US decision to withdraw. In some limited cases (...) German officials were able to delay closure. Conversely, some high-level requests to delay closure were denied."

[17]We conducted an extensive newspaper archive search of both U.S./international and German newspapers (including, but not limited to major titles such as *Business Week*, *The Economist*, *The Wall Street Journal*, *Der SPIEGEL*, *Frankfurter Allgemeine Zeitung (FAZ)*, *Süddeutsche Zeitung (SZ)*, *Handelsblatt* as well as *Stars*

exacerbated by the short time frame of 180 days that the U.S. forces envisaged between the announcements and the completion of the withdrawals with the return of the vacated sites to German authorities.

With the U.S. troop levels reaching new target levels of around 90,000 and new safety threats emerging in Europe (for example in the Balkans after the dissolution of Yugoslavia), the pace of the drawdown process slowed down considerably in the mid-1990s. Only after the terrorist attacks of 2001 that resulted in a comprehensive redesign of U.S. security policy, including changes in overseas basing, were new rounds of major U.S. base closures in Germany announced and implemented. This process is still underway: in summer 2010, USAREUR announced a major withdrawal by 2015 from the Heidelberg and Mannheim area, a former stronghold and location of the headquarters of the U.S. Army in Germany.[18]

In summary, three features of the stationing and drawdown process deserve highlighting, as they lay the foundation for the identification strategy in the empirical analysis. Firstly, both the designation of the initial U.S. base locations after the occupation, but even more importantly, the base closure and realignment decisions half a century later, were governed unequivocally by American strategic military considerations. Secondly, local withdrawals of the U.S. forces constituted abrupt "socks" with a surprise element for agents in the local economy. Thirdly, the magnitude of the withdrawal process was large and exhibited strong variation in size and timing at the local level.

3 Empirical Approach and Identification

We identify the causal effect of the U.S. withdrawal on local labor markets by estimating difference-in-difference (DD) models, contrasting the evolution of employment and wages in districts with a U.S. presence and subsequent withdrawal and a group of control districts. In our simplest specification for employment, the empirical model estimated by OLS has the following

& Stripes, the major news outlet for the U.S. military community) for the years 1988 to 2009, either via the news archives of the respective media and/or comprehensive databases such as *Factiva* and *Genios*. Based on alternative search keywords such as "U.S. Army", "U.S. Forces", "bases", "closures", "realignment" and "Germany", the articles that we found in all cases relayed (if any) specific information about the locations, extent and timing of drawdown decisions only after the fact, i.e. after the information had already been officially disclosed by the U.S. Department of Defense and/or the U.S. forces in Germany. A bibliography of all the articles found is available from the authors upon request.

[18]The latest piece of information in this respect appeared in the New York Times on January 12, 2012, announcing that the U.S. will withdraw another brigade (about 4,000 soldiers) from Germany, as the new military strategy focuses on the Asia-Pacific region and on sustaining a strong presence in the Middle East.

form:

$$(1) \qquad logY_{kt} = \alpha_k + \delta_t + \beta \times log\text{U.S. Forces}_{kt} \times \mathbb{1}\left[t > Year_{0k}\right]) + \epsilon_{kt}$$

The dependent variables are district-level measures of employment, denoted by Y, in district k and year t. The parameter of interest, β, is the coefficient on the logarithm of the level of U.S. forces in the given district k in year t, and an indicator function for the post-treatment period that varies according to $Year_{0k}$, the year of the first announcement of a U.S. withdrawal in a given treatment district. All estimates include a vector of district dummies, α_k, that control for mean differences in employment across districts, and year dummies, δ_t, that adjust for employment growth common to all districts. So β reflects the extent to which employment growth in a district varies with the extent of the U.S. forces reduction.

In extensions to the specification of Equation 1, we estimate specifications that include dummies for state-by-year, and linear or quadratic district-specific time trends in order to allow for deviations from the common trend assumption. In the latter, the identification of the effects of U.S. withdrawal comes from whether the withdrawal lead to deviations from preexisting district-specific trends.[19]

For the analysis of the wage outcomes that vary at the individual level, we augment specification (1) with covariates that control for individual characteristics:

$$(2) \qquad logW_{ikt} = \alpha_k + \delta_t + \beta \times log\text{U.S. Forces}_{kt} \times \mathbb{1}\left[t > Year_{0k}\right]) + X_{ikt}\gamma + \epsilon_{ikt},$$

where the subscript i denotes the individual observations. The vector of individual controls, X_{ikt}, includes a quartic in age and dummies for foreign citizenship, occupations, and industries.

In order to capture potentially heterogeneous treatment effects according to industry, we perform both “pooled” estimations across all industries and separate estimations using industry-district samples. Again, all models are estimated in extensions that include state-by-year dum-

[19]In our baseline estimations, we do not weight the district-year observations in any way for two reasons: firstly, as the employment data is summarized from the full universe of establishments, there is no systematically heteroscedastic measurement error that varies with the district size. Secondly, since we are interested in the average "treatment" effect of the U.S. withdrawal in a district, there is no specific reason to place more weight on large districts. See Autor (2003) who puts forward these arguments in his analysis of the effect of exceptions to the common dismissal law on temporary help service employment growth in U.S. states. Moreover, if we do weight the observations by district population, the results - as reported in one of the later robustness checks and available in detail from the authors upon request - are virtually unchanged from the unweighted results.

mies and linear or quadratic time trends.

In the recent applied econometrics literature, two potential problems for the consistent estimation and inference in DD models have received considerable attention. Firstly, Bertrand et al. (2004) show that the inference based on the standard treatment of clustered errors can be misleading in the presence of serial correlation. They demonstrate that next to more complex approaches such as block bootstrap methods[20], the bias in the standard errors can be reduced to viable levels by clustering at the group level if the number of groups is sufficiently large for asymptotic theory to hold. Secondly, following up on seminal contributions by Moulton (1986, 1990), Donald and Lang (2007) report that the standard methods for dealing with a DD model that mixes individual and group-level data and where the regressor of interest varies only at the group level also suffer from severely downward-biased standard errors in the presence of intra-group correlations. In our context, we address these concerns by following the recommendations by Angrist and Pischke (2009, chap. 8): in our baseline employment and wage estimations, we use Huber-White robust standard errors clustered at the district level to allow for arbitrary forms of correlation within districts and rely on the large number of districts and time periods in our setting.[21] For our wage estimations, where we face the "Moulton" problem, we further confirm the robustness of our results by implementing a two-step estimation procedure as proposed by Donald and Lang (2007) that first aggregates on the group level and then performs the DD estimation on covariate-adjusted district averages.[22] Finally, in some of our robustness checks, we also show that our results are robust if we implement an alternative two-way clustering method recently suggested by Cameron et al. (2011) or cluster standard errors at the higher aggregation level of labor market regions to allow for spatial autocorrelations across districts within the same labor market region.

4 Data and Descriptive Evidence

We consider employment and wage outcomes from 1975 to 2002 for 202 districts (NUTS-3, "Landkreise und kreisfreie Städte") that are located in the four German federal states of Hesse,

[20]See, for example, Fitzenberger (1998), Conley (1999).

[21]Although the minimum required number of clusters cannot be easily determined as it depends on the application, Angrist and Pischke argue that the evidence from DD research on U.S. states suggests that more than 50 clusters should be sufficient. In our baseline setting, we use a total of 182 districts with 86 in the treatment and 96 in the control group.

[22]The results from these estimations are available upon request.

Rhineland-Palatinate, Baden-Württemberg, and Bavaria. More than 95 percent of the U.S. Military personnel was based in these states in 1990, that is before the beginning of the drawdown.

Our treatment variable is a measure of the annual level of U.S. forces in a given district. The data on the regional level of U.S. forces are calculated from a newly constructed database that combines official U.S. data on the number of assigned personnel at the individual base level with the geocoded location of the base and the dates when realignment and closure decisions were first announced. Details on the original U.S. military data sources and the construction of the database are provided in the Data Appendix. Figure 3 illustrates the extent of the base realignments between 1990 and 2002 across West Germany, while Table 1 shows the selection of the districts and Figure 4 shows the distribution of the U.S. forces for the treatment districts in 1990. The mean (standard deviation) of this variable is 3,707 (4,477), the median is 2,151 and the min (max) is 4 (20,087). Districts within the four southern federal states in which the U.S. Military was never present constitute the control group. Figure 5 exhibits the geographical distribution of the 86 treatment and 96 control districts in our baseline sample.

Data on our outcome variables of employment and wages for all districts in our sample is based on the full universe of the social security records which are provided by the German Institute for Employment Research (IAB). The individual employment spells, drawn from the entire set of employment histories (*Beschäftigten-Historik, BeH*), provide information on the employees' characteristics (for example, age, nationality, and education) and with the use of an employer identifier for each spell, are merged with firm-level information from the histories of establishments (*Betriebs-Historik-Panel, BHP*) that detail the industry affiliation, and workplace location. Our outcome data is thus very similar to the variables contained in the IABS, a widely used and well documented 2% subsample of the social security records that is publicly available to researchers.[23]

The use of the full universe of the employment spells is crucial to our analysis. We expect that the effects from the military drawdown primarily accrue to employees of small and medium-sized companies that are active in local non-tradable industries. Using the IABS directly for the analysis would therefore not be a suitable alternative, since employment spells from employees in large firms and/or large industries are more likely to be included there.[24]

[23]See the latest IABS data documentation in Drews (2007).

[24]See Dustmann and Glitz (2008) for a similar argument in the context of firm-level responses to changes in local labor supply. They report that for 1995, the share of large firms with more than 100 employees included in

The data include all employment that is subject to social security contributions, but excludes the self-employed, civil servants, students enrolled in higher education, and the German military. More importantly for the purpose of our analysis, the data do not contain information on hours worked, and as part-time employment is only covered consistently from 1999 onwards, we restrict our analysis to full-time employees. In the interest of abstracting from other potentially confounding factors, we further limit our sample to prime age employment of employees aged 25 to 55 and exclude employment in agriculture, mining, and sectors that are dominated by government activities and public ownership. We also exclude German employees who are employed by U.S. bases and other foreign forces.[25]

Information on individual education levels in the original BeH employment spells is improved using the standard imputation algorithm developed for the IABS by Fitzenberger et al. (2006). In line with similar previous studies, the education information is then separated into three categories distinguished primarily according to vocational qualification: (1) low education for people without any occupational training; (2) medium education for people who have either completed an apprenticeship or graduated from a vocational college and (3) high education for people who hold at least one degree from a technical college or a university.

Similarly, we distinguish and code for each employment spell, based on the employer information, three categories of establishment sizes: (1) firms with up to 25 employees, (2) those with more than 25 but less than 100 employees, and (3) those with 100 employees or more.

Our wage outcome variable is real gross daily wages. The wage information in the BeH data has the advantage of being very accurate, as it stems from administrative records of the employers. On the downside, wages are top-coded at the social security contribution threshold (SSCT). The share of censored wages increases with education.[26] We will show later that the employment of highly educated employees is not affected by the withdrawal of the U.S. forces. Therefore, we exclude them from most of the wage analyses. For all other employees we impute and replace the right-censored wages using an imputation algorithm developed for the IABS by Gartner (2005) and implemented in a similar fashion by numerous studies that use some

the IABS is almost 15%, while the true share over the whole population of firms in Germany is less than 2%.

[25]These are identifiable in the industry classification of the Federal Employment Agency of 1973 which we use (the 3-digit code is 921, labeled "Dienststellen der Stationierungsstreitkräfte", i.e. 'agencies of the stationed forces'). See Bundesanstalt für Arbeit (1973) and Blien et al. (1992).

[26]In our full sample, for male (female) employees, the wage information is right-censored each year for up to 2.8% (0.8%) of the spells in the case of low education, 12.7% (1.9%) in the case of medium education, and 67.0% (25.8%) in the case of high education.

IAB dataset (for example, Dustmann et al. (2009) and studies cited in the review by Büttner (2010)).[27] Wages are deflated by the common consumer price index (base year: 2000) for West Germany provided by the Federal Statistical Office.

We construct a panel of yearly cross-sections for each district at the reporting date of June 30 in each year. For the employment outcome that does not vary on the individual level, we summarize the level of district employment into district-year observations; beyond total employment, we also calculate the annual district employment level according to age, education level, and industry groups in order to enable the separate analyses of heterogeneous effects. For the analysis of wages that do vary at the individual level, we focus on male employees and draw a 10 percent random subsample of the individual employment spells for each district in each year.[28] We merge district level information on population and area size from the German Federal and State Statistical Offices with the data, and include information on two basic area types (districts in "urban" areas versus "rural" areas), using a classification developed by Möller and Lehmer (2010) for their analysis of the urban wage premium that builds upon the original classification scheme by the German Federal Office for Building and Regional Planning (BBR). Finally, we separately identify all "border" districts that share a common border with any neighboring foreign state.[29]

Table 2 presents summary statistics on several indicators for the treatment and control districts in our baseline sample for the years 1990 and 2002. Columns (1) and (4) report the means for the treatment districts and columns (2) and (5) report those for the control districts in 1990 and 2002 respectively. Columns (3) and (6) include the respective differences and indicate the statistical significance from t-tests on the equality of means. The treatment districts in our

[27] Specifically, we first ran a series of tobit regressions of log wages separately for each year, gender and education group with covariates that include a quartic in age and dummies for foreign citizenship, occupations, industries and the local districts (the results from theses estimations are available upon request). The top-coded wage observations were then replaced by draws from normal distributions that are truncated from below at the SSCT and the moments of which are determined from the respective tobit estimations. Büttner and Rässler (2008) and Büttner (2010) have recently criticized this "homoscedastic single imputation" as it may lead to biased variance estimates and develop a Bayesian multiple imputation method allowing for heteroscedasticity (MI-Het), which they can show to perform better in simulations with the IABS. Given the higher computational intensity of this approach and the fact that we use a much larger dataset with the entirety of records for a long panel, we decided to remain with the simpler method.

[28] As a single annual cross-section for all our districts consists of up to 8.4 million individual spells, working with the full population panel was not feasible due to standard limits of memory size and computational speed.

[29] While we do include these districts in our baseline sample, we confirm our results by excluding them in some of our later robustness checks, as these regions could potentially be influenced by workers who commute to the other side of the border. Moreover, the districts that are located on the former border to East Germany or the Eastern border to other former member states of the Warsaw Pact benefit from higher levels of regional subsidies (for example, from the European Regional Development Fund) in response to their marginal location.

sample have, on average, a higher population and are more densely populated. This is also reflected in the figures of the third subpanel in Table 2: 33% of the treatment districts are located in urban areas compared with 16% of the control districts. The bottom panel quantifies the geographic distribution across the four federal states (see Figure 5). It suggests that the spatial distribution is quite balanced, although within Hesse, the treatment districts outnumber the control districts. In the entire sample, this is compensated for by a higher share of control districts located in Bavaria.

In summary, the unconditional cross-sectional comparisons for the two selected years reveal some differences between our treatment and control regions. The key identifying assumption in the difference-in-difference framework that we employ only requires that the outcomes in the treatment and control group follow similar time trends in the pre-treatment period (see Angrist and Krueger, 1999, Angrist and Pischke, 2009, chap. 5). Cross-sectional differences only lead to a violation of this assumption if they affect changes in the outcomes in a time-varying way. As outlined in the previous section, we control for any source of potential misspecification by including in all our regressions district and time-fixed effects, and also estimate specifications that are enriched by state-by-year fixed effects and full sets of district-specific linear or quadratic time trends. Finally, given the strong variation in the U.S. force numbers at the district level, our identification relies as much on the comparison between the districts in the treatment group as on the comparison with districts in the control group.

5 Results

5.1 Employment

A. Initial Estimates

Table 3 reports the results for our initial OLS regression of equation (1). Each column presents three separate regressions of the log of total district private sector employment, with entries in panel A reporting the results for the pooled employment outcome of all employees and Panel B and C considering the outcome separately for male and female employment.

The regressions reported in the first column include only the measure of the U.S. withdrawal treatment and district and year fixed effects on the right hand side of the equation. The estimated coefficient is negative and statistically significant, with the point estimate being larger for men than for women, and being more precisely measured for males. The second column adds state-by-year dummies for the four different federal states to the model in order to absorb state-specific shocks, with the estimate for the overall employment being virtually unaltered, although the point estimate for males drops slightly and that for females rises and is now significant.

Once we add the full set of 130 district-specific linear time trends to the model in column (3), the precision of the estimates is increased considerably, with the size of the standard errors being more than halved. The absolute value of the negative coefficient estimate drops to about 0.4 log points, but remains highly significant. Replacing the linear by quadratic trends in column (4) only slightly alters the results, and the negative point estimate on the U.S. forces level variable remains highly significant despite the inclusion of almost 550 covariates. The hypothesis that the state-year interactions or district-specific time trends are jointly zero is strongly rejected by the data in standard F-tests. In these two final specifications, the effects are the same both in magnitude and in terms of precision for the two genders.

The size of the coefficients in the last two specifications suggests that the elasticity of private sector employment with respect to the reduction of U.S. Forces is -.004. This implies that a reduction of U.S. forces by 100 persons leads to a loss of about 4.6 full-time private sector jobs.[30]

In the remainder of the paper, we always tabulate results for the latter two specifications that include the district-specific linear or quadratic time trends and which provide in our view robust and conservative estimates of the withdrawal effect. [31]

B. Heterogeneity of effects

As argued in the introduction, the U.S. withdrawal shock constituted a consumption shock that affected local labor demand because it was concentrated among locally produced, non-traded goods and services. In this section, we test this notion by allowing for heterogeneity of effects across different subsets of total district employment. We first partition local employment along the industry margin.

Table 4 shows the results for specifications analogous to specification (1), estimated for

[30]The mean level of U.S. forces was about 3,700 in 1990, and the mean level of private sector employment in treatment regions was about 42,300.

[31]We have also tested specifications that include a square or cubic function of our treatment variable, but these specifications were rejected in favor of the simpler linear model.

employment separated according to industry groups.[32] Consistent with the nature of the shock, the largest and most significant effects are found in the sectors of food and consumption goods, and particularly private household services.

The top part of Table 5 provides estimates for our withdrawal coefficient for the district employment in the three age groups (again subdivided in panels according to gender), with odd-numbered columns reporting the specifications with linear, and even-numbered columns the specifications with quadratic district time trends. The coefficient estimates suggest that the adverse effect of the withdrawal mainly manifests itself in lower employment growth for younger male and female and for older female workers, while the point estimates for the other groups are smaller in absolute value and not significantly different from zero. Similarly, the bottom part of the table reveals that it is primarily the employment of low and medium educated workers that is affected, although surprisingly, we find the strongest effects of approximately -0.5 to -0.7 log points for the employment of high-qualified female workers.

C. Dynamic pattern of effects

In the analysis thus far, we have employed the traditional DD setting that presumes discrete changes in the treatment variable leading to instantaneous effects on the outcome of interest – an assumption that is likely not to hold in our context: even if individual bases were closed down swiftly after the announcement was made, the force reductions at the district level in most cases took a couple of years to reach their full extent. The single coefficient on the treatment variable would then fail to capture these longer-term effects. Similarly, although the first base closure announcement for a district came as a surprise to the agents in the local economy, as we have argued in section 2.2, employers in districts that were only affected late in the withdrawal phase could still have responded by reducing their labor demand before the first announcement for their district occurred if they expected cuts to reach their area at a later stage. These anticipatory effects could lead the estimates of the single coefficient for the withdrawal treatment to be biased towards zero. Since the timing of the withdrawal, measured by the first announcement in a district, exhibits some variation across treatment districts, we can identify and explore the dynamic pattern of the effect separately from the overall year

[32]We only tabulate the results here for the total employment (male and female) by industry since the estimates do not vary systematically by gender. The more detailed results from the separate estimations are available upon request.

effects by augmenting the specification from equation (1) with lead and lag effects. We chose a symmetric window that includes eight lead variables for the eight years before the first withdrawal announcement occurred and eight lag variables for the years 0-7 and year 8 onwards, as our selection of treatment and control districts allows us to have a balanced sample over this time span.

Table 6 presents the results when we reestimate the effect on total district employment in the augmented model using all four different specifications again regarding the combination of state by year dummies and linear or quadratic time trends. In all four specifications, the coefficients on the withdrawal announcement leads are hardly significantly different from zero, showing little evidence of anticipatory employment responses. More importantly, the point estimates on the withdrawal treatment delays continuously become more negative and significant, starting from approximately -0.3 log points in year 2 after the withdrawal announcement up to -0.7 log points after five to six years. Notably, the coefficient for the long term effect for year 8 onwards still exhibits a negative effect that is at least in some specifications significantly different from zero. This dynamic pattern is depicted for all four different specifications in figure 6.

The diffusion of the effect with stronger negative coefficients several years after the withdrawal started in a district is in line with our expectation that the reduction in local demand is only incorporated and adjusted for with some time delay. However, the persistence of the negative "steady state" effect until at least 7 years after the start of the withdrawal might be surprising if one rather expects the effect to fade off at some time. Given our sample period, the results do not preclude that a mean reversion might occur in later periods, particularly, since we do not incorporate in our empirical approach information on the size and timing of redevelopment and conversion efforts in the treatment districts that could compensate for the reduction in employment from the withdrawal of the U.S. forces. However, the available case study literature suggests that apart from a small number of high-profile exceptions, the planning of local conversion projects took several years before they even started to be implemented.[33] In addition, even if conversion projects were successful in promoting local economic development and employment growth, this would lead our estimates to underestimate the true negative effects, and not the other way around.

[33]See Cunningham and Klemmer (1995); Bonn International Center for Conversion (BICC) (1996). Brauer and Marlin (1992) also provide a general overview of the specific challenges of conversion in the economics literature.

D. Further Robustness Checks

We close our empirical analysis on the employment effects by describing some alternative checks on the robustness of our results. While the augmented specification with lead and lag effects from the previous section is probably the most appropriate and flexible one for ascertaining the local effects from the U.S. withdrawals, we return here for ease of exposition to the simpler specification from equation (1). Table 7 reproduces in row 1 the coefficient estimates for the effect on total district employment from the top panel in column (7) and (8) of table 3 as a baseline for comparison.

We first consider the robustness with respect to alternative definitions of our treatment variable. Row 2 reports the estimates for a specification in which the treatment intensity is not defined by the change in the total U.S. personnel level, but only by the change in the military positions. The idea is that the civil employees could potentially have stayed in the respective district and continued to consume in the local economy, living off income from alternative jobs or unemployment benefits. The results are not affected by this change in the treatment variable. The next specification examines whether the treatment effect differs for the withdrawal of U.S. Army versus U.S. Air Force troops. Unfortunately, the Air Force withdrawals affect only 4 districts, so while the absolute value of the point estimates is comparable to the U.S. Army estimates, the Air Force coefficient is no longer significant given the larger standard errors. Another variation of our baseline specification uses the first actual base closure date instead of the first announcement date as the identification of the start of the "post-treatment period". Again, the coefficient estimates are virtually unaltered.

Next, we consider the robustness with respect to alternative sample definitions. Row 5 reports results where we only use the variation in treatment intensity within the group of our treatment districts to identify the withdrawal effect. The point estimates remain almost identical to the baseline. Similarly, the exclusion from the sample of very populous treatment districts (given the structural differences in the population size between treatment and control districts described in the summary statistics in section 4), of districts located in urban areas, of treatment districts that saw only a partial reduction of the U.S. force level, or of Eastern or Western border districts does not seem to affect the coefficient estimates.

In the specifications reported in Rows 11 to 13, we study the potential influence of three specific alternative district-specific shocks that could potentially bias our coefficient estimates. The results in row 11 stem from a specification in which we include an additional covariate (labeled "U.S. Forces 1990 X FX") that interacts the log U.S. forces level in a given district in 1990 (before the start of the drawdown) with the average annual U.S. dollar to German DM exchange rate. This term is supposed to absorb the effects from the elasticity of the U.S. demand active in the West German economy with respect to fluctuations of the exchange rate that would have occurred even if the local force level had remained constant throughout our sample period.[34] By introducing a covariate that explicitly captures this effect, we rule out any negative effects on the local economy that may have occurred already before and/or independent of any reductions in U.S. force levels, and thereby reinforce our identification assumption. The coefficient estimate on this term is highly significant (at least in the specification with quadratic time trends) and has the expected positive sign (i.e. a devaluation of the U.S. dollar seems to be associated with a drop in labor demand in the local economy). Reassuringly, our estimate of the U.S. forces coefficient hardly differs from the baseline estimates.

During the time period of the U.S. withdrawal, the German Army and the French forces (Forces Françaises en Allemagne) also implemented realignments of their bases as part of the CFE treaty. Although smaller in absolute and relative size, these cuts more likely affected the control districts in our sample, due to the spatial partitioning of the combined military presence with the U.S. forces as NATO allies. Figure 8 depicts the regional presence by the German armed forces as a share of the district population in 1991 and identifies the districts where realignments took place between 1991 and 2002, while Figure 9 depicts the districts within the four federal states that also hosted bases of the French forces.[35] If we exclude these districts subsequently from the treatment and control groups, the estimates in row 12 and 13 reveal that the concurrent cuts by the German armed forces and the French forces do not seem to affect our estimates of the U.S. withdrawal effect significantly.

As already summarized in our discussion of the empirical approach in section 3, the question

[34] Bebermeyer and Thimann (1990) note that the "50 percent decrease in the value of the US currency relative to the D-mark from 3.30 in March 1985 to 1.65 in January 1988 has meant a corresponding cut in the purchasing power of US servicemen stationed in the FRG". They document that this devaluation did not only lead to a reduction of the private American demand active in the local economy at the time, but also incited the U.S. bases to reduce their number of German direct employees and substitute some goods previously sourced from West German suppliers with imports from U.S. companies. Figure 7 plots the time series of the exchange rate for our sample years 1975 to 2002.

[35] For details on the respective data sources, see section C.3 and C.4 in the Data Appendix.

of the correct calculation of standard errors and inference in DD settings has recently received increasing scrutiny. In our baseline estimates, we have always reported robust standard errors clustered on the district level that allow for arbitrary correlations within districts over time. Row 14 of table 7 presents the results if we additionally weight the district-year observations by the district population in order to account for potential heteroscedasticity of the error term. The coefficient estimates are again only slightly reduced in their absolute value, and remain highly significant. Next, we also used an alternative specification of the standard errors recently proposed by Cameron et al. (2011). Their method aims to improve inference in situations with non-nested multi-way clustering, and they specifically mention the case of state-year panels where, in principle, it could be desirable to cluster both on the geographic unit (to allow for serial autocorrelation) and year level to account for geographic-based spatial correlation. In the case of two-way clustering, the estimator is calculated by adding up the variance matrices from OLS regressions with errors clustered on the first and second dimension respectively, and then subtracting the variance matrix from a regression with errors clustered on both dimensions. We implement the estimator in our data, with the two dimensions being district and year. The estimated coefficients reported in row 15 remain significant at the 5 and 10 percent level despite the augmented standard errors. A further conservative alternative to account for potential spatial correlation that could bias standard errors downwards is to cluster at higher regional levels of aggregation. In this vein, Row 16 presents the coefficient estimates once we cluster the errors on the levels of German labor market regions: reassuringly, the increase in the standard errors is minuscule, so that the results do not differ from the baseline in a meaningful way.[36]

5.2 Wages

In our analysis thus far, we have documented that the withdrawal of U.S. forces did indeed negatively affect employment in the German local labor markets where they were located. In light of this evidence, we now address the question of whether the withdrawal also led to a downward adjustment of local wages. As already described in sections 3 and 4, we base our wage analyses on random subsamples of the individual employment spells from male employees for each district and year in our sample period.

[36]We also repeated the whole empirical analysis at this higher aggregation level of labor market regions. The coefficient estimates on the U.S. forces variable were negative and highly significant in all specifications, despite the lower variation in the treatment variable and the smaller number of observations.

Tables 8 to 10 present the results from DD estimations of equation 2 of real gross daily wages (in logs), in breakdowns analogous to our employment analyses. All regressions include the full set of available individual-level covariates (age, age^2, dummies for foreign citizenship, education levels, occupational groups) and the control variable for the exchange rate effect. The estimates for the overall wage effect in Table 8 and separately according to age and education groups in Table 9 are hardly significantly different from zero and suggest that local real wages did not respond to the withdrawal shock. If anything, older and low qualified workers seem to enjoy some relative wage increases in the affected districts, an effect that might stem from the primary selection of younger workers to be dismissed. The analyses according to industries in Table 10 only reveal some evidence of potential downward wage adjustments for the sectors of "Food and consumption goods" and "Transport/Information".

5.3 Impact on Unemployment and Migration

In this section, we use aggregate district-level data to examine the impact of the U.S. withdrawal shock on unemployment and migration and provide further evidence of the relative importance of the potential margins of adjustment in response to the withdrawal shock. The Statistics Department of the German Federal Employment Agency (*Bundesagentur für Arbeit, BA*) publishes a time series on district level data on the number of unemployed and the unemployment rate starting in late 1984. Consistent with our timing convention for employment, we use the so-called quarterly statistic reported for the month of June in each year. To analyze the migration response, we use aggregated data on net migration (the difference in the number of in-ward migrants versus out-ward migrants) provided by the Statistische Ämter des Bundes und der Länder (2011) and from complimentary data requests with the individual state statistical offices to construct a consistent panel of district-year observations for the period 1985 to 2002.[37]

In table 11, we first report in panel A and B the results for the estimated impact on total employment and wages for the comparable shorter time period from 1985-2002.[38] The coefficient estimates are consistent with our previous estimates for the longer time period from 1975-2002

[37]Unfortunately, more detailed migration data that reports the number of in-ward migrants and out-ward migrants separately and in further splits by age groups, gender and citizenship is only available at the district level from 1995 onwards.

[38]We focus here on the first three specifications with district and year FE only (1), the inclusion of state-by-year effects (2) and linear district-specific time trends (3), as our results indicate that we cannot robustly estimate the specification with quadratic district-specific trends in this shorter period in which the number of district observations in the pre-period before the withdrawal starts is more than halved compared to the period before.

(see tables 3 and 8), with significant negative effects for local private sector employment and no adjustment in local wages. Panel C reports the coefficient estimates for an analogous DD regression with the district unemployment rate as dependent variable. The results suggest that the withdrawal of the U.S. forces increased unemployment.Panel D provides results from the DD estimation on the net district migration share.[39] The negative sign of the coefficient estimates suggests a shift in the balance of migration towards greater out-ward migration in the treatment districts after the withdrawal, but none of the estimates is statistically significant.

As argued previously for our employment outcome in section 5.1, the estimates for the year-by-year effect could mask a richer pattern of dynamic adjustments, particularly if unemployment and migration are only affected with some timedelay. In table 12, we hence present results from analogous regressions of the dynamic pattern for all outcomes for the time period 1985 to 2002.[40] Column (1) reports the coefficient estimates for the withdrawal leads and lags for employment. Consistent with the previous results in table 6 for the "long" sample period, all lead coefficients are statistically indistinguishable from zero, and the significant negative coefficient estimates on the withdrawal delays in the post period reach their peak around 5 years after the first withdrawal announcement. For real wages in column (2), the results confirm the absence of any significant adjustment effects throughout the whole observation period.

The dynamic pattern of the coefficient estimates for the effect on unemployment and the unemployment rate in columns (3) and (4) does indeed provide some suggestive evidence that the decline in employment was (partially) absorbed by rising unemployment. Even if some lead effects are also marginally significant, the pattern of the lagged effects provides a consistent picture of continuously larger coefficient estimates up to a peak in years 5 to 6 after the initial withdrawal shock. The coefficient estimates remain at this level even through the long term effect for year 8 onward, but the lower precision and loss of significance for the estimates after year 7 prevent conclusive inference on the persistence of the rise in unemployment. Finally, column 5 shows the results for the comparable regressions for the net migration share. Again, the coefficient estimates in the pre-withdrawal period are statistically indistinguishable from zero. The estimates on the lag effects all have a negative sign and are larger in absolute value. However,

[39]We define the net migration share by dividing the net migration balance in year t by the district population in year t-1.

[40]For compactness, we focus here on the specification that includes linear time trends and a consistent time period of between 5 years before and 8 or more years after the beginning of the withdrawal in a given district.

only the coefficient estimate for year 3 after the withdrawal announcement is significant at the 5 percent level. In light of the data limitations outlined above, we interpret this as a qualitative indication that some of the adjustment in response to the withdrawal shock could also occur via increased out-ward or reduced in-ward migration in the affected regions.

Overall, our comparison of the estimated adjustment effects suggest that the withdrawal shock primarily led to adjustments in quantities, and not in prices (i.e. wages). This finding is consistent with Topel's (1986) result for the effect from a permanent local economic shock.

6 Conclusion

Empirical research has had difficulty in establishing the causal effects of local economic shock, and one important reason for this issue is that the measurement of local economic shocks has proven to be difficult. In this paper, we exploit the district variation in the stationing and withdrawal of U.S. military forces in Germany after German reunification and the end of the Cold War to examine the consequences of regional economic shocks on local labor market outcomes.

The unique natural experiment setting of the event allows us to improve on limitations that impaired previous studies analyzing the effect of regional economic shocks on local labor markets. The U.S. forces were stationed in West Germany in the 1950 at strategic points along two major defense lines; local economic considerations were not important in this decision process. In addition, and in a similar fashion to the stationing decision, the withdrawal decisions for the U.S. forces in Germany were made exclusively by U.S. military officials and were neither subject nor responsive to any politicizing: the U.S. Department of Defense decided on the details of the withdrawal process purely on strategic military grounds. Both of these facts alleviate concerns regarding the validity of exogeneity assumptions.

The results show that the withdrawal of the U.S. forces did have negative consequences for private sector employment and for local unemployment. Wages and migration patterns, however, were not affected.

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A Tables

Table 1: Summary statistics for selection of treatment districts

Regional selection	Number of districts	Number of bases	Total U.S. forces (in 1990)	Share of total	U.S. forces personnel according to district 1990: Mean abs.	Mean rel.⋆	Median abs.	Median rel.⋆	Min abs.	Min rel.⋆	Max abs.	Max rel.⋆
Districts with U.S. installation(s) in West Germany in 1990		872	360,091	100.0								
Districts with assigned U.S. personnel in West Germany in any year 1986-2009	130	486	360,091	100.0	2,770	2.2	988	0.4	0	0.0	20,087	21.4
Districts with assigned U.S. personnel in HE, RP, BW, BY	106	441	337,017	93.6	3,179	2.7	1,403	1.1	0	0.0	20,087	21.4
of which												
- Districts with U.S. personnel presence without reduction/withdrawal announcement until 2002	9	25	9,240	2.6	1,027	0.9	448	0.1	0	0.0	3,833	3.9
- Districts with missing information (withdrawal announcement, etc.)	10	10	1,689	0.5	169	0.2	0	0.0	0	0.0	1,346	1.2
- Outlier district Kreis Zweibrücken (id=7320)	1	3	7,247	2	7,247	21.4	7,247	21.4	7,247	21.4	7,247	21.4
Treatment districts in baseline specification	86	403	318,841	88.5	3,707	2.9	2,151	1.4	4	0.0	20,087	17.8
of which												
- Districts in Eastern border regions with Czechoslovakia, former GDR	6	16	10,397	2.9	1,733	1.4	1,396	1.3	5	0.0	3,869	4.0
- Districts in border regions with 'Western' countries†, North or Baltic Sea	10	36	27,599	7.7	2,760	3.0	1,370	0.8	4	0.0	11,740	12.8

Notes: ⋆ Percentage measure relative to total district population † Denmark, Netherlands, Belgium, France, Luxemburg, Liechtenstein, Switzerland, Austria

Table 2: Selected district characteristics according to treatment status

	1990			2002		
	Treatm. (1)	Contr. (2)	Diff. (3)	Treatm. (4)	Contr. (5)	Diff. (6)
Demographics						
Population	184,497	124,736	59,761***	195,965	136,325	59,641***
	(17,768)	(6,410)	(18,889)	(18,128)	(7,109)	(19,472)
Population density	625	346	279***	648	366	282***
(inhabitants per sqkm)	(83)	(41)	(93)	(83)	(42)	(93)
Socio-economic outcomes						
GDP per capita†	23,573	22,000	1,574	28,960	27,136	1,824
(EUR)	(1,004)	(769)	(1,265)	(1,280)	(1,002)	(1,626)
Private sector employment	42,316	23,356	18,960***	42,760	24,973	17,787***
	(6431)	(1580)	(6623)	(6226)	(1681)	(6449)
Ave. growth rate, 1975-1990	.011	.013	-.002			
	(.001)	(.001)	(.002)			
Ave. growth rate, 1990-2002				.002	.005	-.003*
				(.001)	(.001)	(.001)
Unemployment rate	5.5	5.1	0.4	7.7	7.0	0.7**
	(0.2)	(0.2)	(0.3)	(0.2)	(0.2)	(0.3)
Net migration	3,326	2,350	976***	878	697	181
	(325)	(131)	(350)	(102)	(58)	(117)
Area type						
Urban	.326	.156	.169***	.326	.156	.169***
	(.051)	(.037)	(.063)	(.051)	(.037)	(.063)
Geographic distribution						
Hesse	.221	.042	.179***	.221	.042	.179***
	(.045)	(.020)	(.049)	(.045)	(.020)	(.049)
Rhineland-Palatinate	.198	.146	0.052	.198	.146	0.052
	(.043)	(.036)	(.056)	(.043)	(.036)	(.056)
Baden-Württemberg	.209	.260	-0.051	.209	.260	-0.051
	(.044)	(.045)	(.063)	(.044)	(.045)	(.063)
Bavaria	.372	.552	-.180**	.372	.552	-.180**
	(.052)	(.051)	(.073)	(.052)	(.051)	(.073)
N	86	96		86	96	

Notes: †Due to data limitations, GDP per capita reported in 1990 column are 1992 values. * Significant at 10%, ** at 5%, *** at 1%.

Table 3: Estimated impact of U.S. military withdrawal on total district employment, 1975-2002

Dep. Variable: Employment (log)		(1)	(2)	(3)	(4)
		A. - All			
log U.S. forces		-.008***	-.007***	-.004***	-.004***
		(.002)	(.002)	(.001)	(.001)
	R^2	.987	.988	.998	.998
		B. - Male			
log U.S. forces		-.009***	-.007***	-.004***	-.004***
		(.002)	(.003)	(.001)	(.001)
	R^2	.987	.988	.997	.998
		B. - Female			
log U.S. forces		-.006	-.008***	-.004***	-.004***
		(.003)	(.003)	(.001)	(.001)
	R^2	.983	.984	.997	.998
Other covariates:					
State by year dummies		No	Yes	Yes	Yes
District x time trends		No	No	Yes	Yes
District x time^2 trends		No	No	No	Yes
N		5,096	5,096	5,096	5,096

Notes: Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects. Robust std. errors clustered at district level in parentheses. The F-statistics [p-values] for the inclusion of the state by year dummies, linear and quadratic time trends are, respectively: 7.144 [0.00], 745.2 [0.00] and 245.4 [0.00]. * Significant at 10%, ** at 5%, *** at 1%.

Table 4: Impact of U.S. military withdrawal on employment according to industry

Dep. Variable: Employment (log) by industry		(1)	(2)
		1 Basic materials	
log U.S. forces		-.004	-.004*
		(.003)	(.002)
	R^2	.988	.992
		2 Investment goods	
log U.S. forces		-.004*	-.003*
		(.002)	(.002)
	R^2	.995	.997
		3 Food and consumption goods	
log U.S. forces		-.003	-.004**
		(.002)	(.002)
	R^2	.991	.995
		4 Construction	
log U.S. forces		-.004**	-.002
		(.002)	(.001)
	R^2	.989	.993
		5 Retail/Repair	
log U.S. forces		-.000	-.001
		(.002)	(.001)
	R^2	.995	.997
		6 Transport/Information	
log U.S. forces		-.004	-.002
		(.003)	(.002)
	R^2	.985	.991
		7 Corporate services	
log U.S. forces		-.002	-.003*
		(.002)	(.002)
	R^2	.994	.996
		8 Private household services	
log U.S. forces		-.006***	-.005***
		(.002)	(.002)
	R^2	.994	.996
Other covariates:			
State by year dummies		Yes	Yes
District x time trends		Yes	Yes
District x time^2 trends		No	Yes
N		5,096	5,096

Notes: Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects and a control variable for the exchange rate effect (see table 3). Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Table 5: Impact of U.S. military withdrawal on employment according to age and education groups

Dep. Variable: Employment (log)	(1)	(2)	(3)	(4)	(5)	(6)
5-1 Results according to age group						
	A. - All					
	25-35 yrs.		35-45 yrs.		45-55 yrs.	
log U.S. forces	-.005***	-.004***	-.004**	-.004***	-.001	-.002
	(.001)	(.001)	(.002)	(.001)	(.001)	(.001)
R^2	.995	.998	.995	.997	.996	.998
	B. - Male					
	25-35 yrs.		35-45 yrs.		45-55 yrs.	
log U.S. forces	-.005***	-.004***	-.004**	-.003***	-.001	-.001
	(.002)	(.001)	(.002)	(.001)	(.001)	(.001)
R^2	.995	.997	.995	.997	.996	.998
	C. - Female					
	25-35 yrs.		35-45 yrs.		45-55 yrs.	
log U.S. forces	-.004**	-.004***	-.004***	-.004***	-.002	-.003**
	(.002)	(.001)	(.002)	(.001)	(.002)	(.001)
R^2	.994	.997	.995	.996	.994	.997
5-2 Results according to education group						
	A. - All					
	High		Medium		Low	
log U.S. forces	-.003	-.003	-.003***	-.003***	-.004***	-.004***
	(.002)	(.002)	(.001)	(.001)	(.001)	(.001)
R^2	.996	.997	.998	.998	.994	.996
	B. - Male					
	High		Medium		Low	
log U.S. forces	-.003	-.003	-.004***	-.003***	-.004***	-.003***
	(.002)	(.002)	(.001)	(.001)	(.001)	(.001)
R^2	.995	.997	.997	.998	.991	.994
	C. - Female					
	High		Medium		Low	
log U.S. forces	-.007***	-.005**	-.003***	-.003***	-.004**	-.005***
	(.003)	(.002)	(.001)	(.001)	(.002)	(.001)
R^2	.992	.994	.998	.999	.995	.997
Other covariates:						
State by year dummies	Yes	Yes	Yes	Yes	Yes	Yes
District x time trends	Yes	Yes	Yes	Yes	Yes	Yes
District x time^2 trends	No	Yes	No	Yes	No	Yes
N	5,096	5,096	5,096	5,096	5,096	5,096

Notes: Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects. Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Table 6: Dynamic pattern of impact of U.S. military withdrawal on total employment at district level

Dep. variable: Total employment (log)	(1)	(2)	(3)	(4)
WD announcement$_{t-8}$	.000	.000	.000	-.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-7}$	.000	.000	.001	.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-6}$	-.001	.000	.001	-.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-5}$	.001	.001	.001	.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-4}$	.001	.002	.001	.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-3}$	.001	.002	.001	.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-2}$	.001	.002	.001	.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t-1}$	.001	.001	.001	.000
	(.001)	(.001)	(.001)	(.001)
WD announcement$_{t0}$	-.000	.000	.000	-.001
	(.001)	(.002)	(.001)	(.001)
WD announcement$_{t+1}$	-.002	-.001	-.001	-.002*
	(.002)	(.002)	(.001)	(.001)
WD announcement$_{t+2}$	-.006***	-.003	-.002	-.003**
	(.002)	(.002)	(.001)	(.001)
WD announcement$_{t+3}$	-.007***	-.005**	-.004***	-.004***
	(.003)	(.003)	(.002)	(.001)
WD announcement$_{t+4}$	-.009***	-.007**	-.005***	-.005***
	(.003)	(.003)	(.002)	(.001)
WD announcement$_{t+5}$	-.009***	-.008**	-.006***	-.006***
	(.003)	(.003)	(.002)	(.002)
WD announcement$_{t+6}$	-.009***	-.008**	-.007***	-.005***
	(.003)	(.003)	(.002)	(.002)
WD announcement$_{t+7}$	-.009***	-.008**	-.006***	-.005**
	(.003)	(.003)	(.002)	(.002)
WD announcement$_{t+8forward}$	-.008**	-.006	-.005*	-.002
	(.004)	(.004)	(.003)	(.003)
Other covariates:				
State by year dummies	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes
R^2	.987	.988	.998	.998
N	5,096	5,096	5,096	5,096

Notes: All regressions include district and year fixed effects. Robust std. errors clustered at district level in parentheses. The WD announcement dummies are defined relative to the year of the first announcement of the U.S. withdrawal for a district, $t = 0$. * Significant at 10%, ** at 5%, *** at 1%.

Table 7: Robustness analyses for impact of U.S. military withdrawal on total employment

Dep. Variable: Total employment (log) - All	(1)	(2)
1. Baseline Table 3-A. estimates	-.004***	-.003***
N=5,096, N(treatment)=86, N(control)=96	(-.001)	(-.001)
2. Restrict treatment intensity to reduction in U.S. military personnel	-.004***	-.003***
	(.001)	(.001)
3. Separate treatment effect by "U.S. Army" vs "Air Force" personnel		
U.S. WD treatment (%) - Army (N=84)	-.004***	-.003***
	(-.001)	(-.001)
U.S. WD treatment (%) - Air Force (N=4)	-.004	-.003
	(-.003)	(-.003)
4. Use 1st base closure date in district as start of "post" period	-.005***	-.004***
	(.001)	(.001)
5. Include only treatment districts	-.004***	-.003***
N=2,408, N(treatment)=86, N(control)=0	(-.001)	(-.001)
6. Exclude treatment districts with pop. > most populous control district	-.004***	-.004***
N=4,928, N(treatment)=80, N(control)=96	(.001)	(.001)
7. Exclude districts in urban areas	-.005***	-.005***
N=3,892, N(treatment)=58, N(control)=81	(.002)	(.001)
8. Keep only treatment districts with complete closure by 1995	-.004**	-.004***
N=3,192, N(treatment)=33, N(control)=81	(.002)	(.002)
9. Exclude Eastern border districts	-.003***	-.003***
N=4,648, N(treatment)=80, N(control)=86	(.001)	(.001)
10. Exclude Western border districts	-.005***	-.004***
N=3,120, N(treatment)=70, N(control)=60	(.001)	(.001)
11. Include control variable for "US-FX" effect		
log U.S. forces	-.004***	-.003***
	(.001)	(.001)
log U.S. forces 1990 X FX	.001	.002***
	(.001)	(.001)
12. Exclude districts with Bundeswehr reduction 1991-2001	-.004***	-.003***
N=4,060, N(treatment)=76, N(control)=69	(.001)	(.001)
13. Exclude districts with French forces (FFA) reduction 1991-2001	-.004***	-.004***
N=4,620, N(treatment)=80, N(control)=85	(.001)	(.001)
14. Weight by district population	-.002***	-.002***
	(.001)	(.001)
15. Cameron-Gelbach-Miller two-way clustering	-.004*	-.003**
	(.002)	(.002)
16. Cluster standard errors by labor market region †	-.004***	-.003***
	(.001)	(.001)
State by year dummies	Yes	Yes
District x time trends	Yes	Yes
District x time^2 trends	No	Yes

Notes: All regressions include district and year fixed effects and a control variable for the exchange rate effect (see table 3). Robust std. errors clustered at district level in parentheses. †In this specification, standard errors are clustered at the higher aggregation level of labor market regions. * Significant at 10%, ** at 5%, *** at 1%.

Table 8: Estimated impact of U.S. military withdrawal on gross daily wages, 1975-2002

Dep. variable: Real wage (log)	(1)	(2)	(3)	(4)
log U.S. forces	.001	.001**	.000	.000
	(.001)	(.000)	(.000)	(.000)
R^2	.387	.387	.388	.388
Other covariates:				
State by year dummies	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes
N	8,839,146	8,839,146	8,839,146	8,839,146

Notes: Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects and a control variable for the exchange rate effect (see table 3). Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Table 9: Impact of U.S. military withdrawal on gross daily wages according to age and education groups

Dep. Variable: Real wage (log)	(1)	(2)	(3)	(4)	(5)	(6)
9-1 Results according to age group						
	25-35 yrs.		35-45 yrs.		45-55 yrs.	
log U.S. forces	-.000	-.000	.001	.001	.001**	.001**
	(.000)	(.000)	(.001)	(.001)	(.001)	(.001)
R^2	.298	.298	.390	.390	.436	.436
N	3,448,330	3,448,330	2,962,646	2,962,646	2,428,170	2,428,170
9-2 Results according to education group						
	High		Medium		Low	
log U.S. forces	-.000	-.000	.000	.000	.001**	.001**
	(.001)	(.001)	(.000)	(.000)	(.000)	(.000)
R^2	.400	.400	.372	.372	.342	.343
N	937,829	937,829	7,230,040	7,230,040	1,609,106	1,609,106
Other covariates:						
State by year dummies	Yes	Yes	Yes	Yes	Yes	Yes
District x time trends	Yes	Yes	Yes	Yes	Yes	Yes
District x time^2 trends	No	Yes	No	Yes	No	Yes
N	5,096	5,096	5,096	5,096	5,096	5,096

Notes: Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects and a control variable for the exchange rate effect (see table 3). Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Table 10: Impact of U.S. military withdrawal on gross daily wages according to industry

Dep. Variable: Real wage (log) according to industry		(1)	(2)
		1 Basic materials	
log U.S. forces		-.000	-.001
		(.001)	(.001)
	R^2	.497	.497
	N	974,206	974,206
		2 Investment goods	
log U.S. forces		.000	.001**
		(.000)	(.000)
	R^2	.498	.498
	N	2,681,760	2,681,760
		3 Food and consumption goods	
log U.S. forces		-.000	-.00002*
		(.000)	(.000)
	R^2	.366	.366
	N	1,136,738	1,136,738
		4 Construction	
log U.S. forces		.000	.000
		(.000)	(.000)
	R^2	.327	.327
	N	1,243,583	1,243,583
		5 Retail/Repair	
log U.S. forces		-.000	.000
		(.000)	(.000)
	R^2	.287	.287
	N	1,095,586	1,095,586
		6 Transport/Information	
log U.S. forces		.0002*	-.0003***
		(.000)	(.000)
	R^2	.242	.242
	N	634,314	634,314
		7 Corporate services	
log U.S. forces		-.001	-.001
		(.001)	(.001)
	R^2	.387	.387
	N	802,316	802,316
		8 Private household services	
log U.S. forces		-.000	.000
		(.001)	(.001)
	R^2	.424	.424
	N	270,643	270,643
Individual level covariates		Yes	Yes
Other covariates:			
State by year dummies		Yes	Yes
District x time trends		Yes	Yes
District x time^2 trends		No	Yes

Table 11: Estimated impact of U.S. military withdrawal on employment, wages, unemployment, net migration, 1985-2002

Dep. Variable	(1)	(2)	(3)
	A. Total employment (log)		
log U.S. forces	-.006***	-.005***	-.003***
	(0.002)	(0.002)	(0.001)
	B. Real wage (log)		
log U.S. forces	.000	.000	-.000
	(.000)	(.000)	(.000)
	C. Unemployment rate		
log U.S. forces	.063**	.063**	.058***
	(.028)	(.028)	(.022)
	D. Net migration share		
log U.S. forces	-.003	-.008	-.015
	(.008)	(.007)	(.011)
Other covariates:			
State by year dummies	No	Yes	Yes
District x time trends	No	No	Yes
District x time^2 trends	No	No	No
N	3,276	3,276	3,276

Table 12: Dynamic pattern of adjustment effects, 1985-2002

	(1)	(2)	(3)	(4)	(5)
Dep. Variable	Total empl. (log)	Real wage (log)	Unempl. (log)	Unempl. rate	Net migr. share
WD announcement$_{t-5}$	.000	-.000	-.003	-.031**	.002
	(.001)	(.000)	(.002)	(.016)	(.010)
WD announcement$_{t-4}$	-.000	-.000	.001	-.004	.000
	(.001)	(.000)	(.003)	(.016)	(.013)
WD announcement$_{t-3}$	-.000	.000	.003	.014	.008
	(.001)	(.000)	(.003)	(.015)	(.014)
WD announcement$_{t-2}$	.000	.000	.005*	.021	.007
	(.001)	(.000)	(.003)	(.016)	(.014)
WD announcement$_{t-1}$	.000	.000	.005*	0.031*	-.020
	(.001)	(.000)	(.003)	(.018)	(.012)
WD announcement$_{t0}$	-.001	.000	.004	.019	-.011
	(.001)	(.000)	(.003)	(.018)	(.014)
WD announcement$_{t+1}$	-.002**	.000	.006*	.037*	-.001
	(.001)	(.000)	(.003)	(.021)	(.013)
WD announcement$_{t+2}$	-.002**	.000	.008**	.065**	-.018
	(.001)	(.000)	(.004)	(.029)	(.019)
WD announcement$_{t+3}$	-.003***	.000	.009*	.090**	-.043**
	(.001)	(.000)	(.005)	(.036)	(.021)
WD announcement$_{t+4}$	-.004***	-.000	.010**	.120***	-.038
	(.001)	(.001)	(.005)	(.040)	(.026)
WD announcement$_{t+5}$	-.005***	-.000	.011*	.133***	-.034
	(.001)	(.001)	(.005)	(.041)	(.023)
WD announcement$_{t+6}$	-.004***	-.000	.010*	.132***	-.027
	(.001)	(.001)	(.006)	(.047)	(.024)
WD announcement$_{t+7}$	-.003**	-.000	.009	.119**	-.032
	(.001)	(.001)	(.007)	(.055)	(.026)
WD announcement$_{t+8m}$	-0.000	.001	.010	.079	-.013
	(.002)	(.001)	(.008)	(.071)	(.032)
Other covariates:					
State by year dummies	Yes	Yes	Yes	Yes	Yes
District x time trends	Yes	Yes	Yes	Yes	Yes
R^2	.999	.376	.984	.948	.716
N	3,276	3,276	3,276	3,276	3,276

Notes: All regressions include district and year fixed effects. Robust std. errors clustered at district level in parentheses. The WD announcement dummies are defined relative to the year of the first announcement of the U.S. withdrawal for a district, $t = 0$. * Significant at 10%, ** at 5%, *** at 1%.

B Figures

Figure 1

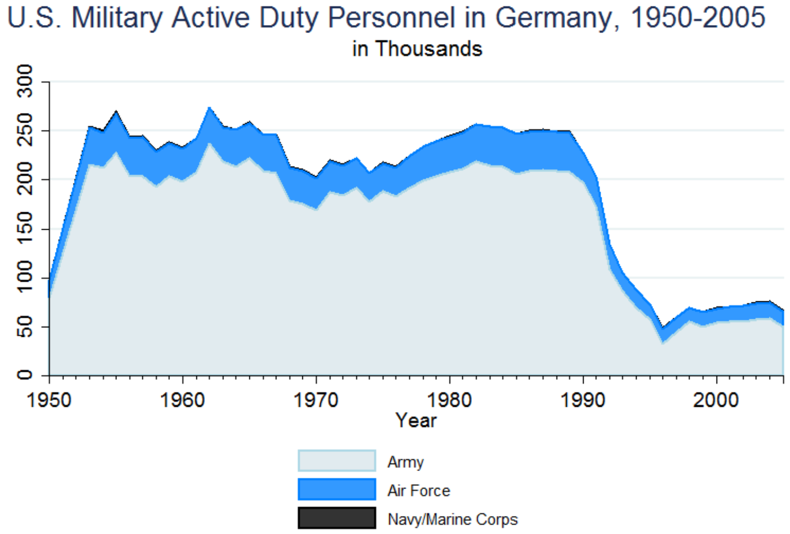


Figure 2

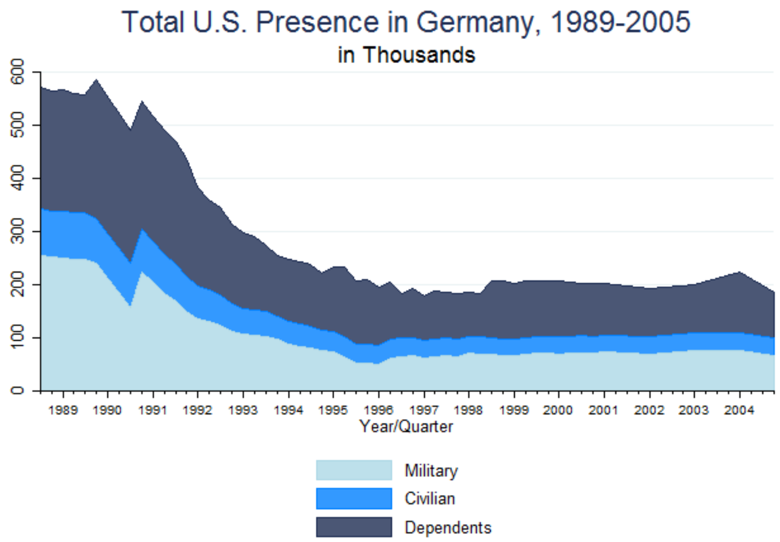


Figure 3: U.S. Military Bases in Germany, 1990 and 2002

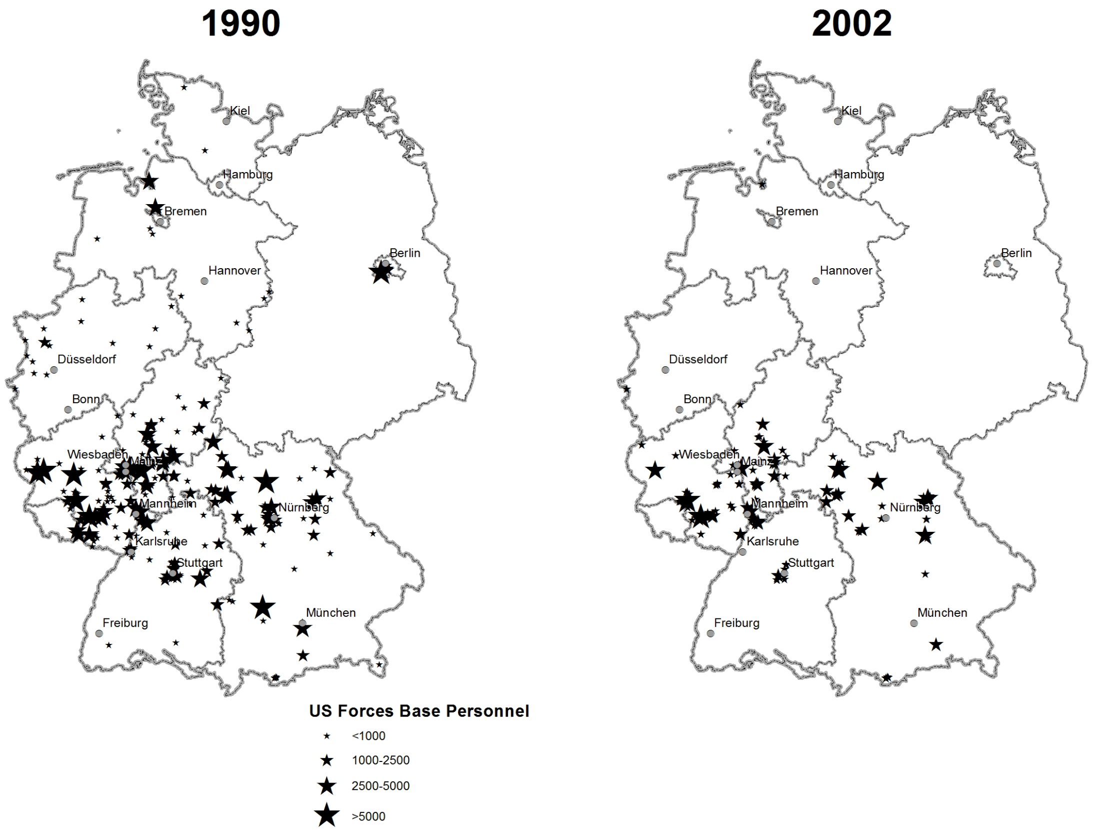


Figure 4

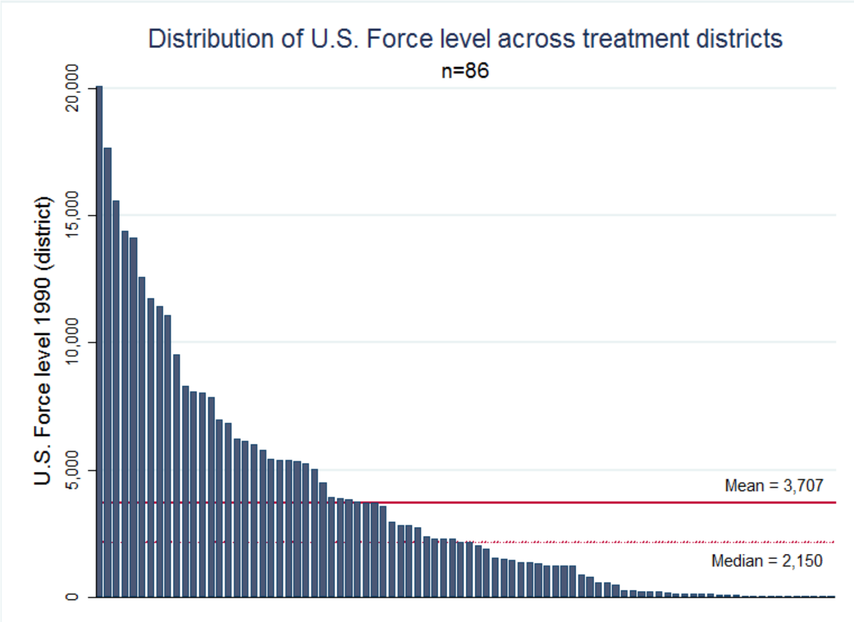


Figure 5: Treatment and control districts in baseline sample

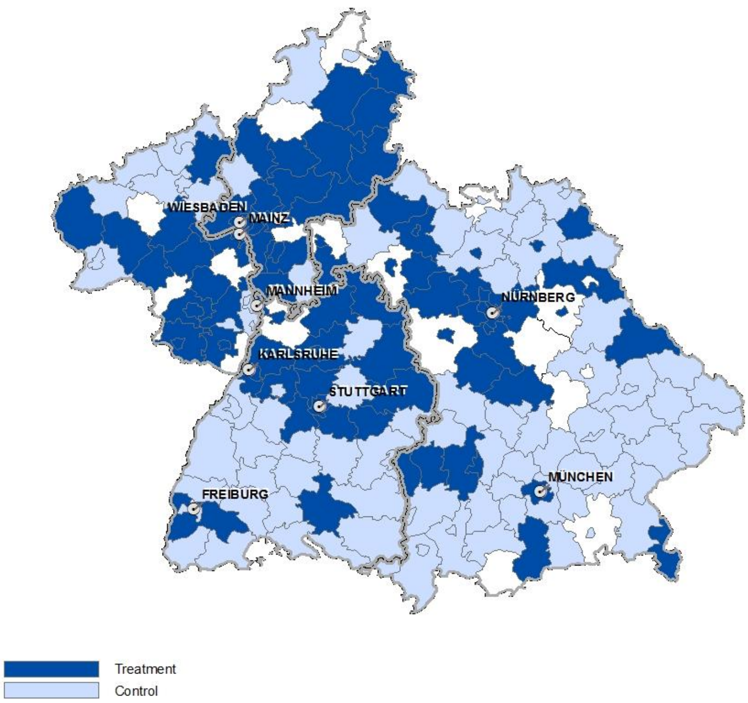
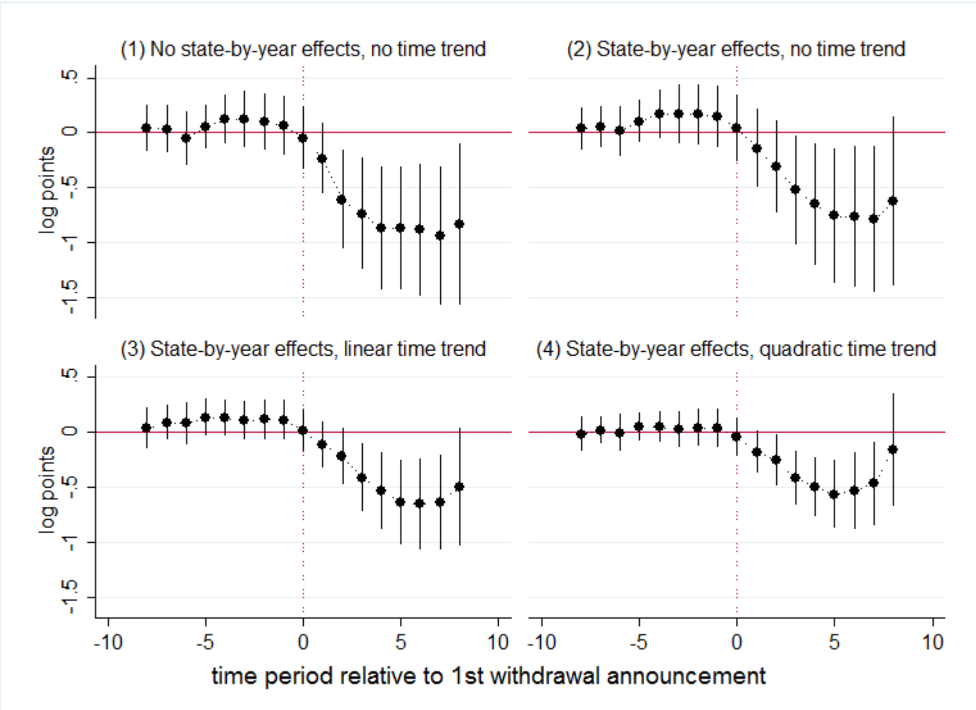


Figure 6: Dynamic pattern of impact of U.S. military withdrawal on total employment at district level



Notes: See text and table 6 for details. Vertical bands represent ± 1.96 times the standard error of each point estimate.

Figure 7: Average yearly USD/DM exchange rate

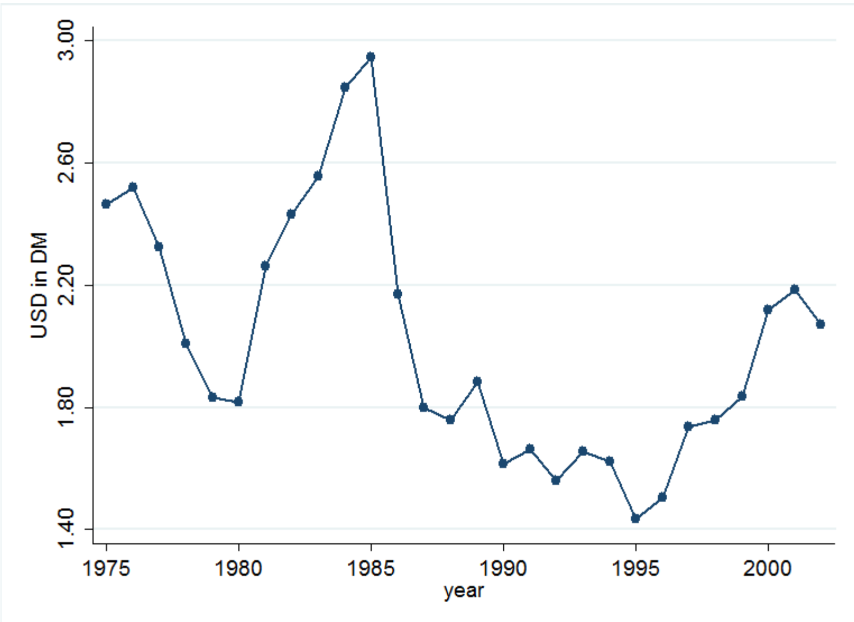
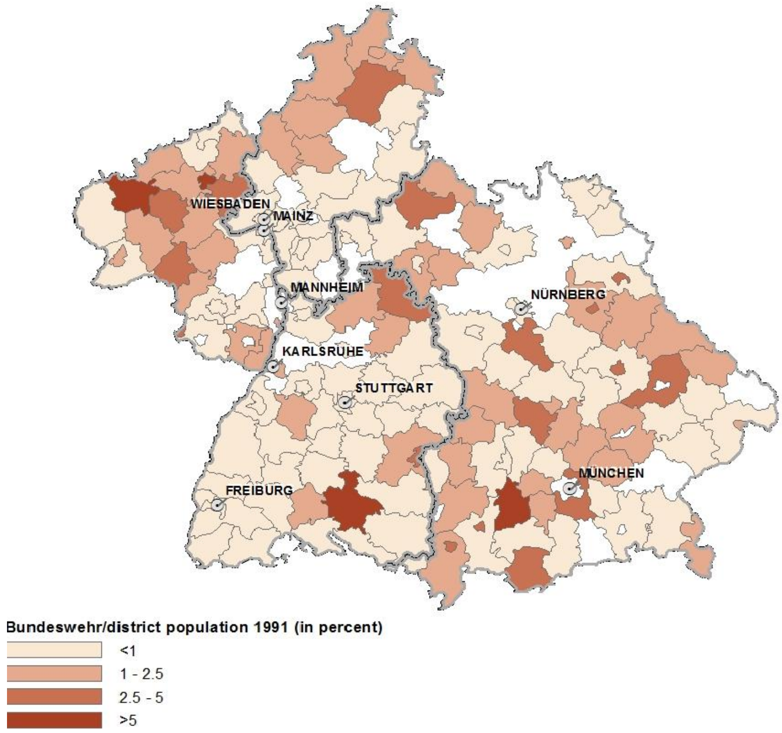


Figure 8:
A.(top) - Presence of German Armed forces (Bundeswehr) as share of district population, 1991
B. (bottom) - Districts with cuts in level of German Armed forces, 1991-2002



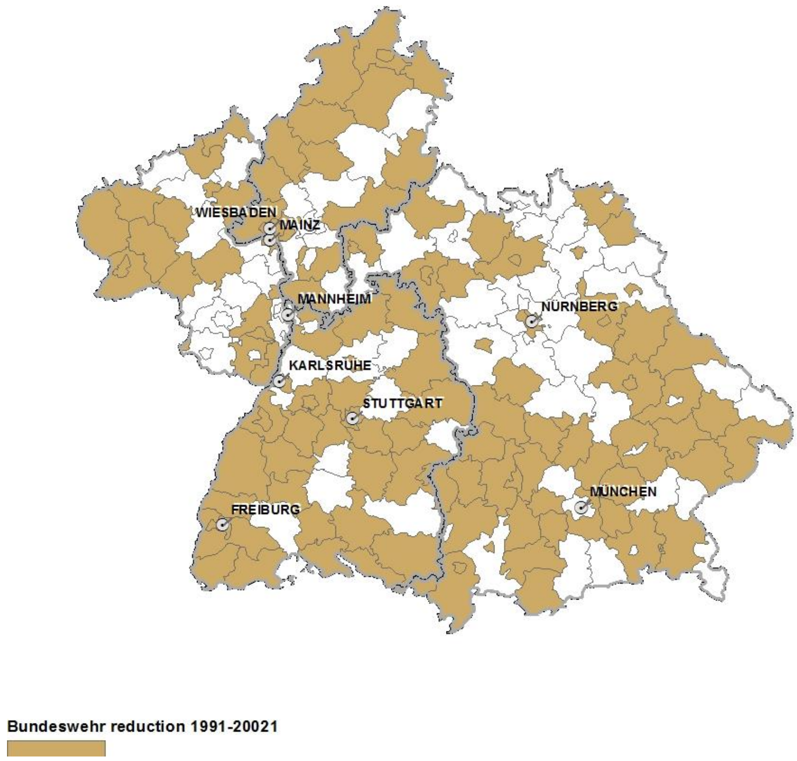
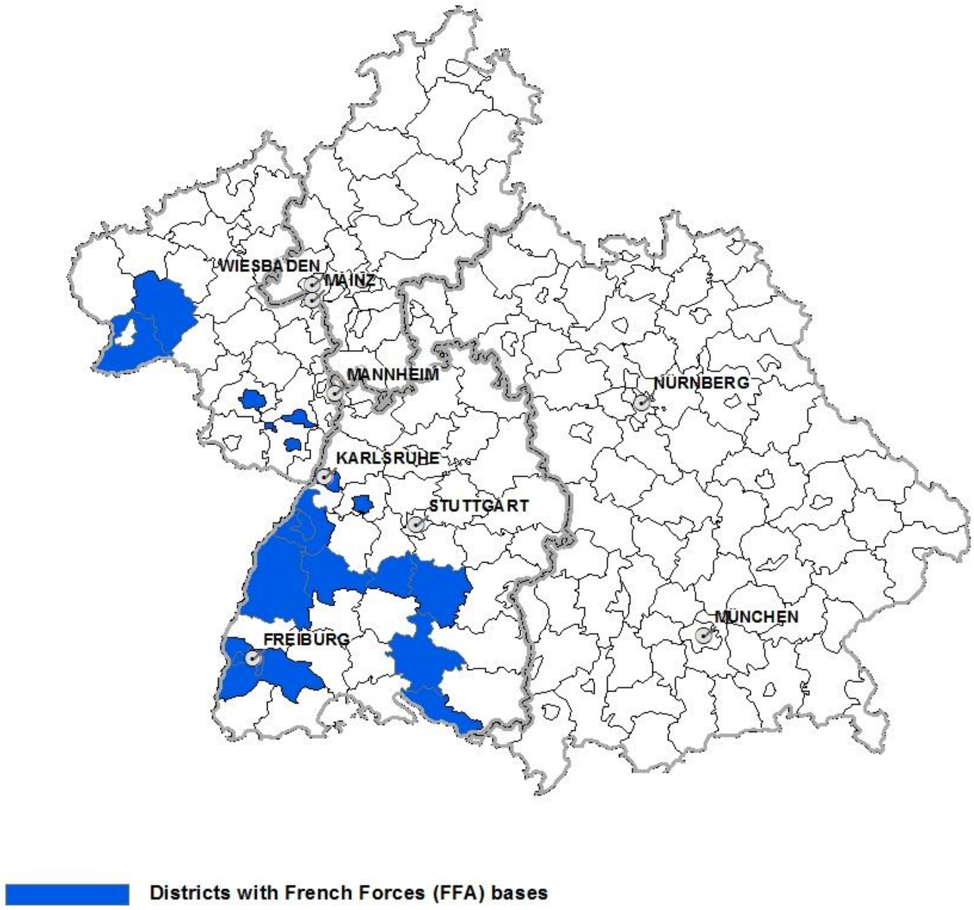


Figure 9: Districts with French force (FFA) bases



C Data Appendix

C.1 Data on U.S. Military forces in Germany

In the paper, we use data from a newly compiled database on the presence of U.S. military forces in Germany. As the primary original source, we drawn on administrative records from the U.S. Department of Defense, the so-called "U.S. Base Structure Reports" (U.S. Department of Defense, 1988-2009). Section 115 of title 10, U.S. Code, stipulates that as part of the annual budget process, the U.S. Secretary of Defense is required to submit an annual report to Congress that details the base structure both in the U.S. and abroad. Over the course of the recent decades, the exact reporting requirement as well as the format in which the U.S. Department of Defense fulfills this requirement has been subject to some modifications. However, starting with the report for the 1990 fiscal year, several editions of the report identify each individual U.S. base and installation in Germany and provide (in addition to information on the type of military use and the total acreage) manpower numbers of the assigned active duty military personnel. While for the purpose of our analysis, our dataset would ideally comprise manpower observations at the base-year level for the entire period under consideration (1975-2002), we primarily use the data from the 1990, 1992 and 2002 fiscal year editions.[41] In addition, we include data for 1995 that was directly gathered from the U.S. forces in Germany by Cunningham and Klemmer (1995) for their descriptive report of the ongoing base realignment process, and that is provided in a format compatible to the earlier official reports. Since the genuine purpose of the Base Structure Reports (as with most other data from administrative or private sources) was not to provide a consistent cross-section or time series for an econometric analysis, there are at least three limitations of the data that we will now briefly discuss in turn.

Firstly, the manpower data in the reports provides figures for the *authorized* number of personnel for the subsequent U.S. fiscal year from which actual force levels could deviate. However, comparing authorized versus actual figures reveals that deviations are small in the aggregate. While this does not exclude larger deviations at the micro level of individual bases, we do not have any evidence that these deviations differ systematically, for example according to military branch or base size, and thus introduce a type of measurement error that could bias the results towards finding a spurious effect.[42]

Secondly, the manpower data in the Base Structure Reports is compiled separately for the U.S. Army Europe (USAREUR) and the U.S. Air Force Europe (USAFE) in Germany and includes a breakdown according to military, civilian and other personnel which includes employees of full-time contractors working on the base and the local German nationals directly employed by the base. In our primary analysis, we combine the three categories into one total measure of the U.S. personnel numbers on the regional level. This definition disregards the fact that the various groups have different pay scales and are likely to spend different amounts of their income

[41] For 1999 and 2001, the only other years for which a report containing information at the base level is available, the data exhibit some obvious omissions of large, active bases in Germany that are likely due to a switch from a manual to an automatic data gathering process by the U.S. Department of Defense.

[42] In fact, the usual attenuation bias would rather work against finding an effect significantly different from zero.

in the local economy.[43]

Thirdly, the regional distribution of the U.S. personnel in the data could be subject to some minor measurement error as the strength levels are sometimes “rolled up by parent unit”(U.S. Department of Defense, *FY1999*, p.4). Starting with the report for the 1999 fiscal year, the U.S. Department of Defense also reported data for individual overseas bases only if the site has “more than 10 acres OR a plant replacement value exceeding one million dollars.” (ibid, p.3). The 2002 edition of the report (which we use for the construction of our primary withdrawal treatment indicator) lists 47 such smaller Army sites and 26 Air Force sites. While these numbers do not seem negligible, the combined authorized personnel at these sites constituted less than 0.7% of the total force level[44], so our estimation results are unlikely to be affected in any important way. In a related aspect, earlier attempts to quantify the regional U.S. presence in Germany were also hindered by the fact that manpower data were often aggregated at the military community or garrison level that could stretch large regions across several districts or in some cases even beyond federal state boundaries with the inclusion of so-called “remote sites”. Our data allow us to overcome this shortcoming: the information on the exact name of the base and the nearest city from the Base Structure Reports enables us to augment the original base information with its exact address and geographic position, using information from internet directories that include historic maps and satellite photos (from Elkins, nd). We can thus attribute the manpower data with great precision and confidence to the districts in which the respective sites are located.

C.2 Data on Timing of U.S. Base Realignments

Our empirical approach requires an accurate identification of when the withdrawal begins at the district level. As outlined in the paper, we date the start of the post-treatment period at the district level as the first reporting date after the first base closure was announced for a given district. The announcement dates at the base level were collected and verified drawing on three different data sources:

(1) An official list compiled by the History Office of the U.S. Army Europe that details which closure “rounds” affected which site and specifies the projected or actual closure dates (USAREUR History Office, nd);

(2) A collection of official news releases issued by the U.S. Department of Defense which allowed us to match the closure round numbers to exact calendar dates;

[43]Based on information requested at the U.S. Army and Air Force Headquarters in Germany, Bebermeyer and Thimann (1990) detail that in 1987, air force personnel in Germany earned on average 80% more than their U.S. Army counterparts which they ascribe to “(a) the substantially higher proportion of officers and higher-ranking soldiers within USAFE, and (b) the higher average income of each USAFE rank as a result of the greater length of service and the many supplementary payments, that is, for hazardous work or weekend and night shifts.” (Ibid, pp. 104-105) In addition, USAFE service members are more likely to live off base and, due to their seniority, have on average more dependent family members, which further increases the share of their income which they spend in the local economy compared to the U.S. Army personnel.

[44]The comparison with earlier editions shows that many of these sites are likely to be small radio or radar sites and small storage compounds, some of which are also located outside the four federal states on which we focus our analysis.

(3) A complete scan of historical newspaper accounts from the "Stars & Stripes", the official news outlet of the U.S. Department of Defense, around the dates when drawdown announcements were supposed to have taken place based on the two previous data sources.

The available data allowed us to identify and confirm all but one public announcement date of all the U.S. base realignments occurring in Germany.[45]

C.3 Data on Base Realignments by the German Armed forces (*Bundeswehr*)

As we explain in the body of the article, other economic shocks operating at the district-year level across the four states would violate our identification of the effect of the U.S. withdrawal. We therefore gather data on the regional base realignments implemented by the German armed forces over the time period 1991-2002 in order to check the robustness of our estimation results once we exclude districts where U.S. reductions coincide with German base realignment. The data on the German Armed forces is compiled from two different data sources:

(1) Luber (1991) includes an appendix of comprehensive data from three official reports by the German Federal Ministry of Defense in 1991 on the existing force levels and the planned reductions in the following years at the level of local postal codes

(2) In 1995, the German Federal Ministry of Defense published an updated plan for the realignment of the German armed forces (Bundesministerium der Verteidigung, 1995).

The data from both sources are combined to identify all districts in the four states in our study where reductions by the German armed forces took place between 1991 and 2002.[46]

C.4 Data on Base Realignments by French forces in Germany (*Forces Françaises en Allemagne, FFA*)

Similar to the U.S. and German army base realignments, we also obtained data on the location and closure of bases that were used by the French forces in Germany. The data on the French forces is compiled from three complimentary data sources:

(1) The early offficial account by the German government (Deutscher Bundestag, 1991b) lists 68 bases used by the FFA, hosting an aggregate force of 44,200 up to the start of the withdrawal and realignment in the early 1990s.

[45] The only exception where the exact calendar date could not be ascertained is the Base Closure Round 21 that occurred sometime between the previous round, dated August 1, 1995, and the subsequent round, dated February 13, 1997. However, Round 21 affected only 5 sites, of which 2 were located in Bremerhaven and thus are not in our estimation sample. For the other 3 sites located in the medium to large cities of Darmstadt, Frankfurt and Fürth, this announcement round was not the first to affect the respective district, thus not altering our empirical analysis for these districts.

[46] The German Ministry of Defense enacted further rounds of realignments in 2001 and 2004 (Bundesministerium der Verteidigung, 2001a,b, 2004). We do not include these realignments in our analysis as they were implemented after the end of our sample period.

(2) Guth (1991) provides some data on the regional breakdown of the French troop levels, clustered according to major garrison city. She puts the total number of French soldiers stationed in Germany at 52,000 in 1987 and estimates that another 30,000 civilian workers supported the French troop presence.

(3) A dedicated entry on Wikipedia (2011) provides a comprehensive list of all (active and closed) foreign military bases in Germany that was used to validate the information obtained from sources (1) and (2).

As data on the exact local French force levels according to base and their development over time was not available, we restricted our attention to identifying the 17 districts in Rhineland-Palatinate and Baden-Württemberg where the French forces maintained a presence according to any of the sources (1)-(3) and excluded them from our estimation sample in one of the robustness checks in order to document the fact that their inclusion did not bias our estimation results.

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