Essays on the Impact of Economic Shocks in Local Labor Markets

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Abstract

This thesis consists of four essays that contribute to the empirical analysis of local labor markets. The first essay exploits the massive withdrawal of U.S. Armed Forces in the aftermath of the German Reunification as a natural experiment that enables the identification of the causal impact of local labor demand shocks. It introduces a novel dataset which has been compiled from a range of official historical sources and details the evolution of the U.S. manpower levels at the disaggregated regional level and thereby enables the measurement how U.S. base closures affected the demand for local non-tradable goods and services. The results from the empirical analyses suggest that the associated drop in local labor demand caused a significant loss of private sector employment and generated a subsequent rise in local unemployment rates. In contrast, local wages and aggregate migration patterns do not exhibit any significant responses. The second essay explores the rigidity of wages in local labor markets in response to the U.S. base closures in greater depth. The presence of two types of institutions and their interplay are characterized as potential sources of heterogeneities in downward wage rigidities between segments of the local labor market. The presence of a works council determines the strength of worker influence in firm level decision-making whereas the German Trade and Crafts Code acts as an entry regulation that gives rise to the existence of product market rents. While in isolation these two institutions do not seem to alter the pattern of insignificant wage adjustments, their interaction is found to introduce a channel for small downward wage adjustments from the rent-sharing bargaining in establishments with weak worker influence. The third essay is concerned with the change in local crime rates as a key component of the broader socio-economic outcomes of the U.S. presence and withdrawal in the local German communities. The empirical findings suggest that the drawdown of the U.S. military presence can be related to large and significant drops in the rate of drug and sex offenses in the local communities. Finally, the fourth essay provides an empirical analysis of the diverging patterns of employment in temporary help services across labor markets in Germany over the last 30 years. The differential growth pattern both at the level of occupations and across regional labor markets are found to be related to the initial intensity of routine and non-routine manual tasks.

Keywords:
Labor economics, local labor markets, demand shock, natural experiment, base closures, employment, wages, regulation, crime, temporary help services

Zusammenfassung

Diese Dissertation besteht aus vier Aufsätzen, die einen Beitrag zur Literatur über die empirische Analyse von lokalen Arbeitsmärkten leisten. Der erste Aufsatz nutzt den Abzug eines Großteils der amerikanischen Stationierungsstreitkräfte in Deutschland seit 1990 als ein natürliches Experiment, das die Identifikation der kausalen Effekte von Nachfrageschocks in lokalen Arbeitsmärkten ermöglicht. Als Datengrundlage dient ein originärer und neu zusammengestellter Datensatz mit detaillierten Informationen über die Veränderungen der Personalstärke der U.S. Stützpunkte in Deutschland auf der regionalen Ebene, der zur Analyse der Nachfragewirkungen des Abzugs der U.S. Streitkräfte für nicht-handelbare Güter und Dienstleistungen verwendet wird. Die empirischen Ergebnisse belegen, dass der Abzug zu einem signifikanten Rückgang der Beschäftigung in der lokalen Privatwirtschaft und einem nachfolgenden Anstieg in der lokalen Arbeitslosenrate führte. Im Gegensatz dazu weisen die Schätzergebnisse keine Evidenz für signifikante Anpassungen in den lokalen Arbeitslöhnen oder für den Saldo der regionalen Wanderungsbewegungen aus. Der zweite Aufsatz vertieft die Frage der lokalen Lohnrigiditäten angesichts des Nachfrageschocks durch die U.S.- Standortschließungen. Der Einfluß von zwei Institutionen wird als mögliche Quelle von heterogenen Lohnanpassungen in lokalen Arbeitsmärkten identifiziert. Insbesondere werden die Rolle der Arbeitnehmervertretung durch Betriebsräte sowie die Regulierung des Zugangs zu Produktmärkten und die damit verbundene Entstehung von Produktmarktrenten thematisiert. Die Ergebnisse der empirischen Analyse ermöglichen die Schlussfolgerung, dass der Einfluss der beiden Institutionen isoliert mit keinen heterogenen Lohnanpassungen verbunden ist. Es zeigt sich jedoch, dass das Zusammenwirken beider Dimensionen in Arbeitsmarktsegmenten, die gleichzeitig durch eine geringe Verhandlungsmacht der Arbeitnehmer auf Betriebsebene sowie die Existenz von Produktmarktrenten gekennzeichnet werden können, verbunden ist mit differenziellen Lohnreduktionen. Der dritte Aufsatz erweitert die Analyse der Folgen des amerikanischen Truppenabzugs um die Frage nach möglichen weiteren sozialen Auswirkungen, die die Wohlfahrt in den betroffenen Regionen beeinflussen können. Als zentrales Element für derartige Effekte wird die Entwicklung der lokalen Kriminalitätsrate untersucht. Die Ergebnisse zeigen, dass der Truppenabzug mit einem Rückgang der Kriminalität insbesondere von Drogen- und Sexualstraftaten verbunden ist. Der vierte Aufsatz untersucht die langfristige Entwicklung der Zeitarbeit in den regionalen Arbeitsmärkten in Deutschland in den vergangenen 30 Jahren. Basierend auf der Klassifikation der Beschäftigung nach detaillierten Tätigkeitsprofilen zeigt sich, dass die anfängliche Verteilung der Beschäftigungsanteile für manuelle Nicht-Routine- und insbesondere für Routine-Tätigkeiten eine starke Vorhersagekraft für das unterschiedliche Beschäftigungswachstum von Zeitarbeit in Deutschland besitzt.

Schlagwörter:
Arbeitsmarktökonomik, lokale Arbeitsmärkte, Nachfrageschock, natürliches Experiment, Standortschließungen, Beschäftigung, Löhne, Regulierung, Kriminalität, Zeitarbeit

- In memoriam Willy Plöger
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1 Introduction

> *"The topic of local labor markets should be of great interest to labor economists for two reasons. First, the issue of localization of economic activity and its effects on workers welfare is one of the most exciting and promising research grounds in the field. This area, at the intersection of labor and urban economics, is ripe with questions that are both of fundamental importance for our understanding of how labor markets operate and have deep policy implications. [...] Second, and more generally, the issue of equilibrium in local labor markets should be of broader interest for all labor economists, even those who are not directly interested in economic geography per se."*
>
> *Moretti (2010), p.1239*

1.1 Economic shocks and local labor markets

The question how the economic and social fortunes of local communities and entire regions within countries are affected by adverse economic shocks of various types has been a perennial source of interest both to policymakers and to researchers in many academic disciplines.

From the perspective of economic policy, this question has been at the core of the debate about the need for many subsidy and transfer programs directed at potentially disadvantaged regions, both at the national and supranational level. While many doubts about their effectiveness have been voiced, the sheer size and fiscal importance of these programs is large. For the U.S., Moretti (2010) reports that state and local governments spend $30-40 billion per year on local development policies, while the federal government spends $8-12 billion. Within the European Union, Dupont and Martin (2006) state that one third of the EU budget is spent on regional policies, in addition to an estimated 1% share of GDP on average spent by individual member states. In Germany, the focal country in this thesis, the size of the direct regional subsidy grants for investment and infrastructure support (*Gemeinschaftsaufgabe "Verbesserung der regionalen Wirtschaftsstruktur", (GRW)*) amounted to a total of 63 billion EUR over the 1991-2010 period (Bundesamt für Wirtschaft und Ausfuhrkontrolle, 2011). However, this does not yet account for the large regional component of transfers and subsidies embedded in many other policy instruments (e.g., the financial equalization scheme between the German federal states, as well as defense, energy, traffic and research infrastructure programs), as highlighted by a recent report commissioned by the German Ministry of Economics (see BAW Institut für regionale Wirtschaftsforschung GmbH Bremen and IW Consult GmbH Köln, 2009).

A key criterion both in the initial assessment of the potential need for transfers and subsidies, as well as in the later evaluation of the success of such programs is the regional labor market performance. At the same time, the measurement of the causal effects of local demand shocks has been for a long time an elusive area of research in labor economics. On the one hand, local labor markets seem to provide a useful real-world laboratory to understand the incidence of economic shocks as the within-country perspective allows to isolate the underlying economic mechanisms from the confounding effects from heterogeneous (and often unobserved) institutions and restrictions on the mobility of labor and capital that has plagued empirical cross-country analyses on this topic (Moretti, 2010). On the other hand, the measurement of exogenous shocks that affect some regions within a country, but not others, has proven to be difficult.

Traditionally, labor economists have relied on simple techniques that sought to isolate local labor demand shocks by analyzing deviations in local employment series from national trends and the classification of such deviations into labor demand and supply shocks based on the observed coincident movement of local wages (see, among many others, Topel, 1986; Bartik, 1991; Blanchard et al., 1992). A major drawback of this approach is that the local labor demand shocks are only a residual and hence difficult to interpret. Another often used approach is based on Bartik's (1991) idea to instrument employment changes for U.S. states by applying national industrial level growth rates to the state industry composition in a base period. Of course, the validity of this instrument is based on the rather strong assumptions that state supply shocks are uncorrelated both with national industry growth rates and with the initial state industry compositions.

Against the backdrop of these limitations, many researchers have subsequently turned towards specific, but plausibly also more exogenous shocks to local labor demand. Examples include incidents of large variations in energy prices (e.g., Black et al., 2002, 2005a; Marchand, 2012), export price levels (e.g., Pennington-Cross, 1997; Hollar, 2011), as well as the competitive tendering procedure for large industrial plants (Greenstone and Moretti, 2003; Greenstone et al., 2010a) and natural disasters like hurricanes (Belasen and Polachek, 2008).

For Germany, the analysis of local effects of similar types of demand shocks has remained an understudied area which might be partly explained by the lack of detailed data at the local level. This thesis aims at partly filling this gap by exploiting the quasi-natural experiment of the massive withdrawal process of the U.S. Armed Forces in Western Germany after the fall of the Iron Curtain and the Berlin Wall in 1989. The historical "stroke of luck" of the following German Reunification has not just marked a fortunate turning point in the political and economic history of the country, but has also been a source of inspiration to many economists, providing them with a rare set of quasi-natural experiments that has enabled the identification of causal effects for a diverse set of questions.[1] In this spirit, the doctoral thesis contributes to the literature by introducing a novel dataset that includes

[1] Some prominent and recent examples include Alesina and Fuchs-Schündeln (2007); Prantl and Spitz-Oener (2009); Redding et al. (2011); Burchardi and Hassan (2011).

detailed information at the local level about the size of the initial U.S. deployment and the exact timing and extent of the subsequent base realignments. The empirical analysis of the incidence of the local demand shocks from the U.S. drawdown on labor market outcomes as well as the analysis of the impact of the U.S. base closures on other socio-economic outcomes at the local level constitute the major part of this work.

1.2 Outline of this thesis

This thesis includes four essays on various aspects of local labor markets in Germany. Chapter 2 is the main analysis of how the economic shock of the U.S. base closures impacted on the local labor markets in West Germany. It introduces the historical background of the U.S. military presence in Germany and describes the novel data set on the U.S. military in Germany that we compiled at the detailed local level. The U.S. base closures are characterized as a surprise adverse shock that led to a drop in product demand for non-tradable goods and services in the regions that hosted the U.S. military. With regards to the crucial exogeneity assumption that enables the identification of the causal effects of the U.S. base closures, we provide ample evidence collected both from a broad range of official and historical sources which underline that the selection of the bases which were closed was governed exclusively by U.S. military considerations. We exploit this quasi-natural experiment to study how the local demand shock was absorbed by different margins of adjustment in the local labor market, including employment, wages, unemployment and net migration. Our results suggest that the base closures caused a notable loss of private sector employment in the affected regions. Our baseline estimates suggest that on average, a reduction of U.S. force levels by 100 servicemen led to the loss of 4.6 full-time private sector jobs in the local economy. Consistently, the employment losses are concentrated among younger workers, those with low to medium education levels and those employed in private household services. Based on the analysis of the dynamic pattern of the effect on various outcomes, we also find evidence of a subsequent increase in local unemployment rates. In contrast, wages and net migration rates do not seem to play a significant role in the local adjustment process.

The analysis in chapter 3 uses the drop in local demand induced by the U.S. drawdown to revisit and investigate the question of wage adjustments in response to economic shocks in greater depth. The motivation for this study is the insight from the literature on wage formation that stresses the importance of product and labor market regulations and their interplay for the size and shape of the wage responses to economic fluctuations. Building upon the recent work of Prantl and Spitz-Oener (2011), we explore if labor market segments distinguished by the presence of works councils determining the strength of worker influence in firm decision-making as well as by the entry regulation of the German Trade and Crafts Code differ in their wage adjustment pattern. While in isolation neither of the two institutions seems to introduce any downward wage flexibility, their interplay in a labor market segment that is simultaneously characterized by weak worker influence

and the potential for sharing of product market rents arising from the restrictions on firm entry exhibits a differential wage effect with a significant negative response to the drop in local demand from the U.S. withdrawal. Notably, the evidence of this effect seems to be limited to the sector of private household services for which the analysis in chapter 2 also documented the largest employment adjustments.

In the study that forms the basis of chapter 4, we expand the perspective beyond the immediate labor market outcomes and investigate whether the withdrawal of U.S. servicemen could have also been associated with some positive effects for the local communities. The starting point of this analysis are anecdotal accounts by historians and economists alike who argue that the U.S. presence did not just bring about economic benefits to their German hosts, but also imposed social burdens with potentially non-negligible economic costs. One particular concern during the time of the stationing was the impact of the U.S. bases on local crime rates. The results suggest that the U.S. base closures are associated with a significant drop in the rate of drug offenses in the local communities by about 20 percent and also reduced the count of sex offenses. However, the total crime rate is not significantly affected. Finally, we document some evidence indicative of heterogeneous effects depending on the area type, with the drop in crime rates associated with the U.S. withdrawal being more pronounced in urban compared to rural districts.

The topic in chapter 5 differs from the previous chapters as the question and the research design are unrelated to the analysis of the U.S. base closure shock. In contrast to the analysis of the incidence of local labor demand shocks in the short and medium run, the analysis is concerned with the question if diverging patterns of the use of temporary help services (THS) across local labor markets in Germany can be traced back to long run differences in the structure of local labor demand. Building upon concepts of the task and trade-in-task literature, it first documents that the task content of occupations is a strong predictor whether some type of work can be shifted into temporary help services. At the level of regional labor markets, the results suggest that long-run differences in the intensity of routine and non-routine manual tasks in local employment have some robust explanatory power for the differential spread of THS employment across Germany in the last 30 years.

All four subsequent chapters are supposed to be self-containing and can be read independently. Chapter 2 and 3 are based on joint work with Alexandra Spitz-Oener. Chapter 4 is co-authored with Annemarie Paul. Chapter 5 is based on joint work with Alexandra Spitz-Oener and Hanna Wielandt.

2 The Impact of the U.S. Military Drawdown on Local German Labor Markets

2.1 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 federal states ("Bundesländer") in West Germany, but not in others. Specifically, we exploit the variation in the stationing and withdrawal of U.S. military forces in Germany after German reunification and the end of the Cold War at the district level 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-making 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 political lobbying: the U.S. Department of Defense decided on the details of the withdrawal process purely on

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

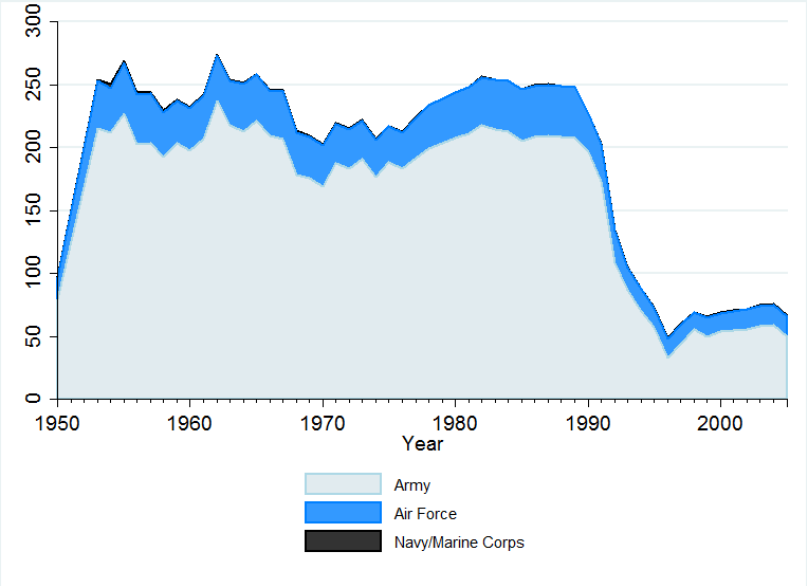
[2] It also allows us to improve on limitations that previous studies on the effects of base closures faced. For example, the Base Realignment and Closure Process (BRAC) in the U.S. and the realignment of the German Army are both likely to be influenced by strong local or regional stakeholders lobbying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

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 bases acted as employers of German civilian workers.

Although German civilian employees directly employed at the bases 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), the existing descriptive studies of the U.S. military presence were mostly 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]

Figure 2.1: U.S. Military Active Duty Personnel in Germany, 1950-2005



Source: U.S. Department of Defense (1950-2005), Table 309A; own calculations.

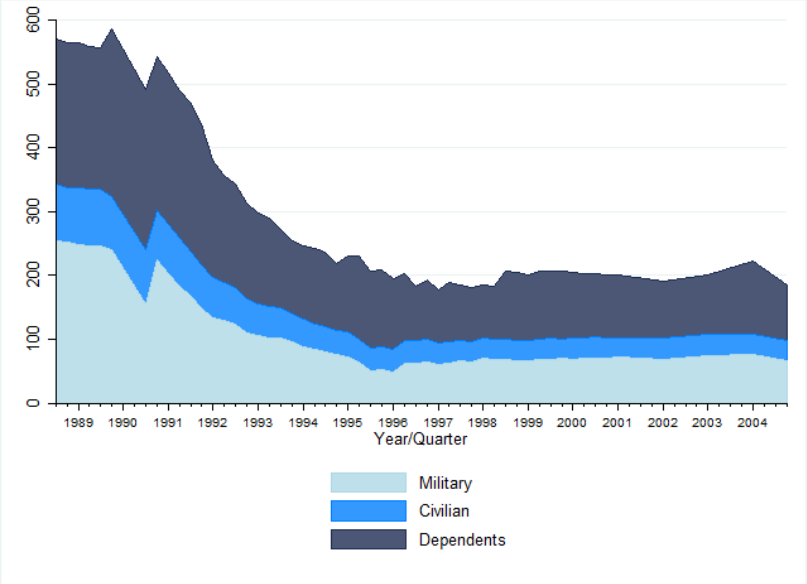
We focus on effects that are mainly driven by changes in channels one and two: at the end of the 1980s, there were about 250,000 U.S. servicemen stationed in Germany (see Figure 2.1). Together with the U.S. civilian personnel that the bases employed and the family members they brought along, the total U.S. presence in West Germany amounted to nearly 600,000 persons in 1989 (see Figure 2.2). 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,

[3] See, for example, Blien et al., eds (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.

which represents 2.9 percent in terms relative to the civilian district population.[5,6] The U.S. forces consumed mainly local, non-traded goods and services from German sources, whereas with respect to traded goods and services, the U.S. bases were mostly self-sufficient. Consequently, the U.S. withdrawal represented a large negative consumption shock to the affected regions which translated into an adverse shock to local labor demand.

Figure 2.2: Total U.S. Presence in Germany, 1989-2005



Source: U.S. Department of Defense (1950-2005), Table 309C; own calculations.

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 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

[5] See Table 2.1. 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 over dependence 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.

pronounced for local goods and services sectors that were prone to suffer most from the drop of local purchasing power, and were primarily borne by young and old, and by low- to medium-skilled workers. We also find evidence for a rise in local unemployment, whereas we do not find effects along the migration margin or in terms of downward adjustments in 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 separate these effects. 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.[7]

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.[8] The coal bust led to negative effects that are smaller than the positive effects during the boom.[9] 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

[7] Interestingly, Blanchard et al. (1992) also use military spending based on military prime contracts data at the state level as an alternative instrument for shocks to regional employment demand. In several recent contributions, Ramey and Shapiro have further extended and refined this approach and used a "narrative method" to construct a rich variable of defense shocks for the U.S. between 1939 and 2008 in order to provide estimates of the government spending multiplier. See Ramey and Shapiro (1998); Ramey (2011a,b).

[8] For a similar analysis using Canadian data see Marchand (2012).

[9] 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).

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,[10] 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 in our study are as rural or remote as the regions of main interest in these earlier studies.[11]

Our analysis also differs from these earlier studies with respect to the information set of the local economic actors. In the setting of most previous 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. (2010b), who study the regional industry-level employment and productivity effects from the awarding of "Million Dollar Plants". While their analysis provides an original identification design for regional spill-over effects by focusing 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 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 2.3 we present our estimation strategy, and Section 2.4 discusses the data. Section 2.5 reports our results, separately for regional employment, wages, unemployment and net migration, as well as several robustness checks. Section 2.6 provides a conclusion.

[10] 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.

[11] 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).

2.2 Historical background

2.2.1 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.[12]

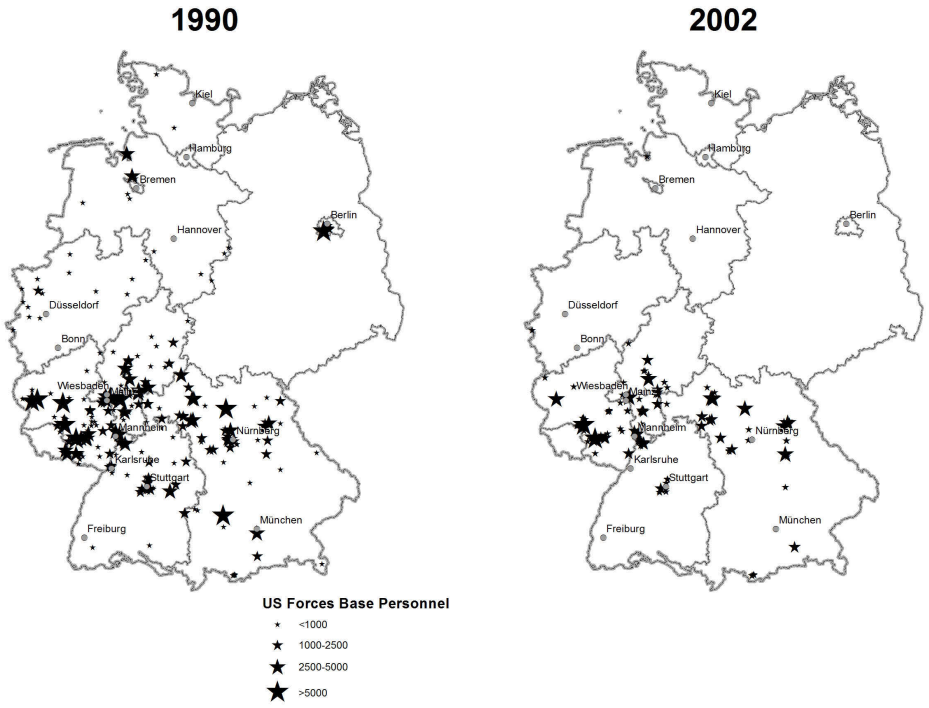
An estimated number of 1.9 million American soldiers were stationed on German soil at the end of World War II.[13] After the temporary reduction to less than 100,000 in the first years of the occupation up to 1950, the strength of the U.S. forces was consolidated to around 250,000 in the mid 1950s. Figure 2.1 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.[14] 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 2.2). The U.S. forces in Germany maintained over

[12] 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).

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

[14] The numbers in Figure 2.1 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.

Figure 2.3: U.S. Military Bases in Germany, 1990 and 2002



Source: Own calculations and illustration based on original data on U.S. forces in Germany. See text and appendix 6.1 for details.

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 2.3 illustrates the regional distribution and the relative personnel size of the U.S. bases across Germany in 1990.

2.2.2 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)* (see Cunningham and Klemmer, 1995) provide detailed insights into the planning and execution 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."[15]

On September 18, 1990, the Pentagon publicly announced the closure and realignment of 110 sites in Germany, starting the first phase of the withdrawal.[16] By 1996, another 20 rounds of base closures in Germany had been subsequently 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 only learnt about the imminent closures around the time of the public announcements by the U.S. forces in the news media.[17,18] The "drawdown shock" at the local level was further 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

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

[16] 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).

[17] 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."

[18] 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 & 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.

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.[19]

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 rather sudden "shocks" 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.

2.3 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 outcomes in districts with a U.S. presence and subsequent withdrawal with those in a group of control districts. In our simplest specification for employment, the empirical model estimated by OLS has the following form:

$$\log Y_{kt} = \alpha_k + \delta_t + \beta \times \underbrace{\log \text{U.S. Forces}_{kt} \times \mathbb{I}\left[t > Year_{k0}\right]}_{\text{"U.S. withdrawal"}} + \epsilon_{kt} \tag{2.1}$$

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 $Year_{k0}$, 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. Hence, the estimate for β captures the extent to which private sector employment in a district responds to the U.S. forces reduction.

In extensions to the specification of Equation 2.1, we estimate specifications that include dummies for state-by-year, and linear or quadratic district-specific time trends in order to

[19] 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.

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.[20]

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

$$\log W_{ikt} = \alpha_k + \delta_t + \beta \times \underbrace{\log \text{U.S. Forces}_{kt} \times \mathbb{I}\left[t > Year_{k0}\right]}_{\text{"U.S. withdrawal"}} + X_{ikt}\gamma + \epsilon_{ikt}, \tag{2.2}$$

where the subscript i denotes 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 dummies 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 standard errors can be misleading in the presence of serial correlation. They demonstrate that next to more complex approaches such as block bootstrap methods,[21] 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 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.[22] For our wage estimations, where we face the "Moulton" problem, we further confirm the robustness

[20] 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.

[21] See, e.g., Fitzenberger (1998), Conley (1999).

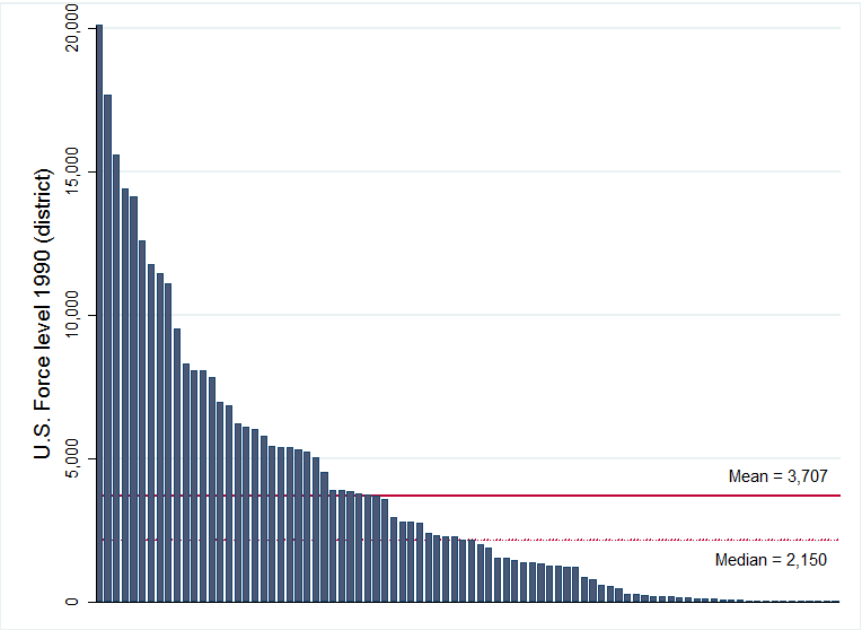
[22] Although the minimum required number of clusters cannot be easily determined as it depends on the application, Angrist and Pischke argue that the evidence from DD research on U.S. states suggests that more than 50 clusters should be sufficient. In our baseline setting, we use a total of 182 districts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

of our results by implementing a two-step estimation procedure as proposed by Donald and Lang (2007) that first aggregates the individual data at the group level and then performs the DD estimation on covariate-adjusted district averages.[23] Finally, in some of our robustness checks, we also show that our results are robust if we implement alternative methods to calculate standard errors, including two-way clustering methods suggested by Cameron et al. (2011) or if we 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.

2.4 Data and descriptive evidence

We consider employment and wage outcomes from 1975 to 2002 for 182 districts (NUTS-3, "Landkreise und kreisfreie Städte") that are located in the four German federal states of Hesse, Rhineland-Palatinate, Baden-Württemberg, and Bavaria. Almost 94 percent of the U.S. Military personnel was based in these states in 1990, that is before the beginning of the drawdown.

Figure 2.4: U.S. Force level 1990 at district level



Source: Own calculations based on original data on U.S. forces in Germany. See text and appendix 6.1 for details.

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

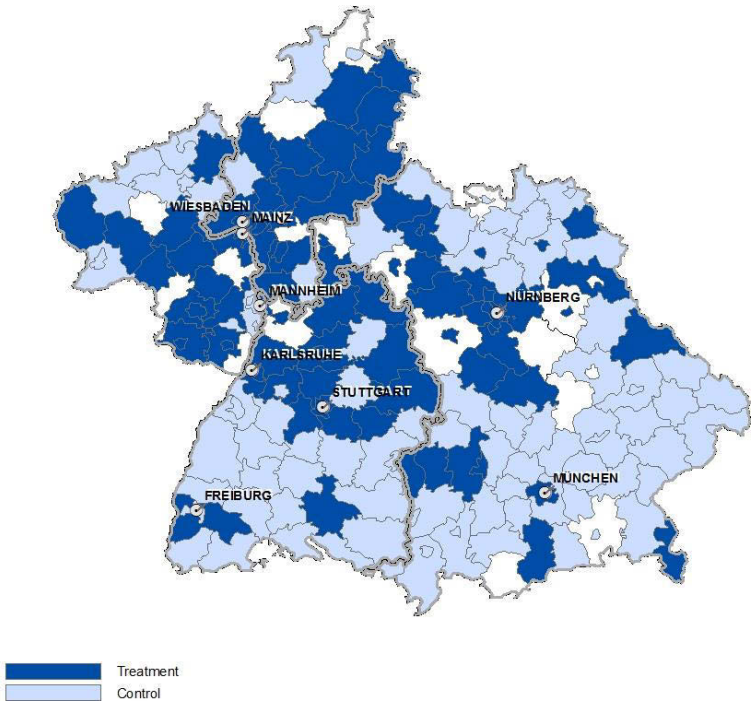
Table 2.1: Summary statistics for selection of treatment districts

Regional selection	No. of districts	No. of bases	Total U.S. forces (in 1990)	Share of total	U.S. forces personnel according to district 1990							
					Mean		Median		Min		Max	
					abs.	rel.⋆	abs.	rel.⋆	abs.	rel.⋆	abs.	rel.⋆
Districts with U.S. installation(s) (incl. unmanned) in West Germany in 1990	130	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 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

Our treatment intensity 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 geo-coded 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 2.3 illustrates the extent of the base realignments between 1990 and 2002 across West Germany, while Table 2.1 shows the selection of the districts and Figure 2.4 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 2.5 exhibits the geographical distribution of the 86 treatment and 96 control districts in our baseline sample.

Figure 2.5: Map of treatment and control districts in baseline sample



Source: Own illustration. See text for details.

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.[24]

Table 2.2: 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%.

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

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

there.[25]

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.[26]

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.[27] 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 IAB dataset (for example, Dustmann et al. (2009) and studies cited in the review by Büttner (2010)).[28] Wages are deflated by

[25] See Dustmann and Glitz (2012) 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 IABS is almost 15%, while the true share over the whole population of firms in Germany is less than 2%.

[26] 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., eds (1992).

[27] 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.

[28] 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,

the common consumer price index (base year: 2000) for West Germany provided by the Federal Statistical Office.[29]

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 at 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 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.[30] 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).[31] Finally, we separately identify all "border" districts that share a common border with any neighboring foreign state.[32]

Table 2.2 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 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 2.2: 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

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 universe of individual records for a long panel, we decided to keep to the simpler method.

[29] Despite some recent concerted efforts, consistent regional price indices are unfortunately still not available for Germany (see Möller et al., eds, 2010). Our wage outcome thus comes with the caveat that the regional variation in the real wages reflects - at least in part - differences in regional price levels that we cannot properly adjust for.

[30] 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.

[31] For convenience, we reproduce their classification in table 6.1 in appendix 6.3.

[32] 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.

states (see Figure 2.5). 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.

2.5 Results

We start the exposition of our empirical results by first reporting detailed estimates for the effect of the U.S. withdrawal on private sector employment in the affected districts, before we turn to the discussion of the impact on wages, unemployment and net migration in the subsequent subsections.

2.5.1 Employment

A. Initial Estimates

Table 2.3 reports the results for our initial OLS regression of equation (2.1). Each column presents results from separate regressions for three alternative sample definitions, 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 182 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

Table 2.3: Estimated impact of U.S. military withdrawal on total district employment, 1975-2002

Dep. Variable: Employment (log)		(1)	(2)	(3)	(4)
		A. - All			
U.S. withdrawal		-.008***	-.007***	-.004***	-.004***
		(.002)	(.002)	(.001)	(.001)
	R^2	.987	.988	.998	.998
		B. - Male			
U.S. withdrawal		-.009***	-.007***	-.004***	-.004***
		(.002)	(.003)	(.001)	(.001)
	R^2	.987	.988	.997	.998
		B. - Female			
U.S. withdrawal		-.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 U.S. withdrawal 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%.

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 coefficient estimates on the U.S. withdrawal treatment variable can be converted into an elasticity estimate. It implies that a reduction of local U.S. forces manpower by 100 positions leads to a loss of about 4.6 full-time private sector jobs.[33]

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

[33]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.

view robust and conservative estimates of the withdrawal effect. [34]

B. Heterogeneity of effects

As argued in the introduction, the U.S. withdrawal shock constituted a consumption shock that affected local labor demand as the U.S. demand was concentrated on 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 2.4 shows the results for specifications analogous to specification (1), estimated for employment separated according to industry groups.[35] 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 2.5 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 highly 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 2.2.2, 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

[34] 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.

[35] 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.

Table 2.4: Impact of U.S. military withdrawal on employment according to industry

Dep. Variable: Employment (log) by industry		(1)	(2)
		1 Basic materials	
U.S. withdrawal		-.004	-.004*
		(.003)	(.002)
	R^2	.988	.992
		2 Investment goods	
U.S. withdrawal		-.004*	-.003*
		(.002)	(.002)
	R^2	.995	.997
		3 Food and consumption goods	
U.S. withdrawal		-.003	-.004**
		(.002)	(.002)
	R^2	.991	.995
		4 Construction	
U.S. withdrawal		-.004**	-.002
		(.002)	(.001)
	R^2	.989	.993
		5 Retail/Repair	
U.S. withdrawal		-.000	-.001
		(.002)	(.001)
	R^2	.995	.997
		6 Transport/Information	
U.S. withdrawal		-.004	-.002
		(.003)	(.002)
	R^2	.985	.991
		7 Corporate services	
U.S. withdrawal		-.002	-.003*
		(.002)	(.002)
	R^2	.994	.996
		8 Private household services	
U.S. withdrawal		-.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 (see table 2.3). Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Table 2.5: Impact of U.S. military withdrawal on employment according to age and education groups

Dep. Variable: Employment (log) by age/education groups	(1)	(2)	(3)	(4)	(5)	(6)
2.5-1 Age groups			A. - All			
	25-35 yrs.		35-45 yrs.		45-55 yrs.	
U.S. withdrawal	-.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.	
U.S. withdrawal	-.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.	
U.S. withdrawal	-.004**	-.004***	-.004***	-.004***	-.002	-.003**
	(.002)	(.001)	(.002)	(.001)	(.002)	(.001)
R^2	.994	.997	.995	.996	.994	.997
2.5-2 Education groups			A. - All			
	High		Medium		Low	
U.S. withdrawal	-.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	
U.S. withdrawal	-.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	
U.S. withdrawal	-.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%.

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 effects by augmenting the specification from equation (2.1) 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 2.6 presents the results when we reestimate the effect on total district employment in the augmented model, using again all four different specifications 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 2.6.

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, as such data is not available, we are not able to 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.[36] 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.

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

[36] 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.

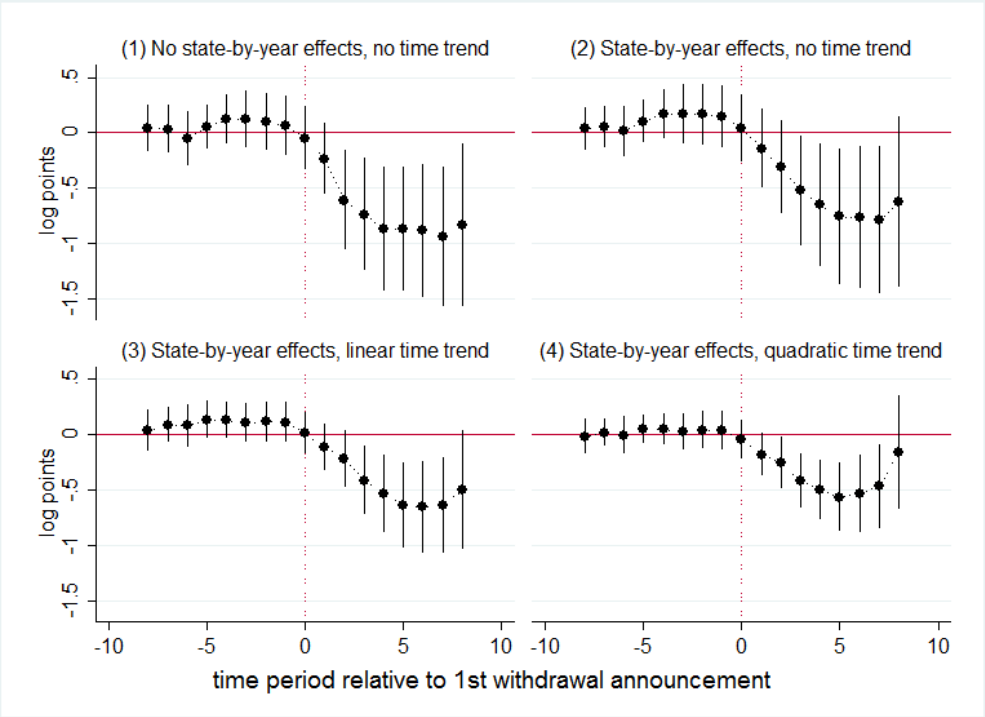
Table 2.6: Dynamic pattern of impact of U.S. military withdrawal on total employment at district level

Dep. variable: Total employment (log)	(1)	(2)	(3)	(4)
U.S. withdrawal$_{t-8}$	.000	.000	.000	-.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-7}$	.000	.000	.001	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-6}$	-.001	.000	.001	-.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-5}$	.001	.001	.001	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-4}$	.001	.002	.001	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-3}$	.001	.002	.001	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-2}$	.001	.002	.001	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t-1}$	.001	.001	.001	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal$_{t0}$	-.000	.000	.000	-.001
	(.001)	(.002)	(.001)	(.001)
U.S. withdrawal$_{t+1}$	-.002	-.001	-.001	-.002*
	(.002)	(.002)	(.001)	(.001)
U.S. withdrawal$_{t+2}$	-.006***	-.003	-.002	-.003**
	(.002)	(.002)	(.001)	(.001)
U.S. withdrawal$_{t+3}$	-.007***	-.005**	-.004***	-.004***
	(.003)	(.003)	(.002)	(.001)
U.S. withdrawal$_{t+4}$	-.009***	-.007**	-.005***	-.005***
	(.003)	(.003)	(.002)	(.001)
U.S. withdrawal$_{t+5}$	-.009***	-.008**	-.006***	-.006***
	(.003)	(.003)	(.002)	(.002)
U.S. withdrawal$_{t+6}$	-.009***	-.008**	-.007***	-.005***
	(.003)	(.003)	(.002)	(.002)
U.S. withdrawal$_{t+7}$	-.009***	-.008**	-.006***	-.005**
	(.003)	(.003)	(.002)	(.002)
U.S. withdrawal$_{t+8 forward}$	-.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%.

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 (2.1). Table 2.7 reproduces in row 1 the coefficient estimates for the effect on total district employment from the top panel in

Figure 2.6: Dynamic pattern of impact of U.S. military withdrawal on total employment at district level



Notes: See text and table 2.6 for details. Vertical bands represent ± 1.96 times the standard error of each point estimate.

column (7) and (8) of table 2.3 as a baseline for comparison.

We first consider the robustness with respect to alternative definitions of the treatment intensity. Row 2 reports the estimates for a specification in which the treatment intensity is not defined by total U.S. personnel level, but only by 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 2.4), 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.[37] 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, FFA*) 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. We identify the districts that are affected by the German and French base realignments with the help of additional data.[38] 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 2.3, the question 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

[37] 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 6.1 plots the time series of the exchange rate for our sample years 1975 to 2002.

[38] For details, see section 6.1.3 and 6.1.4 in the Data Appendix 6.1. In appendix 6.2, Figure 6.2 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 6.3 depicts the districts within the four federal states that also hosted bases of the French forces.

Table 2.7: Robustness analyses for impact of U.S. military withdrawal on total employment

Dep. Variable: Total employment (log) - All	(1)	(2)
1. Baseline Table 2.3-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. withdrawal (%) - Army (N=84)	-.004***	-.003***
	(-.001)	(-.001)
U.S. withdrawal (%) - 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		
U.S. withdrawal	-.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 (see table 2.3). 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%.

robust standard errors clustered on the district level that allow for arbitrary correlations within districts over time. Row 14 of table 2.7 presents the analogous 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 have also used an alternative specification of the standard errors recently proposed by Cameron et al. (2011). Their method aims at improving 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 larger 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.[39]

2.5.2 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 2.3 and 2.4, 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.

Tables 2.8 to 2.10 present the results from DD estimations of equation 2.2 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).

The estimates for the overall wage effect in Table 2.8 and separately according to age and education groups in Table 2.9 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

[39] We also repeated the whole empirical analysis at this higher aggregation level of IABS 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.

Table 2.8: Estimated impact of U.S. military withdrawal on gross daily wages, 1975-2002

Dep. variable: Real wage (log)	(1)	(2)	(3)	(4)
U.S. withdrawal	.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 (see table 2.3). Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Table 2.9: Impact of U.S. military withdrawal on gross daily wages according to age and education groups

Dep. Variable: Real wage (log) by age/education groups	(1)	(2)	(3)	(4)	(5)	(6)
2.9-1 Age groups	25-35 yrs.		35-45 yrs.		45-55 yrs.	
U.S. withdrawal	-.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
2.9-2 Education groups	High		Medium		Low	
U.S. withdrawal	-.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 (see table 2.3). Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

dismissed. The analyses according to industries in Table 2.10 only reveal some evidence of potential downward wage adjustments for the sectors of "Food and consumption goods" and "Transport/Information".

Table 2.10: Impact of U.S. military withdrawal on gross daily wages according to industry

Dep. Variable: Real gross daily wage (log) according to industry		(1)	(2)
		1 Basic materials	
U.S. withdrawal		-.000	-.001
		(.001)	(.001)
	R^2	.497	.497
	N	974,206	974,206
		2 Investment goods	
U.S. withdrawal		.000	.001**
		(.000)	(.000)
	R^2	.498	.498
	N	2,681,760	2,681,760
		3 Food and consumption goods	
U.S. withdrawal		-.000	-.00002*
		(.000)	(.000)
	R^2	.366	.366
	N	1,136,738	1,136,738
		4 Construction	
U.S. withdrawal		.000	.000
		(.000)	(.000)
	R^2	.327	.327
	N	1,243,583	1,243,583
		5 Retail/Repair	
U.S. withdrawal		-.000	.000
		(.000)	(.000)
	R^2	.287	.287
	N	1,095,586	1,095,586
		6 Transport/Information	
U.S. withdrawal		.0002*	-.0003***
		(.000)	(.000)
	R^2	.242	.242
	N	634,314	634,314
		7 Corporate services	
U.S. withdrawal		-.001	-.001
		(.001)	(.001)
	R^2	.387	.387
	N	802,316	802,316
		8 Private household services	
U.S. withdrawal		-.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

Notes: Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects, cf. table 2.3. Robust std. errors clustered at district level in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

2.5.3 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.[40]

Table 2.11: Impact of U.S. military withdrawal on employment, wages, unemployment, net migration, 1985-2002

Dep. Variable	(1)	(2)	(3)
	A. Total employment (log)		
U.S. withdrawal	-.006***	-.005***	-.003***
	(0.002)	(0.002)	(0.001)
	B. Real wage (log)		
U.S. withdrawal	.000	.000	-.000
	(.000)	(.000)	(.000)
	C. Unemployment rate		
U.S. withdrawal	.063**	.063**	.058***
	(.028)	(.028)	(.022)
	D. Net migration share		
U.S. withdrawal	-.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

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%.

[40] 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.

In table 2.11, 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.[41] The coefficient estimates are consistent with our previous estimates for the longer time period from 1975-2002 (see tables 2.3 and 2.8), with significant negative effects for local private sector employment and no immediate 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.[42] 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 2.5.1, 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 time delay. In table 2.12, we hence present results from analogous regressions of the dynamic pattern for all outcomes for the time period 1985 to 2002.[43] Column (1) reports the coefficient estimates for the withdrawal leads and lags for employment. Consistent with the previous results in table 2.6 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, only the coefficient estimate

[41] 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.

[42] We define the net migration share by dividing the net migration balance for in- and outward moves of residence across district borders in year t by the district population in year t-1.

[43] 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.

Table 2.12: 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
U.S. withdrawal$_{t-5}$	.000	-.000	-.003	-.031**	.002
	(.001)	(.000)	(.002)	(.016)	(.010)
U.S. withdrawal$_{t-4}$	-.000	-.000	.001	-.004	.000
	(.001)	(.000)	(.003)	(.016)	(.013)
U.S. withdrawal$_{t-3}$	-.000	.000	.003	.014	.008
	(.001)	(.000)	(.003)	(.015)	(.014)
U.S. withdrawal$_{t-2}$	.000	.000	.005*	.021	.007
	(.001)	(.000)	(.003)	(.016)	(.014)
U.S. withdrawal$_{t-1}$	.000	.000	.005*	0.031*	-.020
	(.001)	(.000)	(.003)	(.018)	(.012)
U.S. withdrawal$_{t0}$	-.001	.000	.004	.019	-.011
	(.001)	(.000)	(.003)	(.018)	(.014)
U.S. withdrawal$_{t+1}$	-.002**	.000	.006*	.037*	-.001
	(.001)	(.000)	(.003)	(.021)	(.013)
U.S. withdrawal$_{t+2}$	-.002**	.000	.008**	.065**	-.018
	(.001)	(.000)	(.004)	(.029)	(.019)
U.S. withdrawal$_{t+3}$	-.003***	.000	.009*	.090**	-.043**
	(.001)	(.000)	(.005)	(.036)	(.021)
U.S. withdrawal$_{t+4}$	-.004***	-.000	.010**	.120***	-.038
	(.001)	(.001)	(.005)	(.040)	(.026)
U.S. withdrawal$_{t+5}$	-.005***	-.000	.011*	.133***	-.034
	(.001)	(.001)	(.005)	(.041)	(.023)
U.S. withdrawal$_{t+6}$	-.004***	-.000	.010*	.132***	-.027
	(.001)	(.001)	(.006)	(.047)	(.024)
U.S. withdrawal$_{t+7}$	-.003**	-.000	.009	.119**	-.032
	(.001)	(.001)	(.007)	(.055)	(.026)
U.S. withdrawal$_{t+8m}$	-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%.

for year 3 after the withdrawal announcement is significant at the 5 percent level. The pattern of the point estimates provide a qualitative indication that some of the adjustment in response to the withdrawal shock could also have occurred via increased out-ward or reduced in-ward migration in the affected regions, but the data limitations outlined above preclude the opportunity for a more in-depth analysis.

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. local wages). This finding is consistent with Topel's (1986) result for the effect from a permanent local economic shock.

2.6 Conclusion

Empirical research has had difficulty in establishing the causal effects of local economic shocks, 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 has 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 political lobbying: 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 in a significant way.

3 Local economic shocks and heterogeneous wage adjustments

3.1 Introduction

The rigidity of regional wages in Germany is a recurring topic and has often been cited in the literature as one reason for the large and persistent differences in unemployment rates across German regions, particularly in comparison to the U.S., but also other European countries (see, e.g., Siebert, 1997, OECD, 2005, Chap. 2). At the core, this argument is based on the insight that in a simple supply and demand framework, flexible wage adjustments could help mitigate the consequences of a negative demand shock by re-equilibrating the wage with the marginal product of labor and hence reduce the pressure on firms to cut employment. Indeed, in his influential analysis Topel (1986) provides evidence that regional wage adjustments do moderate the drop in employment in response to negative regional shocks in the U.S..[1]

In contrast, empirical studies for Germany usually find evidence that wages are not (or at least substantially less) downward flexible at the regional level. This finding is supported both by studies that closely follow Topel's approach using appropriately filtered national employment changes as a proxy for regional demand shocks in micro-econometric wage regressions (see Mertens 2002), as well as a large body of literature on the "wage curve"[2], which for Germany finds only a comparatively small elasticity of wages to regional unemployment rates (see, among others, Baltagi and Blien, 1998; Büttner, 1999; Baltagi et al., 2009). Further empirical evidence on wage rigidities in Germany is also established in several contributions to the "wage cyclicality" literature that focuses on the effect of changes in the aggregate unemployment rate arising from business cycle fluctuations on wages (see, e.g., Gartner et al., 2010; Anger, 2011).

A common thread in these interrelated strands of the literature is that most authors point towards the differences in wage bargaining institutions as a potential key determinant of the lower regional wage flexibility in Germany, with a focus on collective bargaining

[1] Based on the seminal papers by Blanchard et al. (1992) and Decressin and Fatas (1995) who focus on the patterns of adjustment to a regional shock in employment, participation and migration, another strand of the literature has focused on the lower mobility rates in Germany, particularly for low-skilled workers, as an explanation particularly for the persistence in the incomplete adjustment at the regional level over the medium to long run. See Arntz (2011), Arntz et al. (2011).

[2] The "wage curve"-relationship between regional unemployment and wages was first proposed and analyzed by Blanchflower and Oswald (1994). See Card (1995) for a review and, for example, Nijkamp and Poot (2005) for a meta-analysis on the international evidence.

agreements that effectively limit the amount of regional wage differentiation. Despite this seemingly broad consensus, there is still a lack of empirical evidence that documents the effect of wage bargaining institutions on wage responses in a situation where an exogenous regional labor demand shock can be directly observed. This limitation is potentially severe, given the fact that the regional unemployment rate is endogenously determined and hence the empirical analysis of wage changes in response to an above average increase in the unemployment rate can provide only little insight into whether more flexible wages could have helped to moderate the increase in unemployment in the first place.

Our purpose in this paper is to investigate the heterogeneity of wage evolutions in labor market segments characterized by differences in the wage determination process in the aftermath of the withdrawal of U.S. Armed Forces from Germany. In previous work (see aus dem Moore and Spitz-Oener 2012), we have shown that the base closures by the U.S. Army and Air Force constituted an exogenous demand shock in some districts in Western Germany, and that this shock led to a significant drop in private sector employment in the affected districts, whereas wages by and large exhibited only little and mostly insignificant reactions.[3] In this setting, the general pattern of employment and quantity adjustments dominating wage adjustments seems consistent with Topel's 1986 conjecture for a permanent shock as agents in the local markets were quick to understand that the base closures would cause an irreversible drop in demand for local goods and services.[4] This interpretation notwithstanding, the absence of any wage response could still be the outcome of wage rigidities arising from wage bargaining agreements and other institutions affecting the functioning of the local labor markets.

In our empirical work, we thus further explore the question of the effect of the U.S. withdrawal shock on wages in local labor markets by exploring the heterogeneity of wage adjustments in various labor segments. In distinguishing the segments, we broadly follow the approach by Prantl and Spitz-Oener (2011) and focus on the presence and interaction of a labor and a product market institution that both potentially affect the degree of downward wage flexibility. As a labor market institution, the German Works Constitution Act legally mandates the option of the establishment of a works council and its co-determination rights as a function of the number of employees in an establishment. As a consequence, the share of establishments with works councils largely differs by establishment size, with almost full coverage of establishments having more than 50 employees and much lower prevalence in smaller establishments. In our context, the existence of a works council should likely matter when it comes to the renegotiation of wages in the face of an adverse demand shock as it has a strong influence on the bargaining power of workers. With respect to the product market dimension, the German Trade and Crafts Code (GTCC) is a regulation that substantially limits firm entry into certain product markets. As an entry barrier, it reduces competitive pressure and is conducive to the generation of product market rents.

[3] Our analysis also found some evidence of an increase in the local unemployment rate and a shift towards more out-migration in the affected districts, although these effects were less precisely estimated.

[4] In Topel's setting of a spatial equilibrium framework, a larger expected persistence of a local demand shock leads to stronger supply adjustments for mobile workers and hence smaller adjustments of wages.

If firms operating in these markets share at least part of their rents with their employees, this component of the wage should be more sensitive to the profit situation of the firm and hence could also introduce more flexibility for downward wage adjustments for workers in the associated segment of the labor market.

Our findings suggest that when operating in isolation, neither the labor market institution nor the product market regulation introduce any substantial effect heterogeneity in the wage adjustment process to the adverse local economic shock of the U.S. withdrawal. On the other hand, for a labor market segment where the two institutions interact, we do observe some evidence of a differential negative wage adjustment for workers in establishments with weak influence in wage bargaining, but operating in regulated product markets. Consistent with the specific nature of the shock that we consider, this effect applies only to those local non-tradable service industries that are prone to suffer most from the drop in local demand.

Our analysis integrates well into the literature that stresses the importance of the regulatory environment characterized by the presence and interplay of institutions both on labor markets and product markets for the heterogeneity of wage adjustments in response to economic shocks. While the interrelations between product and labor market regulations is a long-standing topic in the macroeconomic literature, the focus of most studies in this area has been on the question how deregulation of product markets can have a heterogeneous impact on employment and wages across countries with different labor market institutions, particular by contrasting the evidence from European countries with the U.S. (see, among many others, Blanchard and Giavazzi, 2003; Griffith et al., 2007). A closely related strand of the literature has analyzed the heterogeneity of unemployment evolutions across countries from the interaction of macroeconomic shocks with different labor market institutions,[5] but usually then ignored the product market dimension. The latter approach has also been adapted in recent studies in the wage cyclicality and firm insurance literature that distinguish between more detailed labor market regimes within countries.[6] While being based on a similar theoretical motivation, our approach differs from these studies as we use the quasi-natural experiment of the U.S. military withdrawal from Germany to identify a specific exogenous product demand shock and analyze the effect heterogeneity arising from two specific institutions that we identify in our micro-econometric analysis.

The remainder of this paper proceeds as follows. The next section contains a short theoretical discussion how institutions could affect the incidence of wage adjustments to local economic shocks. In Section 3.3, we summarize the background of the U.S. base closures in Germany and also provide more detail on the institutional background of the labor and product market regulations that we investigate. In Section 3.4, we introduce our empirical approach and section 3.5 describes the data that we use. Section 3.6 presents the

[5] A key reference here is Blanchard and Wolfers (2000).

[6] For Germany, see, e.g., studies by Gartner et al. (2010), who study the heterogeneity of wage cyclicality with respect to the presence of works councils and collective bargaining agreements, and the studies by Gürtzgen (2009a,b) who analyzes the dependence of rent-sharing and firm wage insurance on bargaining institutions.

empirical results, and section 3.7 concludes.

3.2 Institutions and wage determination: conceptual background

In Germany, the large majority of wages is determined in agreements between trade unions and employers' associations, particularly for low and medium educated employees that we analyze in the empirical part of this paper. It thus seems sensible to focus here on union bargaining as the appropriate basic theoretical framework for analyzing the heterogeneous impact of labor and product market regulations and later interpret our empirical results.[7]

In a bargaining framework, employers and unions negotiate over the surplus arising from the labor exchange.[8] Unless the union has no bargaining power at all, the wage will thus deviate from the competitive outcome by a mark-up relative to the marginal product of labor. Beyond the strength of the union, the size of the mark-up will be higher the lower the elasticity of labor demand, the lower the responsiveness of profits to wages and the higher the reservation wage of workers. The surplus that can be bargained over can arise from two different sources: first, if product markets are perfectly competitive, the surplus is limited to a quasi-rent on capital that exists if capital cannot be adjusted in the short run. A larger surplus is possible in non-competitive product markets where firms have market power and thus earn economic rents.

The question whether and how much of this surplus workers can appropriate by negotiating higher wages is the central topic in the rent-sharing literature (see, among many others, Christofides and Oswald, 1992; Hildreth and Oswald, 1997; Nickell, 1999 and the comprehensive review of both the theoretical and empirical literature on rent-sharing in Manning, 2011). The workers' share in firm rents introduces a link between the profit situation of a firm and wages. Consequently, the magnitude of this relationship is related both to the overall size of economic rents as well as the bargaining power of workers.[9]

An important caveat regarding the applicability of the rent-sharing argument in our context is that both theoretically and empirically, most studies focus on a context of positive

[7] Of course, in a neoclassical labor market model with perfect competition, wages would drop in response to a negative demand shock as real wages need to equalize the lower marginal product of labor. However, the long-standing and overwhelming empirical evidence favors the notion that downward wage adjustments are rather rare and sluggish. Beyond the union bargaining approach, economists have developed a broad range of theoretical models that can help to explain downward wage rigidities. Notably, other key approaches in the literature on wage determination which all provide alternative (though not mutually exclusive) rationales why firms might be reluctant and/or inhibited to cut real wages even in adverse market conditions include implicit contract theory, efficiency wage theories and search models, as well as insider-outsider models of the labor market.

[8] For a formal treatment of simple static bargaining models, both of the "right to manage" and "efficient bargaining" types, that can be used to derive the main arguments in this paragraph, see, for example, Boeri and van Ours (2008, chap. 3).

[9] While in line with our reasoning here, the discussion of rent-sharing in the literature is frequently set in a collective bargaining framework that combines a non-competitive labor market with a non-competitive product market, there are also other types of models where the labor market is competitive, but workers still benefit from improvements in a firm's profitability. One possibility is that the labor reallocation from less productive firms is costly in the short run. Another setting is based on the idea that risk-neutral firms and risk-averse workers share risk from shocks to profitability via an implicit contract. See Blanchflower et al. (1996) for a summary.

shocks to firm productivity. The argument then is reduced to the question if workers can realize higher wages as part of an increasing pie of profits. In contrast, the existing evidence suggests that wage adjustments are not symmetric for positive and negative shocks to product demand (see, e.g., Holzer and Montgomery, 1993). An obvious explanation for this asymmetry could be that the inter-relation between the degree of product market competitiveness and the wage bargaining process plays out differently in these contexts. In particular, we would expect downward wage adjustments to occur predominantly in the segment of the labor market where workers participate in product market rents, but do not have strong bargaining power to defend these rents in the face of an adverse demand shock.[10]

In our empirical application, we thus distinguish and compare labor market segments along the two key dimensions identified in the bargaining framework that should affect how much wages adjust to the negative demand shock induced by the withdrawal of the U.S. Forces, namely worker bargaining power and product market competition. After first studying the effects of these two dimensions in isolation, we extend the analysis to explore how their interaction might differentially affect the wage response in the event of an adverse shock. The next section first describes the setting of the U.S. Base Closures as a local product demand shock and then explains the institutional background for the two types of regulation in greater detail.

3.3 Local economic shocks, labor and product market regulations and wages

3.3.1 U.S. Military Base closures and the local economy in West Germany

The closure process of U.S. bases in Western Germany induced by the unexpected fall of the Berlin Wall and the end of the Cold War constitutes a rare opportunity to study effects of an adverse demand shock that exogenously hit some regions of the country, while leaving others unaffected.

Up to 1989, the U.S. Department of Defense maintained in Germany one of the largest and most long-lasting peace time deployments of troops in a foreign country in modern history, including about 250,000 U.S. servicemen, around 90,000 American civilian employees and 230,000 dependents. While the United States originally had not planned for a permanent presence of its troops after the defeat of Nazi Germany by the Allied Powers and the

[10] Of course, this argument might raise the question why workers with rather weak influence in wage determination were able to obtain a share of the rents in the first place. However, the empirical evidence suggests that under favorable economic conditions, firms operating in regulated product markets share part of their rents with workers rather broadly. Some theoretical reasons for this behavior of rent-sharing even in the absence of strong worker bargaining power that are cited in the literature include managers' preferences for a "quiet life", the objective to keep unions out of the bargaining process as well as considerations of fairness, based on an efficiency wage argument that workers shirk more often if they feel that they are unfairly treated. See, e.g., the short summary of these arguments and the references in Nickell et al. (1994) and Nickell (1999).

end of World War II, the rising confrontation with the Soviet Union and the Warsaw Pact countries during the Cold War led to the gradual build-up of over 800 U.S. military installations across Western Germany and West-Berlin. Determined mostly already by their initial location in 1945, as well as by the development of a joint military strategy in coordination with its NATO partners in the 1950s, the U.S. bases and military communities were heavily concentrated in the south-western area of Germany. In particular, the four federal states of Hesse, Rhineland-Palatinate, Baden-Wüerttemberg and Bavaria hosted more than 90 percent of all U.S. installations with assigned personnel, making up for almost 94 percent of the total U.S. military and civil personnel stationed in West Germany and Berlin.

The type of U.S. military bases in Germany ranged from small unmanned radio stations and underground armories up to single air bases that (at the peak of their Cold War activity level) employed up to 12,000 soldiers and employees. While initially designed on the concept of autarky, the American communities that developed around the military bases closely interacted with the local German population and the economy over the course of almost half a century of coexistence. First, the U.S. bases did source certain non-tradable goods and services from local German suppliers. Second, some U.S. bases employed direct civil employees, although their share in local employment was usually rather small. Third, the U.S. soldiers and their dependents were active private consumers in the local economy, creating extra demand for local retail, hotel, catering and entertainment businesses and local transport companies. Hence, the economic importance of the U.S. bases closely varied with their local personnel strength.

The fall of the Berlin Wall is the central turning point in the history of the U.S. military presence in Germany. Although formal negotiations between NATO and Warsaw Pact countries about troop reductions in Europe on both sides of the Iron Curtain had already started in early 1989, the reciprocal agreements on ceilings for the number of weapons and manpower applying both to domestic and foreign powers across Europe now became an essential part of the design of a new balance of power in Europe in the lead-up to the formal German Reunification on October 3, 1990. Hence, as early as September 1990, the U.S. Department of Defense announced a first major round of base closures in Germany affecting 110 individual sites, with 20 more to follow until 1996 that would reduce the number of U.S. personnel by more than 75 percent compared to the 1989 level. The process for the selection of sites to be closed was developed by military planners in the command of the U.S. Army Europe and in the Pentagon. Official U.S. documents as well as reviews and case studies of the drawdown process by political scientists provide ample and concordant evidence that the decisions on base closures were first and foremost taken based on military and geo-strategic assessments. Moreover, the extent and speed of the required cuts left little to no room for the the consideration of concerns of local stakeholders.

3.3.2 Institutional background

A. German works councils and their role in wage bargaining

Works councils represent worker interests in hiring, firing and pay scheme decisions on the establishment level according to the German Work Constitution Act (*Betriebsverfassungsgesetz, BetrVG*). Their precise institutional design and concrete influence mainly depend on the size of the respective establishment in terms of number of employees, as stipulated in detail in the 1972 amendment of the act (BetrVG, 1972) that was in force during our period of main interest. A works council must be created on demand of the workforce in an establishment with more than 5 employees. The actual presence of works councils is strongly related to firm size as reported consistently by various studies, among them Addison et al. (1997), Beckmann et al. (2010) and Gartner et al. (2010). In a simple comparison of only two size classes, "small establishments" with less than 50 employees versus "large establishments" with 50 or more employees, Addison et al. (1997) show that about 92 percent of the employees in large establishments do have a representation by a works council whereas the corresponding number for small establishments is only 18 percent. There is an additional reason why establishment size matters: In firms with more than 100 employees, a works council must set up an economic committee ("Wirtschaftsausschuss") which has to be informed by the management in a comprehensive and timely manner about the current and future economic and financial situation of the firm. German works councils play an important role in the wage setting procedures that are carried out on two different levels. First, collective agreements are negotiated between trade unions and employer organizations at the regional and industry level. The wages listed in these collective agreements provide the lower bound for the wages paid in an industry or sector. They form the starting point for subsequent negotiations at the firm level between works councils and the individual employers. In these negotiations, the parties bargain about payments above the tariff norm. Firm-level wage bargaining has become much more widespread since the late 1980s. Consequently, as Bauer et al. (2007) describe in more detail, the number of employees who received wages above the limit fixed by collective agreements rapidly expanded. Actual wages often exceed the collectively agreed minimum rates by a substantial amount, cf. Jung and Schnabel (2011).

B. Regulation of firm entry by the German Trade and Crafts Code

The German Trade and Crafts Code (GTCC) is a regulation that substantially restricts firm entry in a precisely defined set of product markets. It covers many occupations that were organized as guilds in the Middle Ages and various later additions.[11]

[11] Its roots date back to the historical guild system in Germany which was partly re-institutionalized in 1897 as a first backlash to the freedom of trade ("Gewerbefreiheit"), introduced in the German Reich in 1871. The GTCC attained its impact as a factual entry regulation in two steps: The master craftsman certificate in its original form, introduced in 1908, was made mandatory for individuals who wanted to train apprentices in one of the regulated occupations ("Kleiner Befähigungsnachweis") and therefore posed a restriction rather on the growth of existing establishments, but not on the entry of new firms.

The product markets subjected to the GTCC are therefore in very diverse fields (such as metalworking, food, clothing and textiles, building and interior finishing, printing, glass production, food or body care) and often quite close to fields with unregulated product markets: "for example, confectionary, hairdressing, and printing and bookbinding are regulated, but ice cream production, beautician services and copy and paper production are not." (Prantl and Spitz-Oener, 2011, p. 11) Earning a master craftsman certificate involves a substantial investment of time as well as significant direct costs, such as fees for preparation courses. To obtain the certificate, candidates have to complete several long lasting stages of training, gain in-depth work experience and pass demanding examinations.[12] Critics of the GTCC, such as the German monopoly commission and other German or EU institutions (Deregulierungskommission, 1991; Monopolkommission, 1998a,b), judge this firm entry regulation as unnecessary to ensure a high quality of goods and services and sufficient training activities. They argue that the incentives present in unregulated markets, like reputation effects, should be sufficient to ensure efficient outcomes, since many of the goods produced in these regulated markets are standard experience goods. Higher product prices and lower production quantities in the respective markets are seen as direct effects of the regulation. Additionally, the GTCC is supposedly responsible for lowering entry and industry dynamics, competition intensity, job creation, innovation and economic growth in the regulated markets.[13]

3.4 Empirical approach and identification

Our goal is to investigate whether the regional demand shock induced by the drawdown of U.S. Military Forces led to heterogeneous wage adjustments in segments of the labor market that can be characterized by differing degrees of worker influence in wage bargaining and by differing levels of product market competitiveness. In an extension to the empirical model employed already in aus dem Moore and Spitz-Oener (2012), we thus use a version of a fixed-effects differences-in-differences-in-differences (DDD) framework, which allows us to exploit four dimensions of variation for identification.

This changed substantially with the amendment of 1935: Now, every individual who wanted to start a legally independent business in one of the regulated product markets had to give prove of holding a relevant master craftsman certificate ("Großer Befähigungsnachweis") in the mandatory process of registration. This strict entry regulation also outlasted World War II and was confirmed in the post-war version of the West German Trade and Crafts Code of 1953. A couple of minor reforms and adjustments (see Boyer, 1990; Deregulierungskommission, 1991) let inter alia to the addition of some regulated occupations, but did not change the basic character of the GTCC and its impact as a major entry barrier to many product markets.

[12] The starting point are two or three years of apprenticeship training that lead to a basic vocational training degree in a relevant occupation ("Lehre und Lehrabschluss"). The second step is the accumulation of work experience in the respective occupation for several years and the demonstration of a high level of proficiency in all occupation-specific tasks, documented by the related journeyman degree ("Gesellenzeit und -brief"). The final step requires passing the master examination ("Meisterprüfung"). The courses offered by private institutions to prepare for this examination in part- or full-time take 1 to 3 years to complete. In the exam, the candidate has to prove his competence in both occupation-specific and general tasks, like law, book-keeping, controlling, marketing and human resource management.

[13] Cf. Prantl and Spitz-Oener, 2011

The first is the regional variation by distinguishing (within the four south-western states of Hesse, Rhineland-Palatinate, Baden-Württemberg, and Bavaria that form the basis of our data sample) the set of treatment districts with a U.S. Military presence before 1990 from a control group of districts that did not host any U.S. bases. The second dimension is the temporal one and consists of comparing wage outcomes for workers in a pre-period when only minor changes in the U.S. presence occurred (both at the aggregate and at the local level), to the post-period that is initiated by the first announcement of a U.S. base closure in a given treatment district. The third is a measure of the exogenous variation in the U.S. force level in the treatment districts in the post-period. It enters the empirical specification in the form of a continuous treatment intensity and captures both the variation in the intensity and in the speed of the drawdown between treatment districts. Finally, the fourth dimension is the distinction between workers within districts by their employment in different labor market segments.

In a nutshell, the strategy is designed to compare workers in the same labor market segments in periods before and after the shock across treatment and control districts, while keeping close control of the exact timing and extent of the shock within the treatment districts. Moreover, the regression framework allows us to purge our estimates of a host of other, potentially confounding influences. In our preferred specifications, we estimate via OLS regression equations of the following form:

$$\begin{aligned}\log w_{ikt} = \alpha_k + \delta_t + \beta_1 \underbrace{(\log \text{U.S. Forces}_{kt} \times \mathbb{I}\left[t > \text{Year}_{k0}\right])}_{\text{"U.S. withdrawal"}} + \beta_{2k}(\text{LS}_i \times \alpha_k) \\ + \beta_{3t}(\text{LS}_i \times \delta_t) + \beta_4 \underbrace{(\log \text{U.S.Forces}_{kt} \times \mathbb{I}\left[t > \text{Year}_{k0}\right]}_{\text{"U.S. withdrawal"}} \times \text{LS}_i) \\ + X'_{ikt}\gamma + \theta_{lt} + \varsigma_{ot} + \phi_{zt} + \epsilon_{ikt}\end{aligned} \tag{3.1}$$

The outcome variable denoted by w_{ikt} is the real gross daily wage of a West German employee i working in district k in year t. All equations include a vector of district dummies, α_k, that control for time-invariant mean differences in wages across districts, and year dummies, δ_t, that adjust for time-series changes (e.g., macroeconomic shocks) affecting workers in all districts over time. The overall treatment effect of the U.S. withdrawal on wages for all workers is given by β_1. It is the coefficient on the interaction of an indicator variable that identifies for each treatment district the years in the "post" period when the U.S. drawdown occurs (i.e. if t is bigger than the respective base year Year_{k0}) with the log of the U.S. Force level that (as a measure of the treatment intensity) captures the extent and the timing of the reductions specific to each treatment district. The final dimension in the DDD logic enables the comparison of wage outcomes in response to the shock *within* districts by contrasting two different segments of employment. This dimension enters into the specification by means of a dummy variable identifying the relevant labor market segment LS_i in separate interactions with the district effects, year effects, and the

main treatment effect. In our empirical analysis, we thus implement the LS_i variable in two distinct ways: for the analysis of the heterogeneity by degree of worker influence in wage bargaining this variable is taking the form of a dummy variable identifying workers in small establishments as a proxy for the low coverage by works councils. Analogously, in regressions where we test for differences in wage adjustments by degree of product market competitiveness, it is implemented by a dummy variable identifying all workers employed in product markets regulated by the GTCC. The interactions of this variable with the district fixed effects (β_{2k}) thus control for time-invariant wage differences for workers in the two respective segments between districts, whereas the interactions with the year fixed effects (β_{3t}) absorb any systematic wage variations specific to workers in the two segments over time across all districts. Finally, β_4 is our key parameter of interest, a triple difference estimate of the U.S. withdrawal effect that captures all variation in wages for workers in one segment of the labor market (relative to its complement) in the treatment districts (relative to the controls) in the years with the U.S. withdrawal (relative to before). The set of individual demographic covariates is included by the column vector X_{ikt} that includes a quadratic polynomial in age, dummies for employees with medium education, and a dummy for foreign employees. These individual covariates control for potential endogenous changes in the composition of the workforce after the shock that have been shown to bias estimates that are based on average regional or average firm-level wages.[14] As for wage effects related to education and occupation, we include interactions of the dummy variable for low education with all year dummies (θ_{lt}) as well as a full set of 2-digit occupation-year interactions (ς_{ot}) in order to allow for time-varying returns to education and occupation. This set-up thus allows us to partial out any composition effects related to the secular increases in wage inequality by formal education or occupation-specific capital in this time period.[15] Finally, ϕ_{zt} is a full set of 2-digit industry-time interactions which capture potential demand shocks specific to detailed industries at the aggregate level of all four federal states. Moreover, they also control for potential time-varying effects of collective bargaining on wages, for example via the establishment of industry-specific national wage floors.

One limitation of estimating just one aggregate DDD effect is that the labor demand shock from the U.S. base closures does not affect all industries to the same extent. Based also on the heterogeneity of employment effects in response to the U.S. withdrawal shock already documented in aus dem Moore and Spitz-Oener (2012), we would expect the pressure on wages to be concentrated in those sectors that depend largely on the local demand for non-tradable goods and services. We thus re-estimate equation 3.1 separately for sub-samples of eight industry sectors, thereby obtaining separate DDD estimates for each industry.

[14] For prominent examples, see, e.g., Solon et al. (1994) for evidence of the countercyclical composition bias plaguing aggregated average wages in the wage cyclicality literature and Guiso et al. (2005) for a similar argument applied to the potential bias in estimates of risk-sharing within firms using firm-level average wages.

[15] See, e.g., Dustmann et al. (2009)

Finally, as motivated already in the conceptual background in section 3.2, we extend the specification by including interactions of the DDD terms and the full set of DD terms for workers in establishments with weak worker influence in wage bargaining in the regression given in equation 3.1 with the GTCC indicator. The coefficient on the DDD treatment variable for workers in small establishments interacted with the GTCC indicator thus should provide an estimate if, within the treatment districts, workers with weak bargaining power in regulated product markets experience differential wage adjustments compared to workers in similar establishments in unregulated product markets.

All regressions are estimated via OLS with standard errors clustered at the district level, thereby allowing for serial correlation within districts over time as well as contemporaneous correlation between wage observations for workers in distinct labor market segments, but employed in the same districts.

3.5 Data and descriptive evidence

3.5.1 Data sources

We use data that is combined from three major data sources: (1) our novel database on the stationing and withdrawal of the U.S. Military Forces in West-Germany at the local level, (2) the full universe of the individual social security employment spell records for the four federal states in Germany where more than 95 percent of the U.S. personnel was based in 1990 (*Beschäftigtenhistorik, BeH*), and (3) firm-level information from the history of establishments (*Betriebshistorikpanel, BHP*). The construction of the final sample is identical with our previous work; hence, we only summarize in this section the main features of the data and focus our description on those facets that are of particular relevance for our analysis of heterogenous wage adjustments.[16]

The core element of our sample is full population of the social security employment spell data of individuals working in establishments that are located in the four federal states of Hesse, Rhineland-Palatinate, Baden-Württemberg and Bavaria already highlighted before. We draw random 10% subsamples at the district level.[17] The timing of the recorded employment spells matters in our DDD strategy for the identification of the date when the U.S. withdrawal shock starts to hit a given district. Hence, we focus on repeated cross-sections and require individuals to be employed on June 30 in a respective year for inclusion in the sample.

The administrative nature of the BeH spell data has a number of advantages for the purpose of our study. With all employment subject to social security contributions being registered,[18] the sample size allows the analysis of finely distinguished subgroups of workers.

[16] Further details on the data construction on the U.S. Forces are documented in aus dem Moore and Spitz-Oener (2012).

[17] Working with the full sample is neither sensible nor implementable within conventional limits of memory and computational speed.

[18] This reflects about 80% of the total working population. Not included are the self-employed, civil servants, students in higher education and members of the German Armed Forces.

Data quality in the employment data is usually very high with fewer items missing than in comparable survey data. Moreover, each individual employee spell includes a unique establishment identifier that can be used to merge detailed information on the location, the total number of employees, the industry affiliation and the exact date the establishment first was recorded within the social security system. This information is crucial for three reasons. Firstly, it enables us to assign each individual wage information the relevant U.S. Force level at the detailed regional level of districts (NUTS-3 regions). Since the employment spells containing the wage information are reported at the employer's location, any erroneous classification into treatment and control districts of employees who commute to work over district boundaries can be excluded. Secondly, the detailed occupational information available at the 3-digit level allows us to perfectly discriminate between workers in product markets regulated by the GTCC and those working in other areas. Thirdly, as already indicated in the description of the empirical approach, the detailed industry information can be used to construct controls for systematic changes in wages between industries.

On the opposite, the data also has some limitations that we try to address by various measures, but that still need to be taken into account for the later interpretation of our results. Most importantly, the wage information available in the BeH spells records an average gross daily earnings information that is constructed by dividing the total gross compensation paid out to an employee for the complete employment spell by the respective spell duration in days. As separate information neither on the amount of fringe benefits (bonuses, allowances etc.) or overtime pay included in this amount nor on the number of hours worked over the duration of the spell is available, changes in this variable potentially reflect to some degree also unobserved variation in these determinants instead of changes in the regular wage rate. As noted for example by Ludsteck (2008), this is a severe limitation, but one that impairs all studies of the wage curve and wage cyclicality based on the German social security registry data. Moreover, one-time payments were not subject to social security contributions and therefore employers did not systematically have to report them until 1983. This introduces a structural break into the wage data. Finally, the wage information is censored at the threshold that is set as the maximum amount of earnings on which social security contributions have to be paid. As in our previous work, we correct for top-censored wage observations using the standard imputation procedure developed by Gartner (2005), applied separately by groups of observations in each education level.[19] However, the quality of the imputation procedure strongly depends on the extent of censoring in the original data, as in many years more than half of the wage observations for employees with a high degree of education are affected compared to much smaller shares for employees with low or medium levels of education. In our analysis, we thus restrict our sample to male employees aged 25 to 55 with low and medium education in full-time employment for the time period 1984 to 2002.[20] In addition, we exclude employees in

[19] Cf. aus dem Moore and Spitz-Oener (2012) for details.

[20] As discussed in greater detail for example by Bauer et al. (2007), the restriction to (male) full-time

the agriculture and fishing, mining and quarrying as well as in the public sector as we do not expect our argument on the heterogeneity of wage adjustments by the influence and interplay of product market rents and wage bargaining institutions to be applicable in these industries. With these limitations and caveats in mind, we now proceed towards the definition of our main variables.

3.5.2 Definition of main variables

Wages: Our main dependent variable is the logarithm of the real daily gross earnings in year 2000 Euro. The deflator is the common consumer price index for West-Germany. Hence, due to the lack of a regional price index for our sample period, the deflation only corrects for changes in price levels over time, but not for price level variations between districts. However, for the DDD estimates this difference should largely be irrelevant, as workers in all labor segments within a given district face the same local price.

Labor market institution: Based on the discussion in section 3.3.2, the existence and influence of works councils in wage bargaining strongly depend on the size of the establishment. We thus define a proxy variable (W) for wage observations from workers in establishments with weak worker influence that have less than 50 employees, as we expect the management in these establishment to face less resistance in implementing real wage reductions compared to large establishments. Table 6.3 in appendix 6.2 provides descriptive statistics on the distribution of establishment sizes for our sample of individual observations across product market groups. While due to the sampling scheme, employees in large establishments are overrepresented, the overall size of the sample guarantees that we can estimate effects for all subgroups with very similar precision.

Product market regulation: The product markets in which firm entry is regulated by the German Trade and Crafts Code can be directly identified via the 3-digit classification of occupational titles of the Federal Employment Bureau (BA) (*Klassifikation der Berufe 1988, KldB88*). Out of a total of 334 occupational titles in the KldB88, we find 93 to be subject to the GTCC entry regulation. Hence, we create a dummy variable (R) coded one for all workers in these occupations to define the labor market segment in which product market rents from lower levels of competition are likely to occur.

U.S. Force levels: We capture the exact timing and extent of the base closures by means of a treatment intensity variable $\log \text{U.S.Forces}_{kt}$ that reflects (for each treatment district) the exogenous variation over time in the level of U.S. base personnel. For those years where the manpower level of the U.S. Forces in a given district has been reduced to zero in case of a full withdrawal, we add ones such that the log of the variable is well-defined for all treatment districts and all years in the post-periods.

Other variables: In our analysis, we make use of a set of demographic characteristics of the individual employee as well as of additional available information on the establishment.

employees does mitigate unobserved variations in regular hours worked, but cannot adjust for potential variations in overtime pay. To further address this concern, they cite additional evidence suggesting that the incidence of overtime work appears to be fairly stable in Germany.

To account for individual heterogeneity in wages and control for related potential differences in the composition of the group of employees that we analyze, we use a quartic in age, a dummy for foreign citizenship, a dummy for employees with a medium level of education (employees with low education are the base group), as well as dummies for eight occupation groups. As in our previous work, a low level of education is coded for people without any occupational training, whereas employees who have completed an apprenticeship or graduated from a vocational college are classified as having a medium level of education.

3.5.3 Descriptive statistics

In our fixed-effects DDD framework, we test our hypotheses about the heterogeneity of wage adjustments by comparing wage outcomes for employees in corresponding labor markets segments before and after the U.S. withdrawal shock between treatment and control districts. In order to facilitate the comparison and ensure the consistency with our previous work on the effects of the drawdown on various other margins of adjustment, we follow the same classification of districts in the treatment and control group as before.[21]

Table 3.1 provides raw means for our outcome variable of real gross daily wages for the set of 86 treatment and 96 control districts. The structure of the table is based on the standard logic in the literature for a differences-in-differences(-in-differences) setup. Hence we report separate means (and their standard deviations) for treatment and control districts, and in a further breakdown for two separate time periods from 1984 to 1990 and from 1991 to 2002. We also split the full sample along the previously described dimensions of the product market regulation and the labor market institution (as well as their interactions) that we want to investigate as sources of potential heterogeneity.

At first glance, the results seem to suggest some striking differences, with (significantly) higher wages on average in the treatment compared to the control districts, both for the whole sample and the subsample splits. However, in our application these differences do not lend themselves to easy interpretation and the calculation of a DDD estimate from the unadjusted means might be seriously misleading for several reasons. First, as we have documented already in aus dem Moore and Spitz-Oener (2012), our treatment districts are more densely populated and include more urban areas.[22] In our estimations, we can more flexibly control for systematic as well time-varying wage differences across locations by the inclusion of district-specific fixed effects and time trends. Second, the mean wages reported here also do not account yet for composition effects from differences in the distribution of observable characteristics of individual employees.[23]

Third, the split of the time periods around the year 1990 only provides a very rough approximation of the more complex timing structure of withdrawals that we are able to exploit in the regression analysis, as the precise timing of the respective withdrawal shock

[21] See the discussion in Chapter 2, section 3.5.

[22] Cf. table 2.2 in Chapter 2.

[23] In table 6.2 in the appendix 6.3, we provide further evidence that such differences do exist, with the group of employees in the treatment districts being somewhat older, more educated, and larger shares working in better paying occupations and industries.

Table 3.1: Mean gross daily wages in treatment and control districts, 1984-1990 and 1991-2002

	1984-1990			1991-2002		
Sample	Treatment	Control	Diff.	Treatment	Control	Diff.
Full sample (males 25-55)	90.26	84.94	5.32***	95.41	89.93	5.48***
	(1.44)	(1.04)	(1.77)	(1.64)	(1.05)	(1.95)
No GTCC firm entry regulation	93.18	87.28	5.90***	98.02	91.98	6.04***
	(1.51)	(1.12)	(1.89)	(1.71)	(1.12)	(2.05)
GTCC firm entry regulation	82.73	79.81	2.92**	88.01	85.08	2.94*
	(1.05)	(0.89)	(1.38)	(1.22)	(0.94)	(1.54)
Weak worker influence	80.31	77.44	2.87***	81.73	79.80	1.93**
	(0.86)	(0.63)	(1.07)	(0.68)	(0.66)	(0.95)
Strong worker influence	95.22	89.78	5.44***	103.37	97.19	6.18***
	(1.48)	(1.26)	(1.94)	(1.80)	(1.23)	(2.18)
No GTCC entry regulation &	82.77	79.33	3.44***	83.25	80.81	2.44**
weak worker influence	(0.92)	(0.70)	(1.15)	(0.76)	(0.77)	(1.08)
No GTCC entry regulation &	97.37	91.46	5.92***	105.10	98.57	6.53***
strong worker influence	(1.58)	(1.35)	(2.08)	(1.86)	(1.31)	(2.27)
GTCC entry regulation &	76.23	74.56	1.68*	78.87	78.11	0.76
weak worker influence	(0.72)	(0.55)	(0.90)	(0.54)	(0.52)	(0.75)
GTCC entry regulation &	88.02	84.98	3.04*	96.85	92.85	4.00**
strong worker influence	(1.09)	(1.12)	(1.57)	(1.50)	(1.17)	(1.90)

Notes: This table displays means of real gross daily wages (in year 2000 Euro) of full-time male employees with low or medium education aged 25-55, separately for 86 treatment and 96 control districts and time periods 1984-1990 and 1991-2002. Employees in agriculture and fishing, mining and quarrying as well as the public sector are excluded from the analysis. See the text in section 3.5 for details on the data sources, the wage definition, the distinction between the labor market segments and the selection of the treatment and control districts. Data in columns 4 and 7 indicate the mean differences between treatment and control district within the two time periods, with stars indicating significant differences from equality-of-means tests: * denotes significance at 10%, ** at 5%, *** at 1%.

substantially differs within the group of treatment districts, with some districts being only affected after 1994. Finally, the aggregation over all treatment districts does not capture the differences in size and speed of the local reductions in U.S. personnel that we aim to account for in our approach by the use of the treatment intensity variable.

3.6 Results

In our empirical analysis, we exploit the shock of the U.S. withdrawal to investigate if wage adjustments to negative demand shocks are heterogeneous for workers in distinct labor market segments characterized by different degrees of worker influence in wage bargaining and by different degrees of product market competitiveness. In the next two parts, we first explore the heterogeneity by each of these two institutional dimensions separately, before we combine them in part 3.6.3 and try to shed some light on how they might interact.

3.6.1 Heterogeneity of wage responses by level of worker influence

Table 3.2 presents the results from the OLS estimation of the DDD effect for workers in small establishments with weak bargaining power compared to their counterparts in establishments with strong worker influence within the treatment districts. The specification is based upon equation 3.1, with the LS_i indicator being replaced by the dummy variable W, taking the value of 1 if the individual wage observation stems from an establishment with less than 50 employees. For compactness, we only report the coefficient estimates on the relevant DDD effect here,[24] and provide estimates across four alternative specifications. Column (1) reflects the baseline specification as in equation 3.1, whereas in column (2)-(4), we further include state-year interactions, district-specific linear and quadratic time trends. These expansions should alleviate potential concerns about the comparability of our treatment and control districts.

Table 3.2: Heterogeneity of wage responses by level of worker influence

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
U.S. withdrawal x weak workers' influence (W)	-.001*	-.001*	-.001*	-.001*
	(.001)	(.001)	(.001)	(.001)
Other covariates:				
Labor segment indicator (W) x year effects	Yes	Yes	Yes	Yes
Labor segment indicator (W) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes
R^2	.43	.43	.43	.43
N	6,084,907	6,084,907	6,084,907	6,084,907

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

The coefficient on the DDD variable has the expected negative sign, suggesting that wages in small establishments with weak worker influence dropped relative to all other workers within the treatment districts. It is also marginally significant throughout all four specifications at the 10 percent level, but the size of the point estimate is tiny.

As argued above in section 3.4, this aggregate DDD effect might mask stronger differences once we take into account that the local demand shock of the withdrawal only affected demand in specific, mostly non-tradable industries. Hence we report in table 3.3 the results from analogous estimations performed separately for eight industry sectors. However, we find only slightly larger point estimates in absolute values for a negative DDD wage effect in industries like construction, which should have experienced a shortfall in their demand after the closure of the U.S. bases. In summary, these results suggest that the degree of worker influence in firm decision-making alone, as proxied by the lower rates of coverage

[24] More detailed results of these regressions are available upon request.

Table 3.3: Heterogeneity of wage responses by level of worker influence - subsamples by industry sectors

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
	1 Basic materials			
U.S. withdrawal x weak workers' influence (W)	-.001	-.001	-.001	-.001
	(.001)	(.001)	(.001)	(.001)
	2 Investment goods			
U.S. withdrawal x weak workers' influence (W)	-.002**	-.001*	-.001*	-.001*
	(.001)	(.001)	(.001)	(.001)
	3 Food and consumption goods			
U.S. withdrawal x weak workers' influence (W)	-.001	-.001	-.001	-.001
	(.001)	(.001)	(.001)	(.001)
	4 Construction			
U.S. withdrawal x weak workers' influence (W)	-.002**	-.002*	-.002*	-.002**
	(.001)	(.001)	(.001)	(.001)
	5 Retail/Repair			
U.S. withdrawal x weak workers' influence (W)	.001	.001	.000	.000
	(.001)	(.001)	(.001)	(.001)
	6 Transport/Information			
U.S. withdrawal x weak workers' influence (W)	-.002	-.002	-.001	-.001
	(.001)	(.001)	(.001)	(.001)
	7 Corporate Services			
U.S. withdrawal x weak workers' influence (W)	.000	-.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
	8 Private household services			
U.S. withdrawal x weak workers' influence (W)	.001	.001	.002	.002
	(.002)	(.002)	(.002)	(.002)
Other covariates:				
Labor segment indicator (W) x year effects	Yes	Yes	Yes	Yes
Labor segment indicator (W) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

of works councils in small establishments, does not induce differential patterns of wage adjustments to the negative demand shock.

3.6.2 Heterogeneity of wage responses by level of product market regulation

In this subsection, we briefly summarize our evidence regarding the isolated impact of the GTCC entry regulation on the wage response to the U.S. withdrawal shock. Table

3.4 depicts coefficient estimates for the DDD effect of the shock on wages for workers in the labor market segments subject to the GTCC regulation. In analogy to our discussion above, these estimates were obtained in OLS estimations of equation 3.1 on the pooled sample including observations for all industries, with the "wildcard" labor market segment identifiers replaced by the dummy variable R which groups together the observations of all workers employed in regulated product markets. The evidence seems to be clear-cut: the coefficient estimates in all four specifications are indistinguishable from zero. Moreover, these results also turn out to be very robust in the regressions on the subsamples by industry sectors (see table 6.4 in appendix 6.3). Therefore, we conclude that the wage response to the product demand shock does not systematically differ between those workers employed in the labor market segments affected by the GTCC entry regulation compared to all other workers.

Table 3.4: Heterogeneity of wage responses by level of product market regulation - pooled sample

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
U.S. withdrawal x GTCC regulation (R)	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)
Other covariates:				
Labor segment indicator (R) x year effects	Yes	Yes	Yes	Yes
Labor segment indicator(R) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes
R^2	.41	.41	.41	.41
N	6,084,907	6,084,907	6,084,907	6,084,907

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

3.6.3 Heterogeneity of responses from interactions of worker influence and product market regulation

Our results presented in the two preceding sections seem to suggest that neither the labor market institution of the works councils nor the product market regulation of the GTCC create a pathway for heterogeneous wage adjustments in response to negative economic shocks. However, this conclusion might be premature. If we take the theory of rent-sharing seriously and follow also the argument and evidence in Prantl and Spitz-Oener (2011) more narrowly, it is only the interaction of the two dimensions that should matter for differential outcomes in the wage adjustment process. In particular, based on our earlier discussion in section 3.4, we would expect that downward wage adjustments are more likely in segments of the labor market where product market rents allow for a wedge relative to the marginal product of the employees, but workers with weak bargaining power might have to forgo

part of their rent share in deteriorating economic conditions. In the remainder of this section, we aim to investigate this potential source of effect heterogeneity.

Table 3.5: Heterogeneity of wage responses from interaction of worker influence and product market regulation - pooled sample

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
U.S. withdrawal x (W) x (R)	-.001**	-.001**	-.001**	-.001**
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x weak workers' influence (W)	.001	.001	.001	.001
	(.000)	(.000)	(.000)	(.000)
U.S. withdrawal x GTCC regulation (R)	-.001	-.001	-.001	-.000
	(.000)	(.000)	(.000)	(.000)
Other covariates:				
(Weak influence (W) x GTCC (R)) x year effects	Yes	Yes	Yes	Yes
(Weak influence (W) x GTCC (R)) x district effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x year effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x district effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x year effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes
R^2	.43	.43	.43	.43
N	6,084,907	6,084,907	6,084,907	6,084,907

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

We therefore combine the two dimensions in a DDD specification where the interaction of the two dimensions (implemented as the product of the two dummy variables W x R) is also multiplied with the U.S. withdrawal treatment intensity variable.[25] The coefficient estimate on this variable should provide an indication whether the wage outcomes for workers employed in this particular segment where the two regulations interact differs from employees in all complementary labor market segments within the treatment districts.

Table 3.5 provides the coefficient estimates for the pooled sample including all industries. The coefficient on the DDD variable for the interaction of the two labor segments has the expected negative sign and is robustly significant at the 5 percent level, but still tiny. In table 3.6, we report the results from the separate regressions on the industry sector subsamples. Unsurprisingly and consistently with our previous results, the coefficient estimates for most industries are very close to zero. Nonetheless, within the industry sectors that are mainly composed of non-tradable services to businesses and to private

[25] As before, this regression model also contains the full set of interactions of (W x R) with the year as well as with the district fixed effects to account for time-invariant differences across districts and time-varying differences for this segment compared to workers in all other labor market segments. For the specification to be saturated, it also includes all DDD coefficients for the separate effects from the two regulations.

Table 3.6: Heterogeneity of wage responses from interaction of worker influence and product market regulation - subsamples by industry

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
	1 Basic materials			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.002	-.002	-.002	-.002
	(.001)	(.001)	(.001)	(.001)
	2 Investment goods			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
	3 Food and consumption goods			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.002*	-.002*	-.002	-.002
	(.001)	(.001)	(.001)	(.001)
	4 Construction			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.000	-.000	-.000	-.000
	(.001)	(.001)	(.001)	(.001)
	5 Retail/Repair			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	.001	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
	6 Transport/Information			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.000	-.000	-.000	-.000
	(.003)	(.003)	(.003)	(.003)
	7 Corporate Services			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.005*	-.005*	-.006*	-.006*
	(.003)	(.003)	(.003)	(.003)
	8 Private household services			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.006**	-.006**	-.007**	-.007**
	(.003)	(.003)	(.003)	(.003)
Other covariates:				
(Weak influence (W) x GTCC (R)) x year effects	Yes	Yes	Yes	Yes
(Weak influence (W) x GTCC (R)) x district effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x year effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x district effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x year effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

households (sectors 7 and 8), we now find more evidence of effect heterogeneity of the supposed type, with the negative coefficient estimates being five- to sevenfold larger in absolute value.

3.6.4 Robustness checks

Although our DDD design already controls for many potentially confounding factors that could affect these estimates, we further report here the results of two robustness checks for the interacted effect of the product and labor market regulation. The first potential concern is that our proxy indicator for the relevance of the works councils based on the establishment size ignores alternative explanations why the wage formation process could differ along the establishment size distribution. While our research design controls for detailed workers' characteristics that figured prominently as the source of wage differentials between larger and smaller firms in the early firm size literature, other explanations include various behavioral arguments (e.g., cost of monitoring, shirking, ability to pay) or introduce mechanisms that allow for the existence of sustained productivity gaps between small and large firms.[26] One strand of this literature has also raised the question whether firm size can be equated with firm age, with larger establishments being usually also older, perceived as being more profitable and potentially able to pay higher wages compared to newly founded firms.[27] As noted by Prantl and Spitz-Oener (2011), the classification into regulated versus unregulated product markets based on the occupational-level measure might also be less convincing for large establishments who could potentially be operating in many product markets. In contrast, at the very lower end of the establishment size distribution, there is presumably a clustering of small family-owned business with different mechanisms of profit- and risk-sharing. In fact, Addison et al. (2002) provide evidence that the legal status of a family-name firm is also related to a lower works councils presence at the plant level.

As the available information in our data limits our ability to further analytically discriminate among these factors, we try to address these concerns at least to some degree by repeating our analysis on a restricted sample where we exclude (both in treatment and control districts) all observations from workers in very small (less than 5 employees) and very large establishments (500 and more employees). Moreover, we also exclude observations from workers in new establishments ("startups"), founded within the last year.[28] Panel A of table 3.7 depicts the coefficient estimates from the regressions on this sample. For convenience, we restrict our attention here to the two sectors where we previously found significant effects. The coefficient estimate for the DDD term on the interaction of the two types of regulation within the "Corporate Services" sector apparently is sensitive to this

[26] See Oi and Idson (1999) for a review.

[27] However, the existing empirical evidence rather seems to suggest the contrary, with older firms paying low wages once differences in employees' characteristics are controlled for. See, e.g., Brown and Medoff (2003) who find for the U.S. a non-monotonic relationship between years in business and wages, or Kölling et al. (2005) and Brixy et al. (2007) for similar findings for Germany.

[28] A caveat with the IAB BHP establishment data in this respect is that in its standard form, it does not record the actual day of incorporation of a new establishment or the respective firm, but rather the first occurrence of a new employer ID in the social security notifications of employees. This event could also be triggered if an existing establishment is subject to a change of ownership or legal form, or when parts of a company are outsourced. See the BHP data documentation in Hethey-Maier and Seth (2010) for details. More recently, the IAB has provided an extension to infer with greater confidence the true day of incorporation from the analysis of worker flows. Cf. Hethey and Schmieder (2010).

Table 3.7: Robustness checks for effect heterogeneity by product and labor market regulations - selected industries

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
A. Restrict establishment sizes, exclude "startup" establishments				
	7 Corporate Services			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.004	-.004	-.003	-.003
	(.003)	(.003)	(.003)	(.003)
	8 Private household services			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.007**	-.007**	-.008**	-.007**
	(.003)	(.003)	(.003)	(.003)
B. Restrict sample period to 1984-1996				
	7 Corporate Services			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	.000	.000	.000	.001
	(.002)	(.002)	(.002)	(.002)
	8 Private household services			
U.S. withdrawal x weak workers' influence (W) X GTCC (R)	-.008**	-.008**	-.008**	-.007**
	(.004)	(.004)	(.004)	(.004)
(Weak influence (W) x GTCC (R)) x year effects	Yes	Yes	Yes	Yes
(Weak influence (W) x GTCC (R)) x district effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x year effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x district effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x year effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

sample variation, as the point estimate decreases in absolute value and turns insignificant. In contrast, the estimate for the differential effect within the "Private Household Services" sector remains at the same level in size and significance.

A second issue relates to the question about the timing of the wage adjustments in response to the economic shock. While a full-fledged analysis of the dynamic pattern of wage adjustments would require a further extension of the DDD model allowing for distinct effects relative to the respective starting date of the U.S. withdrawal shock, we restrict ourselves here to a simple test based on a variation in the sample period. In panel B of table 3.7, we report the respective DDD coefficient estimates from regressions in which we restrict the sample period to the years 1984 to 1996. Here, the estimates for the differential effect in the "Corporate Services" sector vanish completely and are indistinguishable from zero. For the effect within the "Private Household Services" sector, the point estimates remain unchanged, with standard errors only being slightly increased due to the reduction

in sample size.

3.7 Conclusion

In this paper, we analyze the impact of labor and product market institutions on the wage adjustment process in regional labor markets. We exploit the withdrawal of U.S. Armed Forces from Germany in the years following the reunification. The base closures by the U.S. Army and Air Force constituted an exogenous demand shock in some districts in Western Germany.

Our analysis in this paper is motivated by the argument that the notable absence of wage effects across entire regional labor markets (cf. aus dem Moore and Spitz-Oener 2012) might conceal an underlying effect heterogeneity on the more disaggregated level of labor market segments that differ in their degree of downward wage flexibility. To explore this source for potential heterogeneity of wage adjustments in more depth, we focus in this paper on two dimensions to segment the regional labor markets: Works councils as an institution that influences the bargaining power of employees and the German Trade and Crafts Code (GTCC) as a regulation that, by substantially limiting firm entry into certain product markets, influences the existence and size of rents from which employees might derive a non-negligible part of their wages.

Our results provide further support for the argument that the interaction of these labor and product market institutions lead to differential outcomes in the wage adjustment process. Analyzed in isolation, neither the segmentation along the labor market institution nor along the product market regulation reveal differential patterns of wage adjustments. However, for their interplay in the labor market segment that is simultaneously characterized by weak worker influence and product market rents from restrictions on firm entry, we robustly find the expected negative effect. The source of this effect can consistently be traced to those sectors that are mainly composed of non-tradable services to business and private households and that hence had the largest exposure to the demand shock under consideration.

4 Military bases and local crime - evidence from U.S. base closures in Germany

4.1 Introduction

In this study, we use the natural experiment created by the base closures and realignment process of the U.S. military forces in Germany in the early 1990s after the fall of the Berlin Wall to estimate the impact of military base closures on criminal activities.

After World War II and during the Cold War, West Germany derived many benefits from the presence of Western allied troops on its soil. With more than 60 percent of the major U.S. military bases overseas located in southwest Germany, the servicemen of the U.S. Army Europe (USAREUR) and U.S. Air Force Europe (USAFE) represented one of the largest peace-time deployment of troops in a foreign country in modern history. Beyond their indisputable and priceless geo-political contribution in strengthening and protecting the political freedoms and democratic development in West Germany against external threats during the Cold War, the U.S. military communities also created many economic benefits to their host regions. In previous work (aus dem Moore and Spitz-Oener, 2012), we have provided micro-econometric evidence of these economic benefits by documenting that the process of base closures and realignments after 1990 caused a significant loss in private sector employment, and in combination with the direct job loss of German civilian workers directly employed by the U.S. bases, led to rising unemployment in the affected regions. At the same time, the U.S. military presence in the German communities also involved conflicts between Germans and Americans such as noise disturbances, traffic violations, environmental pollution and in particular increasing levels of crime. Hence, a more comprehensive assessment of the socioeconomic impact of the U.S. drawdown on the affected regions requires evidence whether the adverse economic and labor market effects from the base closures have been offset by a simultaneous reduction in social costs that previously had impaired regional welfare.[1]

[1]The dimension of the social costs with the impact on crime as one core element was already recognized by the descriptive study of Bebermeyer and Thimann (1990) in their attempt of a cost-benefit analysis of the U.S. presence in Germany for the aggregate economy. They acknowledge that these costs are intrinsically hard to quantify:

> "There are numerous social costs linked with the U.S. forces' presence in the FRG which cannot be quantified, even though they represent an economic loss. There is no doubt that problems such as criminality, prostitution and drug addiction are enhanced by the US presence (particularly in larger cities where a high number of US soldiers live), often with costly consequence (e.g., AIDS). For all its economic implications, however, this complex problem is not expressible in figures and remains merely a qualitative element in the equation."(Bebermeyer

The relationship between military bases and local crime has also garnered public attention in other circumstances. Several press reports speculate whether the presence of a military base in the U.S. is associated with higher or lower crime rates in the surrounding communities.[2] Recently, this question has also been at the forefront of the discussions about relocations and closures of U.S. bases in other foreign countries, for instance in Italy, and particularly in Japan with regard to the conflicts about the large U.S. military bases on Okinawa, including a diplomatic stand-off between the Japanese Prime Minister and U.S. President Barack Obama.[3]

Beyond the specific interest in estimating the impact on local crime as a key element of the potential socioeconomic costs of the U.S. presence in Germany, our analysis of this relationship should also contribute to the broader understanding of the determinants of crime rates and their evolution over time at the regional level. Based on the vast and rapidly expanding literature on various determinants of criminal behavior, we see at least three mechanisms, how the presence, and respectively, a later closure, of a military base could impact on local crime rates. These include a mechanical effect from the variation in the potential pool of offenders in the local community, an economic effect spurred by the changing local economic conditions after the base closure process, as well as potential behavioral effects that arise from the social interactions between U.S. soldiers and the German population. While conceptually, our study reviews and analyze the expected role of these channels, the crime data used for our empirical analysis precludes the opportunity to disentangle their respective influence in the overall effect that we estimate. In particular, our data on crime offenses does not detail the nationality or resident status of the offender which would allow us to back out and evaluate the importance of the mechanical effect versus the other channels. However, we still try to shed some light on the relative importance of the various pathways by exploiting the fact that the strength of these effects can reasonably be expected to differ for detailed subcategories of crime as well as relative to the timing when the base closures occur.

In our empirical work, we exploit the withdrawal and realignment of U.S. Forces after the Cold War that led to a substantial reduction and reallocation of U.S. defense resources in Germany to examine the causal impact of military base closures on crime rates. As documented in detail in aus dem Moore and Spitz-Oener (2012), the drawdown of U.S. military personnel provides a unique natural experiment setting. The realignment decisions administered by the U.S. Department of Defense were governed almost exclusively by changing strategic military targets. In light also of the speed of the process, they did not respond to the concerns and preferences of German authorities and local stakeholders in the German communities. Moreover, the available evidence from official documents as well as historical accounts suggests that the selection rule of sites did not depend on the local socioeconomic conditions, including the level of crime.

and Thimann, 1990, p. 109)

[2] See, e.g., Engber (2009); Watson (2009).
[3] See Fackler and Tabuchi (2010); Fackler (2010); Tabuchi (2010); vanden Heuvel (2012).

Although causes of criminal behavior at the regional level are extensively studied in previous research, there is (to the best of our knowledge) only one comparable study examining how military base realignments and closures affect local crime rates. Paloyo et al. (2010) study the impact of a recent wave of national base realignments on local crime rates in Germany between 2001 and 2007, but do not find any effect on local crime rates.[4]

In this paper, the causal effect of the U.S. withdrawal is estimated using a district-level panel covering the period 1984 to 2002. A fixed effects difference-in-difference approach is used to disentangle the impact of U.S. withdrawal on different criminal offenses from potential district-specific and time-variant factors that could influence local crime rates.

The results reveal that the effect of the drawdown of U.S. Forces on local crime rates varies for different categories of crimes. Most notably, our results suggest that the U.S. base closures led to a significant drop of up to 20 percent in local crime rates for drug offenses. This effect is persistent and robust to various alternative variations in the data sample and estimation technique. In addition, we also find evidence for a reduction in the incidents of rape, although these estimates are based on small counts. For our complete sample, no significant effect is found for more aggregated crime rates such as the total crime rate and the rate of total theft. Further analyses reveal some effect heterogeneity with regard to the area type, with the drop in crime rates being concentrated in urban districts.

The remainder of the paper proceeds as follows. In the next section, we first discuss the conceptual background how military bases can affect local crime rate and then briefly summarize the historical context of the withdrawal of the U.S. Forces. Section 4.3 discusses the empirical strategy, and section 4.4 describes the data set that we use. The estimation results are presented in section 4.5, and section 4.6 concludes.

4.2 Background discussion

4.2.1 U.S. bases and local crime

In this section, we describe three key mechanisms that lead us to expect a causal relationship between the presence and later withdrawal of U.S. forces and local crime rates: a mechanical effect from the reduction in the local pool of potential offenders, an economic mechanism working via the increase in unemployment after the base closure shock, and a behavioral channel that works via the social interactions between U.S. soldiers and the local population. For each mechanism, we review arguments from the economic literature on the relevant determinants of crime and discuss on the basis of historical accounts how these factors

[4] A priori, their findings only provide little guidance in forming an expectation about our results at least for three reasons. First, the German base realignments that they study were much smaller in size. Second, the different demographic composition among members of the German Army (including a substantial share of conscripts who often spend their free time in their native home towns) and a smaller sense of alienation in their base communities might lead to a lower propensity to be involved in criminal activities. Third, the exogeneity of the base realignment decisions might be more questionable given the many ways for local stakeholders to influence this process in the German parliamentary system.

could have shaped the empirical effect that we estimate later on.

Firstly and most evidently, we expect a mechanical effect of the withdrawal on local crime. The U.S. military base personnel increases the pool of potential offenders in the local community. The demographic composition of the military personnel also suggests that U.S. soldiers might be more susceptible to become involved in criminal incidents. Several studies find that young men, who constitute the largest share of military personnel, are more susceptible to engage in criminal activities (see, e.g., Freeman, 1996; Levitt, 1998; Grogger, 1998; Jacob and Lefgren, 2003). Moreover, some recent compelling evidence suggests that (mandatory) military service itself could directly cause a higher propensity to commit crimes also in civil life, particularly for violent crimes involving weapons (Galiani et al., 2011; Lindo and Stoecker, 2012).[5] Finally, the exact demographic composition of the base personnel might matter with respect to their level of education and family status, with higher qualifications found to lower the probability of engaging in criminal activities (e.g., Freeman, 1996; Lochner and Moretti, 2004; Machin et al., 2011) and marriage and fatherhood often presumed to have a preventive effect (e.g., Sampson et al., 2006; Bersani et al., 2008; Skardhamar and Lyngstad, 2009). These ideas seem to resonate with the anecdotal accounts of the higher prevalence of criminal activities around U.S. Army rather than U.S. Air Force bases in Germany as the Army service personnel was on average less well educated, younger and exhibited lower marriage rates than their Air Force comrades (Bebermeyer and Thimann, 1990).

In summary, we expect the direct mechanical effect of the withdrawal of U.S. military personnel to cause a reduction in number of offenses relative to the population size of the local community. However, the importance of this effect might be very heterogeneous according to the type of offenses. Table 4.1 reports the number of suspects of all foreign military forces and their dependents stationed in West-Germany as well as their share in non-German suspects and in total number of suspected individuals. The share of foreign military forces in number of recorded suspects overall is relatively moderate (at 0.6 percent in row 1), but above-average shares are evident for offenses like damage to property, sex offenses, bodily injury, serious assault and robbery as well as drug offenses compared to below-average shares for theft.

The argument for an economic mechanism of the relationship between the U.S. presence and local crime can be based on the shift in the economic conditions in the local community after the base closures. Naturally, the relationship between economic conditions both at the individual and regional level has been at the forefront of the analysis of crime by economists since the pioneering works by Becker (1968, 1974) and Ehrlich (1973). Nonetheless, the evidence on the causal nature and the direction of the causation between GDP per capita, income inequality and crime drawing on cross-country evidence seems to be mixed and contradictory (see, e.g., Fajnzylber et al., 2002; Neumayer, 2005). Recently, more robust

[5] Lindo and Stoecker (2012) also consider that military service could reduce crime rates as it serves as an incapacitation especially for young person at the ages at which they are most susceptible to commit crimes. However, this argument is weakened by the fact that soldiers may leave their bases during non-working time and then again are affected by external influences.

Table 4.1: Number of suspects of foreign forces stationed in Germany and their dependents, year 1990

		Percentage share in total number of	
	Number of suspects	Non-German suspects	All suspects
Total offenses	8,653	2.26	0.60
Sex offenses	259	6.80	1.43
of which			
Rape	107	9.32	3.04
Drug offenses	723	3.86	0.90
Bodily injury	1,914	4.70	1.00
Serious assault	916	4.96	1.32
Robbery	211	3.07	1.09
Theft in total	2,614	1.66	0.46
of which			
Breaking and entering	128	1.88	0.35
Theft of motor cars	152	4.56	0.80
Damage to property	1,532	11.88	1.68
Misdemeanors against the public order	338	3.21	0.51
Offenses against life	22	2.91	0.67

Notes: Individuals who are suspected of having committed the same crime several times within a reporting period are statistically recorded once. If a person is accused of having committed several crimes he is counted separately in each crime category. The sum of all suspects in each subcategory of offenses therefore does not add up to the total number of suspects. Additionally, the total number of suspects does not correspond to the number of offenses known to the police. Source: Bundeskriminalamt (1984-2002).

within country evidence (e.g., Kelly, 2000) and in particular, micro-econometric studies using sound methods of identification have established a more convincing causal relationship between labor market opportunities (as measured by unemployment, wages and legal income opportunities) and criminal activities (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Corman and Mocan, 2005; Öster and Agell, 2007; Mocan and Unel, 2011). As shown previously, the closure of U.S. military bases did induce a negative exogenous shock on the local labor market of areas that previously had benefited from the demand of U.S. military personnel for German goods and services and the direct employment of German workers by U.S. services. The withdrawal led to a decrease in private sector employment and rising unemployment in the affected economies (aus dem Moore and Spitz-Oener, 2012). Hence, an increase in criminal activity among the local residents in response to a rise in unemployment could partially offset the direct mechanical drop from the withdrawal of the U.S. forces. However, this potential economic channel might only be active in the medium to long run, as the unemployment rate itself is responding only slowly to the base closure shock and the link between unemployment and offending is rather operating at a time lag as well. Moreover, such a rise in local crime rates should likely be more concentrated in violent and property crimes.

A third mechanism for the link between the U.S. bases and crime is based on the role of social interactions in determining criminal behavior. The core idea is that the strong variation in crime rates between locations with comparable economic conditions can better be accounted for if the rational decision for a criminal activity can positively depend on the actions of a social neighbor (Glaeser et al., 1996). This dependence can then give rise to social contagion and peer effects that might be particularly relevant for the explanation of criminal behavior for certain types of crime (e.g., larceny, drug abuse) and their clustering and persistence in densely populated urban areas. In the case of the U.S. bases, several pieces of anecdotal evidence support the view that such a "behavioral multiplier" (Freeman, 1999) could be at work particularly for drug offenses. The problem of drug abuse among the U.S. forces in Germany was officially documented in 1979, when a Committee of the House of Representatives held hearings and commissioned a report on it, finding that up to 15 percent of the troops stationed in Germany could be consumers of hard drugs and the soft drug usage could exceed 75 percent.[6] In the context of the withdrawal of the U.S. troops, the existence of social interaction effects between the delinquent behavior of U.S. servicemen and criminal activities by local residents could possibly manifest itself in lower reductions of crime rates as a pure mechanical effect might bring about. This would suggest that the districts of former base locations would record inflated crime rates even after the withdrawal.

4.2.2 U.S. bases in Germany before and after 1990

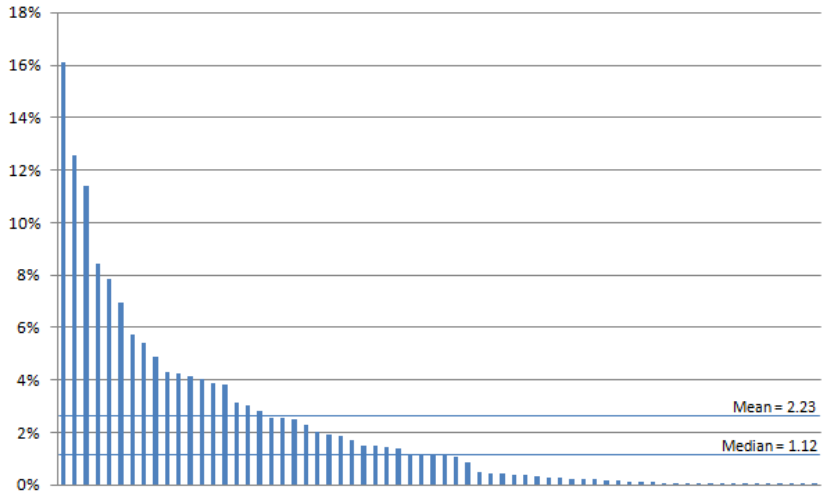
After the end of World War II Germany was marked by the installation of four occupation zones administered by the Allied Forces (American, British, French and Soviet). The deployment in Germany primarily was driven by the fear of a possible restrengthening of the German military. Without an initial intention to install a long-term military presence, the establishment of the Federal Republic of Germany in 1949 and the related threat of a potential Soviet expansion in Germany forced to further extend the U.S. defense network. The Berlin Blockade as well as the Korean War confirmed the menace of a potential conflict resulting from Soviet objectives and forced the creation of NATO in April 1949. Consequently, the U.S. established further strategically important military sites in Germany. This widened U.S. Forces coverage also beyond the boundaries of the U.S. zone assigned in 1945 which initially comprised the area that today forms the federal states of Bavaria, Hesse and the north of Baden-Württemberg as well as Bremen, Bremerhaven and

[6] U.S. Congress (1979, p.11) Browder (1998) argues that alcohol and drug abuse often triggered delinquent behavior which sometimes resulted in arbitrary and unprovoked violent reactions. A paragraph in the article by Seeger (1978) hints toward the tight social link between the drug usage by U.S. soldiers and German youngsters:

> "Many German experts believe that the drug problem of the 1970s is a reversal of a pattern a decade earlier. Hard drug abuse in Germany in the 60s was blamed on the U.S. draftee army and the Vietnam War as abuse spread into the German civilian community. Now hard drugs are cheap and available in the German civilian community, particularly among those in their 20s, and apparently have spread in reverse direction to the U.S. military ranks."

the American sector in Berlin.[7] In numbers, after a vast decrease in troops level to less than 100,000 soldiers in the first post-war years, the installation of more than 800 manned and unmanned sites between 1950 and 1989 raised the level of U.S. Forces significantly. Since the 1950s, around 250,000 U.S. servicemen were permanently stationed in Germany. At the regional level, Figure 4.1 depicts the number of U.S. military forces relative to the total population in 1990 for the districts with U.S. bases included in our sample and shows that with a mean (median) of 2.29 (1.11) percent, U.S. forces had a marked impact on local demography.

Figure 4.1: U.S. military forces relative to total population for treatment districts (N=66), Year 1990



Source: Own calculations based on original data on U.S. forces in Germany. See text and appendix 6.1 for details.

The end of the Cold War and the German reunification on October 3, 1990 induced the reorganization of the U.S. presence in Germany. Sites to be realigned and closed were selected with the goal to maintain those bases that most efficiently fit the strategic requirements of the U.S. military. [8] Between 1990 and 1996, the U.S. Department of Defense and the commands of the U.S. Army Europe and U.S. Air Force Europe publicly announced and implemented 21 rounds of base closures and realignments in Germany. In consequence, the manpower levels of U.S. forces were drastically reduced by more than 75 percent compared to their pre-1990 level.

[7] For more detailed information on U.S. military installations in Germany see Duke (1989), p.56-148 and Cunningham and Klemmer (1995).

[8] A more detailed description of the decision procedure is given in Cunningham and Klemmer (1995), p.44-47.

4.3 Empirical approach

In light of the exogenous selection of districts that experienced a U.S. withdrawal after 1990, the goal is to estimate the effect of this drawdown on district-level crime rates. This section discusses our panel difference-in-difference (DD) model used to estimate this effect.

Our baseline regression specification estimated by ordinary least squares (OLS) is given by the following equation:

$$\log(\text{crime rate}_{dt}) = \lambda_t + \gamma_d + \theta_{st} + \beta \times \text{U.S. Withdrawal}_{dt} + \epsilon_{dt} \tag{4.1}$$

where the dependent variable $\log(crime\ rate_{dt})$ measures the natural logarithm of the number of offenses of a particular crime known to the police relative to 100,000 residents for a district d in year t. The parameter of interest is denoted by β and is determined by the dummy variable $U.S.\ Withdrawal_{dt}$. This dummy variable takes the value of one once a treatment district d has experienced the announcement of a U.S. military base closure and zero otherwise. Accordingly, for a district being exposed to a base closure in the year $t-1$, $U.S.\ Withdrawal_{dt}$ equals one in the period t and afterwards and zero before.[9] $\hat{\beta}$ thereby provides an estimate of the semi-elasticity of the crime rate to the U.S. withdrawal shock.[10] λ_t are the year fixed effects that control for time trends in local crime rates that are common to all districts, and the district-fixed effects γ_d adjust for time-invariant factors that vary across districts.

State-year fixed effects are denoted by θ_{st} and absorb time-varying fluctuations that affect both treatment and control districts within the same federal state. These could be related to differences in reporting of offenses over time at the level of the State Police Offices as well as differences in crime prevention and police activities at the federal state level, the legislative and executive branch governing regular police work within the federal system in Germany. ϵ_{dt} represents the stochastic error term.

In some specifications, we also include a vector of district-specific time varying characteristics that figure prominently as determinants of crime in the literature. These include (in logs) the share of the foreign population, the share of the male population aged 15 to 25, as well as the unemployment rate. However, as the U.S. withdrawal potentially also affects these characteristics, they are (at least in part) endogenous and hence subject to a

[9] Note that this timing convention is conservative and potentially works against the possibility to find an effect, as a partial response in the crime rate might already occur in year $t-1$ if the initial base closure is announced and implemented early in the respective year. In aus dem Moore and Spitz-Oener (2012), we were able to ascertain with higher precision the occurrence of the shock relative to the specific date for which our employment outcomes were recorded each year. Unfortunately, the crime data is available only on an annual level, thereby requiring us to make a more ad-hoc decision on the assignment of observations into the pre- and post-periods. However, our results do not seem to be strongly affected by this convention which we can partly verify in section 4.5.3 when we explore the dynamic pattern of the withdrawal effect.

[10] Note that we employ the standard DD model here and do not include the log U.S. personnel count at the district level as a measure of the treatment intensity as in aus dem Moore and Spitz-Oener (2012). The reason for this difference is that for the crime outcomes, the relationship seems to deviate from a log-linear relationship, and hence the standard non-parametric DD specification provides a more robust estimation method.

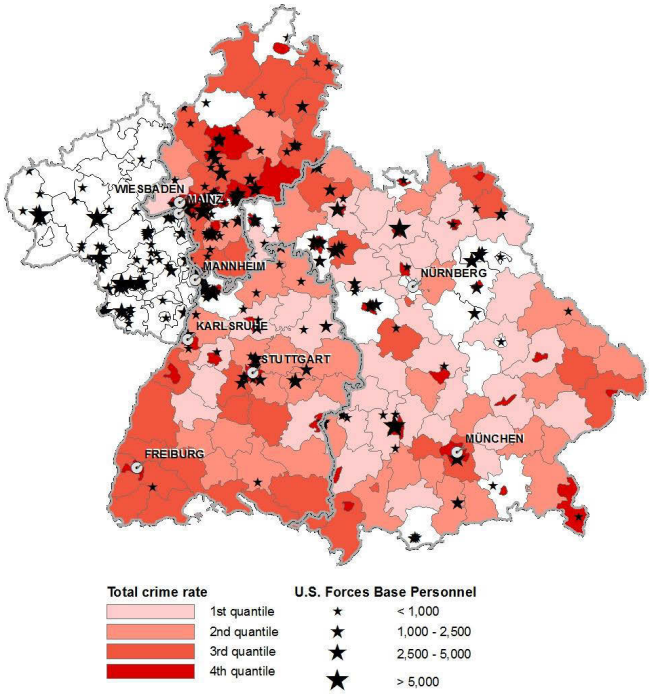
problem of "bad controls", so that we do not use them in our baseline estimation.

Similar to the previous work in aus dem Moore and Spitz-Oener (2012), we also consider further extensions of the model in (4.1) by including a set of separate DD variables for each period defined in relation to the respective timing of the withdrawal shock in the treatment districts to explore the dynamic pattern of the effect. In some models, we also include linear district-specific time trends and consider the sensitivity of our results to the use of alternative estimation procedures. In all specifications standard errors are clustered at the district level to allow for serial correlation within districts over time.

4.4 Data description

The analysis in this paper is based on a district-level panel data set of yearly crime rates for the period 1984 to 2002. With the U.S. military bases being spread out through southwest Germany, the 66 treatment and 82 control districts are located in Baden-Württemberg, Bavaria and Hesse. Unfortunately, no crime data at the detailed district level was available for Rhineland-Palatinate before 1994. This is a severe restriction compared to our previous work, as nearly 30 percent of the total U.S. presence was stationed in this federal state. The map in figure 4.2 illustrates the location of the districts in the sample as well as the spatial distribution of the total crime rate in 1990.

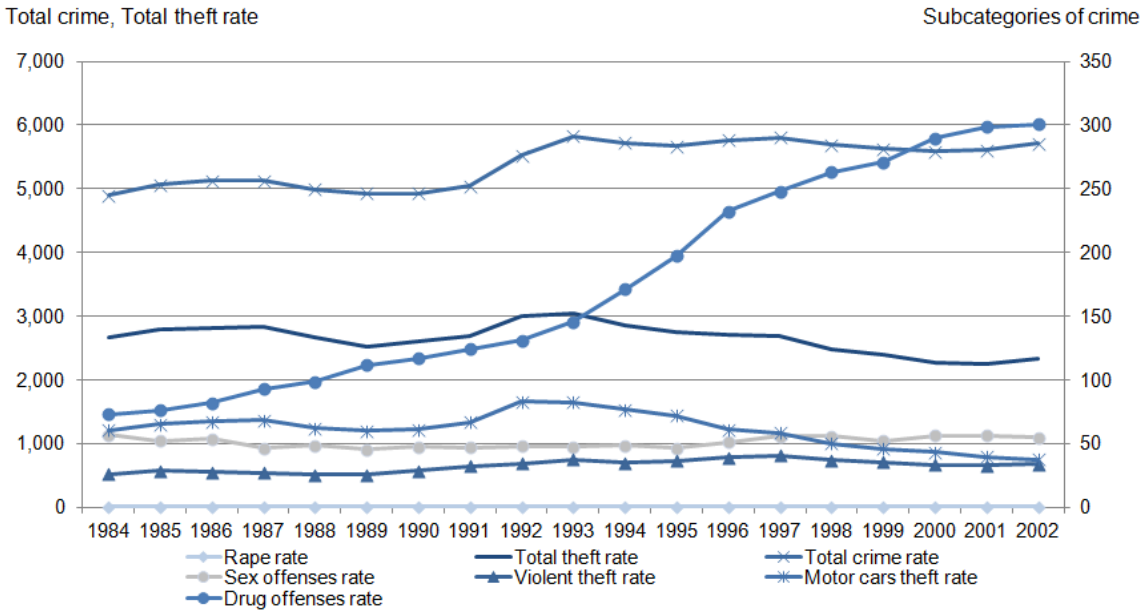
Figure 4.2: Location of U.S. military bases and total crime rates, 1990



Source: Own calculations and illustration based on original data on U.S. forces in Germany. See text and appendix 6.1 for details.

Disaggregated crime data on the number of cases known to the police up to the year 1998 is provided by Entorf and Spengler (2002).[11] They collected data on several categories of offenses on district level from the German State Criminal Police Offices. Based on our own data collection effort with requests to the respective State Criminal Police Offices, we were able to extend the available time series of this data for the three federal states up to the year 2002. Besides the total number of offenses, the subcategories of crime used as an outcome variable in this study include the rates of drug offenses, sex offenses and property crimes such as the total number of thefts, robbery, as well as theft of motor cars.[12] Figure 4.3 shows the development of the respective offense rates over time for our whole sample period.

Figure 4.3: Development of total crime rate and subcategories of offenses, 1984-2002



Source: Own calculations. See text for details on data sources.

The data quality of the official crime statistics depends inter alia on the exact definition of a crime and the willingness of victims to report an offense. Differences in the reporting thereby could lead to a measurement error in the number of crimes.[13] However, since crime rates serve as dependent variables in our analysis, this will not bias the results as long as the measurement error is independent of the regressors. As a hypothetical example, this condition would not be fulfilled if districts with a high share of U.S. soldiers (or a high reduction in level of U.S. Forces during the withdrawal process) systematically

[11] We thank Horst Entorf and Hannes Spengler for graciously making their data available to us for this project. For more information on the data collection see Entorf and Spengler (2002), Ch. 5.

[12] See appendix 6.4 for a detailed description of the offenses included in each category.

[13] As an example, incidences of damages and losses resulting from criminal activities like theft rather correspond to the actual number of crimes as the official complaint of the offense is a precondition for claiming insurance payments. These collection quotas stand in contrast to delinquencies such as corruption or drug offenses where the victim is not always identifiable and numbers of offenses are thus potentially underreported.

underreport crime rates not to put a strain on the relationship with U.S. Forces. As a result, the coefficients would be downward biased. However, we do not have any evidence of such systematic error. Moreover, the inclusion of district and year fixed effects in all our estimations should address any potential source of distortion from unsystematic changes in reporting behavior (see Wooldridge (2002), Ch. 4.4).

Table 4.2: Districts with U.S. military presence included in the sample

	Number of districts	Number of bases	Total U.S. Forces (in 1990)	Share of total
Districts with assigned U.S. base personnel in West Germany in any year 1986-2009	130	486	360,091	100.0
Districts with assigned U.S. personnel in the 3 federal states HE, BW, BY	84	333	243,285	67.6
of which				
Districts with U.S. presence without withdrawal announcement until 2001	8	23	9,240	2.6
Districts with missing data and unmanned bases	7	7	285	0.1
Districts with missing/outlying crime data †	3	12	14,310	4.0
Number of districts in the sample	66	291	219,450	60.9

Notes: In all excluded districts that had no withdrawal announcement until 2002 the first base closures were made public in 2001 and later on. †The districts that were excluded as the crime data was missing or unreliable in all or some of the years in the sample period are: 9374 - Neustadt an der Waldnaab, 9563 - Landkreis Fürth, 9564 - Nürnberg
Source: Own calculations. See text and appendix 6.1 for details.

Data on the drawdown of U.S. military forces in Germany including the number of U.S. base personnel at district level and information on the exact announcement dates of base realignments are based on aus dem Moore and Spitz-Oener (2012). Table 4.2 gives an overview of the districts with U.S. forces comprised in the baseline sample. With 299 military bases installed in the 66 treatment districts used in this analysis about 61 percent of the total U.S. presence in West Germany is covered. The mean value of U.S. forces personnel in the treatment districts in the year 1990 before the start of the drawdown was 3,325 which is equivalent to 2.3 percent relative to the district population. The average (median) withdrawal between 1990 and 2002 is described by a reduction in U.S. personnel by about 2,440 (1,395) individuals.

Additionally, the data set contains annual data for total number of residents, the male population in different age brackets and the foreign population in a district which is obtained from the Federal Statistical Office and the State Statistical Offices. The unemployment rate at the district level is obtained from the Federal Employment Agency and is calculated as the number of unemployed relative to the dependent civilian labor force. For regional information, an adapted classification scheme of the area types as assigned by the German Federal Office for Building and Regional Planning (BBR) is used based on the work by

Möller and Lehmer (2010).[14]

Table 6.6 presents descriptive statistics in a breakdown by treatment and control districts for the time periods 1984-1990 and 1991-2002. The total crime rate measured as the number of crimes known to the police per 100,000 persons reveals a high variation in reported crime incidences across districts in both time periods. Cases of total theft account for about half of the total crime rate in both periods and set of districts. Drug offenses constitute the highest crime rate among the more specific offenses that are analyzed in this paper. When looking at property crimes, the relative number of reported offenses is highest for theft of motor cars. The crime rate for rape makes the smallest proportion of total offenses.[15]

The sociodemographic characteristics reveal that our set of treatment districts includes districts that are on average more densely populated and more urban than the control districts. This highlights the need to allow for district-specific fixed effects in our regression models. However, it would only invalidate our research design if cross-sectional differences in levels determine future changes in our crime outcomes. We try to address this source of potential misspecification in two ways: in our baseline specifications, we include state by year fixed effects. Moreover, in a robustness check we also allow the effect of the U.S. withdrawal treatment to differ between urban and rural districts.

[14] Cf. table 6.1 in appendix 6.3.

[15] In fact, the small count of rape offenses with some districts reporting no incidents at all raises a concern for our model specification based on OLS regressions with the log crime rate as the dependent variable. In our baseline analysis, we add ones to crime counts of zero at the district level so that the log of these rates is always defined. In a robustness check, we later validate our results particularly for those crime rates that are based on small counts by using a negative binomial variant of a Poisson regression that is also often applied in research on crime for regional units of smaller population size.

Table 4.3: Selected district characteristics by treatment status

	1984-1989		1990-2002	
	Treatment	Control	Treatment	Control
	(1)	(2)	(3)	(4)
Incidence of Crime				
Total crime rate	5,724	4,460	6,180	5,090
	(3,264)	(2,103)	(3,110)	(2,351)
	[2,156;23,251]	[1,684;13,863]	[2,255;21,345]	[404;14,894]
Drug offenses rate	108	74	228	203
	(80)	(68)	(180)	(166)
	[7;534]	[3;468]	[17;1,055]	[5;1,223]
Sex offenses rate	55	47	55	48
	(30)	(29)	(28)	(24)
	[8;170]	[8;202]	[15;192]	[6;182]
Rape rate	9	6	8	7
	(5)	(4)	(5)	(5)
	[0;30]	[0;37]	[0;42]	[0;41]
Theft rate (total)	3,204	2,320	3,042	2,280
	(2,298)	(1,400)	(1,892)	(1,328)
	[898;16,354]	[644;9,582]	[840;13,077]	[692;8,985]
Robbery rate	34	21	44	28
	(36)	(20)	(46)	(26)
	[1;219]	[1;132]	[4;374]	[2;205]
Motor cars theft rate	81	50	74	48
	(72)	(32)	(70)	(36)
	[8;456]	[4;205]	[10;563]	[4;402]
Demographics				
Population	195,315	120,882	210,279	134,027
	(174,894)	(62,017)	(178,440)	(68,973)
	[33,703;1,274,716]	[37,284;373,065]	[35,514;1,256,638]	[39,333;415,764]
Population density	635	292	669	317
(inhabitants per sqkm)	(770)	(342)	(787)	(363)
	(66;4,106)	(66;1,795)	(70;4,048)	(71;1,893)
Share young male population	0.093	0.098	0.062	0.065
(age 15-25 years)	(0.030)	(0.034)	(0.015)	(0.020)
	[0.060;0.192]	[0.061;0.202]	[0.047;0.147]	[0.043;0.168]
Share foreign population	0.077	0.057	0.105	0.081
	(0.045)	(0.031)	(0.056)	(0.039)
	[0.007;0.229]	[0.008;0.140]	[0.022;0.280]	[0.015;0.289]
Socio-economic indicators				
GDP per capita†	38,423	32,988	45,217	38,470
(in national currency)	(17,441)	(10,721)	(19,648)	(12,429)
	[12,671;115,366]	[14,033;73,236]	[14,686;127,542]	[17,416;89,604]
Unemployment Rate	6.6	6.5	6.8	5.9

Continued on next page

Table 4.3 – ***continued from previous page***

	1984-1989		1990-2002	
	Treatment (1)	Control (2)	Treatment (3)	Control (4)
	(2.3) [2.6;23.6]	(2.8) [2.6;22.2]	(2.4) [2.1;16.8]	(2.3) [1.8;18.9]
Area type				
Urban	0.364	0.122	0.364	0.122
Border districts	0.152	0.402	0.152	0.402
Geographic distribution				
Hesse	0.288	0.049	0.288	0.049
Baden-Württemberg	0.273	0.305	0.273	0.305
Bavaria	0.439	0.646	0.439	0.646
N	66	82	66	82

Notes: This table display means/shares for the sample of treatment and control districts used in the analysis. Values in round brackets report standard deviations, whereas values in squared brackets are [Min; Max]. Crime rates are defined as the numbers of crimes known to the police per 100,000 residents.
†Due to data limitations, GDP per capita are reported only until 1997.

4.5 Results

4.5.1 Total crime rate

We start our exposition of the results with the regression specifications that use the log of the total crime rate (number of total offenses per 100,000 district residents) as the dependent variable. In the first regression based on the model set out in equation 4.1, the variation in the total crime rate is solely explained by the full set of district fixed effects and year effects and our regressor of interest, the U.S. withdrawal treatment variable which takes the value of one after the first closure of a base hosting U.S. military forces occurred in a district and zero otherwise. The coefficient estimate ($\hat{\beta}$) in column (1) of table 4.4 suggests that the U.S. drawdown is associated with a negative and significant effect on total crime. Taken at face value, the estimate of -0.041 would suggest an average reduction in the total crime rate of 4.1 percent in the post-withdrawal years in the treatment relative to the control districts.

In column (2), we add state by year fixed effects that control for fluctuations common to treatment and control districts located within the same federal state. While this increases the precision of the coefficient estimate, the absolute value of the point estimate drops to -0.015 and turns insignificant. Adding the three demographic and economic covariates in column (3) does not seem to alter our coefficient estimate of interest. While the coefficients on the three covariates do have the expected positive sign (although only the share of the foreign population obtains a large and significant point estimate), the fit of the model

Table 4.4: Effect of U.S. Forces withdrawal on the total crime rate

	(1)	(2)	(3)	(4)
U.S. withdrawal	-.041**	-.015	-.012	-.008
	(.021)	(.018)	(.018)	(.016)
Log share male population 15-25			.016	.014
			(.013)	(.011)
Log share foreign population			.127***	.069
			(.042)	(.048)
Log unemployment rate			.028	.020
			(.028)	(.027)
District, year fixed effects	Yes	Yes	Yes	Yes
State-Year fixed effects	No	Yes	Yes	Yes
District-specific time trends	No	No	No	Yes
N (district x year)	2,812	2,812	2,812	2,812
R^2	.977	.980	.980	.986

Notes: *Significant at 10%, ** at 5%, *** at 1%; Robust standard errors clustered at district level in parentheses.

is not increased substantially. We interpret this as evidence suggesting that most of the time-varying influence of these covariates is already absorbed by the set of district, year and state-by-year effects. Based on our concerns already discussed in section 4.3 that these variables are not exogenous to the withdrawal shock, we decided to leave them out of our preferred baseline specification depicted in column (2). In column (4), we further add district-specific linear time trends. While this in principle increases the amount of variation explained by the regression model, it appears no longer possible to identify any effect on the DD treatment variable or the covariates. This may hardly be surprising given the reduced number of districts in our sample for the three federal states with most of the observations stemming from the post-withdrawal period after 1990. This limits the opportunity to identify stable time trends in the pre-period without sweeping out all potential variation due to the withdrawal shock.[16]

Overall, the results presented thus far only suggest a small and rather insignificant reduction in total crime in the districts with a U.S. presence after the withdrawal. One potential explanation could be related to the composition of total offenses. Beyond incidences of theft which make about half of total offenses, the total number of crimes comprises several other categories of crime such as the forgery of documents, environmental offenses and embezzlement. Based on the insight from our earlier discussion that the majority of offenses that are either directly committed by U.S. servicemen or could be related to the presence of the U.S. bases is limited to some specific categories of crime the estimates show that the effect on the overall crime rate seems to be marginal. This provides the motivation for the analysis of more specific subcategories of crime in the next

[16] Another piece of evidence in support of this rationale is that in analogous regressions for the subcategories of crime, the inclusion of those trends reduced the precision of the estimates, hence suggesting that the linear form may not be adequate.

subsection.

4.5.2 Subcategories of crime

Based on our discussion in section 4.2.1, we expect drug and sex offenses rates to be directly affected by the withdrawal of U.S. military forces due to their disproportionately high share of foreign servicemen in total suspects. On the other hand, property crimes are commonly thought of to respond more strongly to economic incentives. For these offenses, the withdrawal of U.S. Forces therefore might not only work directly through the mechanical reduction in the local pool of potential offenders, but could also lead to a rebound in rates of these offenses in the medium and long run from the emerging adverse economic effects, such as the rise in local unemployment. Hence, the overall effect of the closure of U.S. military bases on the local property crime rate might be more ambiguous.

Table 4.5: Effect of U.S. Forces withdrawal on subcategories of crime

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable: (log rate)	Drug offenses	Sex offenses	Rape	Theft (total)	Robbery & violent theft	Motor cars theft
U.S. withdrawal	-.190***	-.011	-.191***	.005	-.013	-.013
	(.064)	(.033)	(.047)	(.019)	(.038)	(.038)
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N (district x year)	2,812	2,812	2,812	2,812	2,812	2,812
R^2	.977	.980	.980	.986	.930	.924

Notes: *Significant at 10%, ** at 5%, *** at 1%; Robust standard errors clustered at district level in parentheses.

To further investigate this relationship, table 4.5 reports the results for the crime rates of detailed offenses from analogous estimations of the baseline specification. For drug offenses, the coefficient estimate in column (1) suggests that the withdrawal decreased the offenses rate in the treatment districts by 19 percent. This effect clearly seems to support the narrative evidence that districts with a U.S. presence previously suffered from an inflated number of drug offenses relative to the resident non-military population and that this burden to the host regions of the U.S. bases was subsequently reduced by the drawdown. To put the size of this estimate into perspective, we note that it is a little less than half of the 45 percent raw difference in the drug offenses rate between the treatment and control districts that we recorded for the pre-withdrawal period 1984-1989 in table 6.6.

Column (2) provides the equivalent estimate on the total crime rate of sex offenses. Again, the coefficient has the expected negative sign, but is small and insignificant. In column (3), we analyze the relationship for the outcome of rape. Here, the estimate again is highly significant, pointing towards a 19 percent decrease in the crime rate of rapes in the treatment districts after the U.S. withdrawal. However, as already mentioned in section 4.4, our model based on the log rate might be biased if the underlying count of offenses is

rather small and exhibits a lot of zeros. We therefore postpone the further discussion of this category to the description of our robustness checks where we also consider alternative estimation models.[17]

Column (4) reports the results for the total theft rate, and column (5) and (6) for its subcategories of robbery and violent theft as well as the theft of motor cars. All estimates on these property crimes are small and suggest no significant impact of the U.S. withdrawal.

Table 4.6: Dynamic pattern of effects of U.S. Forces withdrawal on crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. variable: (log rate)	Total crime	Drug offenses	Sex offenses	Rape	Theft (total)	Robbery & violent theft	Motor cars theft
Year relative to U.S. withdrawal							
t-5	-.002	.024	.018	-.067	-.003	-.010	.024
	(.012)	(.047)	(.045)	(.084)	(.016)	(.041)	(.039)
t-4	.020	-.038	.050	-.033	.019	-.031	.062
	(.015)	(.059)	(.044)	(.080)	(.020)	(.050)	(.047)
t-3	.014	-.090	.035	-.070	.025	-.001	.073
	(.017)	(.073)	(.048)	(.085)	(.023)	(.049)	(.046)
t-2	-.004	-.084	.006	-.079	-.002	-.030	.042
	(.019)	(.080)	(.052)	(.095)	(.027)	(.057)	(.051)
t-1	.011	-.176**	.074	.037	.024	.024	.071
	(.020)	(.088)	(.055)	(.088)	(.029)	(.047)	(.052)
t0	.011	-0.135	.099	-.104	.021	-.039	.041
	(.022)	(.087)	(.061)	(.096)	(.031)	(.051)	(.056)
t+1	.002	-.185**	0.108*	-.181**	.032	-.035	.061
	(.023)	(.088)	(.060)	(.088)	(.029)	(.052)	(.056)
t+2	-.017	-.230**	-0.027	-.251**	.013	-.014	.075
	(.027)	(.096)	(.051)	(.100)	(.030)	(.056)	(.061)
t+3	-.027	-.290***	-0.023	-.297***	.015	-.003	.031
	(.027)	(.102)	(.056)	(.094)	(.031)	(.055)	(.062)
t+4	-.035	-.258**	-0.057	-.265**	.011	-.015	.019
	(.026)	(.103)	(.057)	(.108)	(.032)	(.058)	(.060)
t+5	-.019	-.214**	0.016	-.207**	.016	.010	.004
	(.027)	(.104)	(.060)	(.095)	(.031)	(.061)	(.064)
t+6	-.011	-.289***	-0.001	-.204**	.012	-.003	.025
	(.028)	(.100)	(.057)	(.083)	(.033)	(.059)	(.071)
t+7	-.004	-.279**	-0.027	-.260***	.030	.052	.014
	(.030)	(.113)	(.059)	(.093)	(.036)	(.062)	(.071)
t+8 & more	-.009	-.339***	0.005	-.249***	.002	-.039	.026
	(.029)	(.110)	(.059)	(.084)	(.031)	(.057)	(.073)
Other covariates:							
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (district x year)	2,812	2,812	2,812	2,812	2,812	2,812	2,812
R^2	.942	.849	.725	.491	.962	.871	.823

Notes: *Significant at 10%, ** at 5%, *** at 1%; Robust standard errors clustered at district level in parentheses.

[17] See section 4.5.4.

4.5.3 Dynamic pattern

A limitation of the static DD framework is that the estimated treatment effect provides only an average response of the outcome variable to the exogenous shock over the entire post-period. In this section, we therefore exploit the fact that within the set of treatment districts, the base closures did not occur simultaneously at one specific date. In our sample, some districts were immediately affected in 1990, whereas others only experienced the shock of base closures some of the later years until 1994. This variation in the timing allows us to break down and identify the response to the U.S. withdrawal in the treatment districts for each time period relative to the respective last pre-withdrawal base year, while still allowing for district and year fixed effects in the estimation.

Table 4.6 reports the results from these estimations for the total crime rate and the same subcategories of crime as before. The picture that emerges seems consistent with our previous results, with significant negative and permanent effects for drug offenses and rape in the post-period and insignificant effects for the total crime rate and the property crimes. The general pattern of effects also broadly supports the validity of our DD design, with significant differences between treatment and control districts only emerging in the post-period after t0.[18] In theory, the set-up should also enable us to shed some light on the question whether the mechanical impact of the U.S. withdrawal on local crime (from the reduction in U.S. servicemen) might be partly counteracted over the medium run by relative increases in crime that could be spurred by the decline in economic opportunities in the affected regions, leading to higher incidents of crime within the local native population. However, our results do not provide any conclusive evidence on this question. Although the coefficients on total crime rate seem to follow a mild inverse U-shape pattern (with decreasing values up to period t+4, and some reversion towards zero afterwards), they are not measured precisely and none of them is significantly different from zero.

4.5.4 Robustness checks

Thus far, the estimated results suggest that the withdrawal of U.S. military forces has led to a permanent relative decline in the rate of drug offenses and potentially (with the caveat noted above), the incidents of rape within the treatment districts. To check the robustness of these findings, we report in this section results from some further modifications of the model. In row 1 of table 4.7 we first reproduce for convenience the coefficient estimates from our baseline regressions for the total crime rate and the subcategories from tables 4.5.1 and 4.5.

In row 2 and 3 we consider two alternative variations in the sample. As discussed above, the anecdotal reports about the concentration of crime incidences in regions with U.S bases suggests that the propensity to engage or become involved in criminal activities might differ

[18] The only deviation from this pattern is the negative significant coefficient estimate at t-1 for the drug offenses rate in column (2). However, this estimate might be in part an artifact of our timing convention as explained in footnote 9 in section 4.3.

between U.S. Army and U.S. Air Force servicemen. Among other factors, members of the U.S. Air Force in Germany have tended to be more senior, more likely to be married, and more educated than their U.S. Army comrades. Hence, regions where the U.S. presence was mainly composed of U.S. Air Force bases could have experienced less inflated crime rates than other districts during the time of the U.S. presence, and hence also the drop in crime rates after their withdrawal could have turned out to be less substantial. Unfortunately, in our available sample for the three federal states, the withdrawal of U.S. Air Force soldiers affects only two districts so that the estimation of a separate coefficient for these districts is not reliable. Excluding the two districts with U.S. Air Force presence from the sample yields the coefficient estimates reported in row 2 in the table. All point estimates only slightly differ in magnitude and significance from the baseline coefficients.

Row 3 depicts the results once we exclude all border districts from the set of treatment and control districts in our baseline sample. The idea behind this sample variation is intuitive: with the fall of the Iron Curtain in 1989 as well as the stepwise abolishment of border controls between EU member countries based on the implementation of the Schengen agreement after 1995, border regions might suffer from higher and increasing crime rates from cross border criminal activities of foreign offenders.[19] This could potentially bias our DD estimates downwards as it introduces district-time variation in our crime outcomes and border regions make up for a larger share within our set of control than within our set of treatment districts.[20] Indeed, the point estimates in this restricted sample for the effect of the U.S. withdrawal on the total crime rate, the sex offenses rate and the theft rates become more negative, but remain insignificant, while the effects for drug offenses and rape change very little.

In row 4, we show the results when we reestimate our model with a Prais-Winsten estimator that more explicitly allows for strong serial correlation of the crime outcomes by assuming a district-specific AR(1) process.[21] This change seems to lower the estimate of the drug offenses rate, but it remains significant at the 5 percent level, and the qualitative nature of the results remains the same.

An alternative estimation method is also applied to produce the results reported in row 5. As already suggested in section 4.5.2, our OLS estimates might suffer from a bias if they are based on very small counts with a non-negligible number of zeros for the detailed subcategories of crime. In particular, this bias could potentially be at the source of the large coefficient estimates for the U.S. withdrawal effect on the rape rate and also affect the detailed theft categories. We address this issue by reestimating our model with a negative binomial (NB) regression model, a standard estimator of choice in the analysis of small

[19] Indeed, figure 4.2 provides some graphical evidence that border districts in southwestern Germany rank relatively high in total crime rates already in 1990.

[20] About 15 percent (or 10 out of 66) of our treatment districts in the baseline sample are border districts, compared to 40 percent (33 out of 82) of the control districts. Cf. table 6.6.

[21] This estimator seems to be often used in the crime literature if crime outcomes are measured at relatively high frequency for small regional units. See, e.g., Adda et al. (2011) for an application of this estimator on monthly time series of drug offenses in their analysis of a policing experiment in a London borough.

count data, including criminal incidents.[22] The results from the NB regression suggest that the OLS estimate on the rape rate is indeed biased upwards, but the new model still provides a highly significant estimate of -0.136, suggesting that the rape rate dropped by 13.6 percent after the withdrawal in the treatment districts.

In a final robustness check and extension, we explore whether the withdrawal of U.S. forces had heterogeneous effects in urban compared to rural districts. The area type of a district could influence the effect of the reduction and realignment of U.S. Forces on local crime rates. Potential sources of this type of heterogeneity that are frequently cited in the literature include the role of social interactions and co-offending that might create contagion effects in more densely populated areas. On the other hand, some types of crime might be generally deterred in more densely populated areas. From the economic point of view, the adverse effects of the U.S. withdrawal might be more severe for rural districts, and thereby contribute to an increase in relative crime rates in these regions in the long-run. To capture this variation, the model is augmented by an interaction term multiplying the U.S. withdrawal dummy variable with a dummy for the rural treatment districts. The estimation results for the respective offenses are presented in row 6 of table 4.7. For the overall crime rate, the main coefficient on the U.S. withdrawal dummy now is negative and significant at -0.044, while the interaction for the rural treatment districts has almost the exact same absolute value and is positive at the 10 percent level. Taken together, these results suggest that the U.S. withdrawal did cause a notable decline in total crime rates, but this effect is limited to the urban districts. The same pattern of heterogeneity now emerges also for sex offenses in columns (3) and (4), as well as for the coefficient estimates on the property crime rates in columns (5)-(7). Here, the positive coefficient estimates for the interacted U.S. withdrawal variable for rural districts even exceeds the absolute value of the negative significant estimates for urban districts that experienced a withdrawal, although the sum of the two is not significantly different from zero. The coefficient for the drug offenses rate that was consistently significant in all previous estimations now exhibits a different picture. Both estimates are negative, but not significant in separation. However, the test for joint significance produces an F-value of 4.68 that is significant at the 5 percent level. Hence, the results suggest that the reduction in drug offenses related to the U.S. withdrawal are stronger in rural than in urban treatment districts. While these results suggest an interesting direction for further research, the small number of observations used to discriminate the differential effects warrant some caution. Moreover, any in-depth analysis of this dimension would need to take the spatial interactions between neighboring districts into account which the ad-hoc classification into urban versus rural districts employed here does not account for.

[22] See, e.g., Cameron and Trivedi (1998) and the summary in Osgood (2000).

Table 4.7: Robustness checks for effects of U.S. withdrawal on crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. variable: (log rate)	Total crime	Drug offenses	Sex offenses	Rape	Theft (total)	Robbery & violent theft	Motor cars theft
1. Baseline: U.S. withdrawal	-.015	-.196***	-.001	-.183***	.005	.002	.008
	(.018)	(.063)	(.033)	(.047)	(.019)	(.038)	(.033)
2. Exclusion of treatment districts with U.S. Air Force personnel	-.015	-.190***	-.011	-.193***	.006	-.014	-.009
N=2,774; n(treatment districts)=64;	(.018)	(.064)	(.033)	(.047)	(.019)	(.038)	(.044)
3. Exclude border districts	-.028	-.193***	-.045	-.216***	-.010	-.040	-.046
N=1,995; n(treatment)=56; n(control)=49	(.020)	(.064)	(.038)	(.052)	(.023)	(.042)	(.044)
4. Use Prais-Winsten estimator with district-specific AR(1) error process	-.008	-0.094**	-.007	-.191***	.002	-.014	-.008
	(.013)	(.043)	(.032)	(.047)	(.014)	(.037)	(.036)
5. Use negative binomial regression model	-.015	-.195***	.005	-.136***	.005	-.012	-.020
	(.017)	(.061)	(.031)	(.036)	(.018)	(.031)	(.038)
6. Include interaction of U.S. withdrawal dummy with rural districts							
U.S. withdrawal	-.044**	-.092	-.111***	-.234***	-.047*	-.099**	-.159***
	(.022)	(.073)	(.039)	(.052)	(.025)	(.040)	(.045)
U.S. withdrawal x rural	.043*	-.146	.150***	.064	.078**	.129***	.225***
	(.026)	(.095)	(.048)	(.065)	(.030)	(.048)	(.057)
District, year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *Significant at 10%, ** at 5%, *** at 1%; Robust standard errors clustered at district level in parentheses.

4.6 Conclusion

In this paper, we analyze the causal impact of military base closures on crime rates by using the withdrawal and realignment of the U.S. Forces in Germany after the Cold War as a natural experiment. Our motivation is twofold: In the specific context of the U.S. presence in Germany, credible estimates for the impact of military bases on local crime rates could add an important piece of evidence with respect to the overall costs and benefits that U.S. military communities entailed for their respective local surroundings. In a broader sense, our study should also further the unterstanding of the determinants of regional crime rates and their evolution over time.

Our results suggest that the U.S. base closures led to a significant reduction for some subcategories of crime, but not for others. In particular, the rate of drug offenses exhibits a significant and substantial decline of close to 20 percent. This result supports the narrative evidence that districts with a U.S. presence suffered from an inflated number of drug offenses relative to the resident non-military population. Obviously, this burden to the host regions was reduced substantially by the drawdown.

We also find a negative and likewise highly significant effect for the specific outcome of rape. The size of the reduction for this type of offenses amounts to about 14 percent, but is based on a small number of counts. Taken together, these two partial results clearly confirm earlier evidence and historical accounts concerning the types of crime that are most likely connected with the presence of U.S. military bases. Notwithstanding, we detect no significant effects for other subcategories of crime and the aggregated total crime rate. Finally, we present evidence indicative of some spatial effect heterogeneity with the drop in crime rates being more pronounced in urban compared to rural districts.

5 Regional task intensity and the growth of temporary help services

5.1 Introduction

The growth of temporary help services (THS) is an important stylized trend in many industrialized countries. In Germany, THS has increased tremendously in recent decades (see, e.g., Rudolph and Schröder, 1997; Deutscher Bundestag, 2009). This evolution has sparked a growing academic as well as public interest in the implications of THS for job stability, wage inequality and labor market segmentation.[1] However, most authors argue that THS is still a minor part of overall employment and thus not relevant as a key explanation for the secular trends of employment and wage polarization. While this assessment seems to be appropriate for the national aggregate, the overall share masks an already much higher impact of THS employment in regional labor markets. For Germany as a whole, THS employment constituted a share of 2 percent of overall employment and 3 percent of dependent employment in 2008 (cf. Deutscher Bundestag, 2009; Jahn and Bentzen, 2010). In our data for 2008, some regions exhibit THS employment shares larger than 8 percent. In this paper, we adopt the task-based approach to study the heterogeneous pattern in regional growth of THS employment, and hence shed further light on the question if THS constitute a boon or bane for regional labor markets.

An important insight of the task-based framework is that the classification of employment by its task content (e.g. routine or non routine tasks) enables a deeper understanding of many trends that shape modern labor markets. Autor et al. (2003) first argued that especially routine tasks which are easily codified are substituted by computer capital.[2] The key idea is that these routine intensive occupations are in the middle of the wage distribution, and that the substitution by computer capital leads to a decline in the demand for those medium skilled occupations, while demand for high- and low-skilled labor increases. The task-based approach therefore predicts a polarization of employment and wages, a stylized fact that has been analyzed and documented for many industrialized countries (see Autor et al., 2006 for the U.S., Goos and Manning, 2007 for the UK, Dustmann et al., 2009 for Germany; Goos et al., 2009 present evidence of labor market polarization for European countries). Recent research by Autor and Dorn (2011) extends the analysis of the impact of nuanced skill-biased technical change on employment and wages to local labor markets.

[1] See, e.g., Nonnenmann (2011); Möller (2011).

[2] See Autor and Acemoglu (2011) for a comprehensive overview of the task literature and Weiss (2008) for a model of the substitution of human routine tasks by computer capital.

Regions that were initially specialized in routine intensive occupations saw a substantial decline in employment in these occupations, but a growth in low-skilled service sector employment which is in line with the idea of employment and wage polarization.

The concept that routine tasks are not only easily codified and computerized, but also easy to explain and easy to monitor, has inspired further research on the tradeability of tasks.[3] Grossman and Rossi-Hansberg (2008) first proposed a model of tradeable tasks and established a positive relationship between factor productivity and the decline in costs of trading the task, which makes the task easier to offshore. Using firm level data on U.S. multinationals, Oldenski (2011) confirms that routine intensive tasks are indeed more likely to be offshored. This finding is also supported by empirical evidence for Germany using industry-level data presented by Baumgarten et al. (2010).

Our research project links this task and "trade-in-tasks" literature to the existing studies of employment services. In their seminal paper for the US, Segal and Sullivan (1997) use CPS data to study the changing nature of temporary work and first documented a shift in the composition of workers in this sector away from office and administrative support occupations towards blue-collar occupations. Furthermore, they argue that labor market outcomes of temporary workers are worse: their job stability is reduced, they gain lower wages and have a higher probability of getting unemployed. Subsequently, most studies on THS employment have further analyzed the impact of THS employment from the perspective of the temp workers, in particular whether temp work provides a "stepping stone" out off unemployment into regular employment[4]

From the perspective of the firm recurring to THS, three key types of potential explanations for their THS use have been set forth in the literature (cf. Abraham, 1990; Abraham and Taylor, 1996; Segal and Sullivan, 1997; Houseman, 2001; Autor, 2001; Burda and Kvasnicka, 2006). First, the use of THS may allow firms to gain *flexibility* to adjust the extensive margin more easily in response to business cycle and/or seasonal fluctuations and also replace permanent employees in cases of (long-term) sickness. Second, firms might employ temps as a way to improve *screening* of prospective permanent employees. Third, the use of THS might provide firms with *cost savings* if the total cost to the employer is lower for a temp worker than for a permanent employee. The empirical evidence on the relative importance of these three motives is still relatively scarce. While the flexibility argument is regularly brought forward by representatives of THS firms and several studies have confirmed that THS employment is indeed procyclical, this argument alone does not seem to explain the secular rise in THS employment, at least in the absence of a

[3] However, it is not a priori clear which tasks are most likely to being offshored. Following Autor et al. (2003), Levy and Murnane (2004) have classified tasks into routine and non routine tasks. Blinder (2006) on the other hand argues that regional proximity and face-to-face contact are important determinants for the tradeability of tasks.

[4] See, among others, Autor and Houseman (2010) who exploit the unique structure of a job placement program in Detroit and find no evidence for the "stepping stone"-effect of temporary-help work, as placement in temporary help jobs does not improve labor market outcomes of low-skilled workers. For Germany, Kvasnicka and Werwatz (2003), Kvasnicka (2009), and Jahn and Rosholm (2010) analyze the transitions into and out of THS work and the associated wage gaps and reach similar conclusions.

commensurate trend towards more aggregate volatility.[5] The appeal of the screening motive seems to be limited to the small group of temp workers that possess higher and more specialized skills, as the empirical absence of a pervasive "stepping stone"-effect into permanent employment at least for the large majority of low qualified temp workers suggests. Due to the lack of representative micro data on the detailed personnel cost structure of employers, the empirical analysis of the cost saving motive relies either on employer surveys or on case studies on a small number of firms. Houseman (2001) reports evidence from a representative survey of U.S. companies conducted in 1996: while only a small share of employers cited cost-savings when asked directly about their motives for the use of temps, a large majority of them still indicated that the total hourly billed rate for temp workers would be equal or lower relative to the hourly compensation including benefits for their comparable permanent employees. Houseman et al. (2003) analyze and compare the use of temp workers in a sample of six hospitals and five auto suppliers. Their analysis of the relative cost of temp workers reveals a split pattern: while the bill rate for high-skilled specialized temps is higher than the average total employer costs of regular employees, the use of temps for low-qualified jobs is associated with cost savings of 10 to 30 percent. For Germany, Oberst et al. (2007) provide a rare inside perspective on the use of temp workers by a service sector company and estimate a cost saving of 29 percent per hour of work for the use of temp workers relative to permanent employees with similar characteristics.[6]

While the lack of data precludes the opportunity of establishing the exact magnitude of potential cost savings for a more representative set of firms that use THS, several studies have pointed to institutional factors that could have made THS use look more attractive to firms in search of opportunities to lower their wage bill. Autor (2003) establishes a causal link between the presence of restrictive employment protection legislation associated with high firing costs to employers and the growth of THS employment over time and across U.S. states. Houseman (2001) points to the level of unionization, leading to permanent workers enjoying rents from non-competitive wages, as a potential explanation for the prevalence of THS use in manufacturing. Dey et al. (2006) find some empirical support for this claim by constructing a unique panel data set on employment by occupation by industry for the years 1989 to 2004, and showing that US manufacturers increasingly use employment services to outsource low-skilled manual and clerical occupations.[7]

Against the backdrop of these strands of the literature, our study proposes a fresh look at the demand-driven explanation of the growth of THS employment by incorporating

[5] Note that the time periods under consideration in almost all existing studies on THS still exclude the current Great Recession that might constitute a turning point towards an era of increased volatility and uncertainty.

[6] Mitlacher (2007) also compares the use of temp work in the US and in Germany and reports cost savings in the German automotive industry of 8.55 EUR per hour and of 10-15 percent per hour in the catering industry for unskilled temp workers.

[7] Based on a smaller sample of U.S. manufacturing companies in Wisconsin, Vidal and Tigges (2009) also find evidence of a "systematic use" of temp workers, which they characterize as a trend towards "permanently tempting out entire positions".

the data and methodology of the task and trade-in-task literature into the analysis of the evolution of THS employment. Our research questions are twofold: first, do firms outsource primarily manual tasks into THS employment, as predicted by the task framework? Second, does THS employment hence grow faster in local labor markets that are initially more intensive in manual labor? Our results indicate that the share of THS employment indeed rises particularly strongly for occupations that were initially intensive in manual tasks, but not for those intensive in non routine interactive or routine cognitive tasks. Taken to the regional level, the initial routine manual share of a region is a strong predictor of the subsequent regional growth in THS employment: a 1 percentage point higher routine manual share in a region in 1979 is associated with an increase in the growth rate of THS employment between 1979 and 2008 by 50 percentage points. This result is robust to the inclusion of a number of regional covariates and alternative levels of regional aggregation. Our paper proceeds as follows: the next section spells out our empirical approach and describes the datasets that we use. Section 5.3 reports the results from the descriptive analysis and the regression estimates. Section 5.4 summarizes our conclusions and details avenues for further research.

5.2 Methods and data

5.2.1 Empirical approach and estimation strategy

Starting point of our analysis is the observation that occupations differ in their skill requirements and task structure (Autor et al., 2003; Spitz-Oener, 2006). Taken these differences as given, occupations witness a differential growth of THS provision depending on their initial task structure. However, it is not a priori clear which task are more likely of being outsourced into THS employment. The offshoring and trade-in-tasks literature (Blinder, 2006; Grossman and Rossi-Hansberg, 2008) hypothesizes that firms can realize cost savings by relocating impersonal/routine tasks into countries with a wage advantage. However, the degree of offshorability is limited by the amount of labor that requires regional proximity or the direct integration into the production process at the firm's own plant (including the use of firm-specific assets). Following this line of reasoning, outsourcing into THS employment would provide employers with an additional mechanism to lower their labor costs for these tasks or in cases when offshoring is not feasible due to other strategic considerations.[8] In line with the observation at the occupational level, regional differences in industry specialization and therefore the task structure should have differential effects on regional THS employment growth. In our econometric analyses, we therefore test two closely related predictions: (1) firms primarily outsource manual tasks into THS employment and (2) THS employment grows faster in manual intensive labor markets.

In order to analyze the relationship between the occupational task intensity in 1979 and

[8]This argument has also been put forward in recent studies on re-shoring (Baldwin and Venables, 2011).

subsequent growth in THS penetration, we set up an empirical model of the following form:

$$\Delta(THSshare_{k,1979-2008}) = \alpha + \beta_1 \times TI^{j}_{k1979} + \epsilon_k \tag{5.1}$$

where $\Delta(THSshare_{k,1979-2008})$ is the change in the share of occupational employment provided by THS between 1979 and 2008, j indexes the task categories and k the detailed occupations. TI^{j}_{k1979} measures the fraction of occupational employment allocated to the respective task in 1979 and the associated coefficient β_1 reveals which tasks are most prone to substitution by THS employment. Estimates are weighted by the average fraction of national employment in each occupation between 1979 and 2008.

In our main analysis at the regional level, we regress the log difference of regional THS employment for a given region and period on the initial task share according to the following specification:[9]

$$\Delta(Y_{rt,t+1}) = \alpha + \beta_1 \times TSH^{j}_{r1979} + \beta_2 \times X_{r1979} + \epsilon_r. \tag{5.2}$$

Our main parameter of interest, β_1, is the coefficient on the measure of the initial task share in 1979, TSH^{j}_{r1979}. The vector X_{rt} includes additional variables aimed at controlling for heterogeneity in the regional distributions of employment. All regressions use robust standard errors and are weighted by start of the period regional total population.[10]

5.2.2 Data and construction of variables

Our measure of regional task intensity as the main explanatory variable is constructed from the combination of two datasets, the Sample of Integrated Labor Market Biographies (SIAB) and the BIBB/IAB survey. The information on the task content of occupations comes from the Qualification and Career Survey, which is an employment survey carried out by the German Federal Institute for Vocational Training (*Bundesinstitut für Berufsbildung; BIBB*) and the Research Institute of the Federal Employment Service (*Institut für Arbeitsmarkt- und Berufsforschung; IAB*). It consists of five cross-sections in the years 1979, 1985, 1992, 1998 and 2006, each covering about 30.000 individuals, including men and women (see Spitz-Oener, 2006). The dataset contains information on workplace characteristics and educational attainment and is particularly well suited for our research, as it includes detailed information on the activities individuals perform at the workplace and on the tools and machines employees use at work. These activities are pooled into five task groups.

[9] Autor and Dorn (2011) employ stacked first differences over three time periods to estimate the relationship between regional routine intensity and the growth of non-college service employment. In contrast, we restrict our analysis to the single difference based on the routine shares and regional covariates in 1979 as the explanatory variable to focus on the long-run component of differences in regional task structures and thus circumvent the potential endogeneity problem related to the use of subsequent task shares. For ease of interpretation and comparison across periods, the outcome variables are adjusted for the length of the different time periods and represent 10 x annual changes.

[10] To correct for outliers, we exclude the regions "Pirmasens" and "Garmisch-Partenkirchen" from our regressions as their manual task shares in 1979 are more than one standard deviation larger compared to the second largest regional task share. Figure 5.4 provides a graphical illustration.

In the assignment of tasks, we follow Spitz-Oener (2006) and construct individual task measures TM^j_{i1979} for task j according to the definition of Antonczyk et al. (2009):

$$TM^j_{i1979} = \frac{\text{number of activities in category } j \text{ performed by } i \text{ in 1979}}{\text{total number of activities performed by } i \text{ over all categories in 1979}} * 100, \qquad (5.3)$$

where j = A, I, RC, RM, M represents the five task groups analytic (A), interactive (I), routine cognitive (RC), routine manual (RM) and non-routine manual (M).[11] The individual task measures are aggregated to construct average task indices for each occupation k, where L_{ik1979} is the number of individuals working in occupation k in 1979:

$$TI^j_{k1979} = \frac{\sum_i TM^j_{ik1979}}{\sum_i L_{ik1979}}. \qquad (5.4)$$

TI^j_{k1979}, the task intensity of occupation k in 1979 is matched to the SIAB employment sample, which is a two percent random sample of administrative social security records in Germany covering the years 1975 to 2008. The sample consists of about 200.000 employment spells per year and provides detailed information on employment spells for dependent employees who contribute to the social security system (civil servants and self-employed workers are not included). The data set also contains information on the individuals' age, gender, educational attainment as well as information on the employer such as industry affiliation, firm size and location. The occupational titles used in the two datasets are categorized according to the 1988 classification constantly throughout time and the datasets are therefore well suited to analyze the development of skill requirements within occupations.

Furthermore, the SIAB provides a time-consistent definition of administrative districts in Germany, which can be used to construct the regional task shares. Since administrative regions in Germany have developed as a result of historical circumstances they do not necessarily depict regional economic entities (Eckey et al., 2006). For the analysis of regional employment changes functional labor markets that exhibit few commuter flows are more suited. A delineation by Eckey et al. (2006) is particularly adequate as it derives labor market regions that consist of one or more administrative districts across state borders and take commuter flows into account. This feature is particularly relevant for our analysis as it limits the potential measurement error if employees of THS firms located in one district are leased to companies in other districts. Following Eckey et al. (2006), we therefore further aggregate the administrative districts into 150 labor market regions (with 113 in West and 37 in East Germany). For each region r the task share TSH^j_{r1979} is given as:

[11] The sample to construct the individual indices includes West German employees aged 20-60, excluding public sector and agricultural employment.

$$TSH^{j}_{r1979} = \frac{\sum_{k=1}^{K} L_{kr1979} * TI^{j}_{k1979}}{\sum_{k=1}^{K} L_{kr1979}}, \tag{5.5}$$

where j indexes the respective task and L_{rk1979} is the employment in occupation k in labor market r in 1979. For example, TSH^{RM}_{r1979} thus represent the share of routine manual labor in total employment on the regional level.[12]

As for our main outcome variable, regional THS employment, we use data for the time period 1975-2008 from the Establishment History Panel (BHP) provided by the German Institute for Employment Research (IAB) (see Hethey-Maier and Seth (2010) for details and e.g., Dauth (2010) for the use of the BHP in an analysis of employment in the context of regional externalities). The BHP data is a cross-section of the complete universe of all establishments that employ at least one employee subject to social security contributions on June 30th of a given year. Based on the mandatory individual social security notifications, the BHP provides for each establishment information on the total number of employees as well as several breakdowns, e.g. the number of employees by gender, age, citizenship, education, and working hour categories. In addition, it details the district (NUTS-3 region) where the establishment is located, the year the establishment was founded (i.e. the starting year of the first employment contract subject to social security contributions) and the detailed 5-digit industry classification.

The BHP data is particularly adequate for the study of the THS employment at the regional level. As it covers the total employment at the granular establishment level, it provides a more complete picture of the regional importance of THS employment than survey data with sampling error or data based on firm statistics. It is also highly accurate due to its official use for the social security administration. However, these advantages come at three types of cost. First, the aggregate nature of the data precludes the use of information at the level of the individual employee in the analysis. Second, the BHP data does not entail additional information at the establishment level on productivity, type of assets etc. Third, the employment information is restricted to the number of full-time employees, as the number of part-time employees is only incompletely recorded until a change in the social security legislation in 1999. However, as part-time work is only a minor share of overall THS employment[13], the last caveat is only of minor importance for our analysis.

In constructing our panel of the regional level of THS employment, we proceed similar to Kvasnicka and Werwatz (2003) to identify all THS establishments via their registration with the industry code exclusively dedicated to THS.[14] One drawback of this approach is

[12] Our measure of regional task intensity improves on that in Autor and Dorn (2011) by exploiting the task intensity of all occupations. Hence, we measure the share of all regional routine employment instead of using only the top third most routine intensive occupations. See Senftleben and Wielandt (2012) for further details on the construction of the index.

[13] See, e.g., Rudolph and Schröder (1997), who find that the shares of part-time workers in 1995 are only 0.3 percent (5 percent) for males (females), compared to 2 percent (27 percent) in overall employment.

[14] For the industry classification 1973, this industry category is labeled at the 3-digit-level with the code 865

that the THS workers that are leased to other companies cannot be easily differentiated from the permanent workforce of the THS firm, i.e. the managerial and administrative staff that organizes the temporary work mission. Again, we rely on Kvasnicka and Werwatz who cite additional evidence that the permanent workforce of the THS firm only constitutes a minor part (less than 8 percent) of the total staff of the THS establishments.[15]

We aggregate the THS employment data from the establishment level to the level of the 113 labor markets and merge them with the measures of the regional routine share in 1979. We further augmented our panel of region-by-year observations by aggregating information from the BHP on the overall composition of the regional employment distribution with regard to the fraction of female employees, the share of young employees (defined as being less than 25 years old), the share of high and low skilled employees, the share of foreign employees and the distribution of employment by three firm size categories.[16]

5.3 Results

5.3.1 Occupational THS growth and job tasks

Temp-workers are mainly employed as laborers, in low-skilled blue-collar or administrative support occupations (Jahn and Pozzoli, 2011; Dey et al., 2006). Figure 5.1 depicts aggregate trends in the growth of occupational employment provided by THS between 1979 and 2008 for 6 broad occupational groups.[17] THS penetration increased over time for all occupational groups, accelerated particularly for manual occupations in the mid 1990's. Unskilled manual occupations (e.g. miners, rockbreakers, welders, unskilled workers) witnessed the largest growth in THS penetration as well as to a smaller extent skilled manual occupations (e.g. glassblowers, bookbinders, precision instrument makers, carpenters). In contrast, the increase in THS employment between 1979 and 2008 in high-skilled occupations such as professionals, managers and engineers is minuscule in size.

For the same 6 occupational groups, Table 5.1 displays aggregate skill inputs in 1979. In line with the task-literature, manual and service occupations are characterized by a high routine and non routine manual task content while interactive and analytic tasks are more

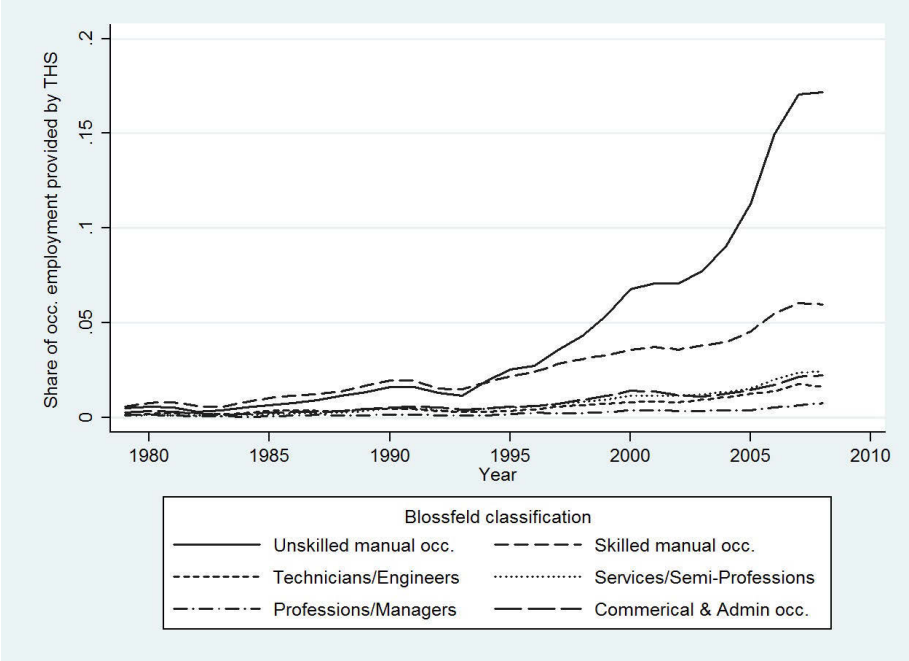
"Arbeitnehmerüberlassung/Leiharbeitskräfte", and for the industry classification 2003, the respective code is 74.50.2 "Überlassung von Arbeitskräften". Kvasnicka and Werwatz provide supplementary evidence that the establishments in this industry category make up for the large majority of THS employment in Germany.

[15] In a robustness check, we also subtracted at the THS establishment level the number of employed managers from the total full-time employment, as we assume most of these high-skilled employees to be *permanent* staff since the market for the temporary lease of managers is miniscule in size. Our preliminary results are not sensitive to this change.

[16] We also considered the share of employment concentrated in manufacturing as potential covariate. However, the manufacturing share is highly correlated with our measures of regional routine intensity and therefore likely a "bad control".

[17] In an effort to build homogeneous occupational groups with respect to their educational requirements and occupational assignments, occupations are aggregated into 6 major occupational groups following Blossfeld (1985). Occupations are classified according to the industrial sector (production, service, administration) and further subdivided by qualification.

Figure 5.1: Share of occupational employment provided by THS, 1979-2008



Source: IAB-BIBB data; own calculations. See text for details.
Notes: Occupations are aggregated into 6 major occupational groups following Blossfeld (1985).

prevalent in high-skilled occupations.[18]

Table 5.1: Aggregate skill inputs in 1979 by occupational group

Occupation	non-routine analytic	non-routine interactive	routine cognitive	routine manual	non-routine manual
Unskilled manual occupations	0.019	0.060	0.093	0.544	0.285
	(0.110)	(0.195)	(0.245)	(0.450)	(0.407)
Skilled manual occupations	0.045	0.107	0.075	0.312	0.460
	(0.161)	(0.237)	(0.195)	(0.398)	(0.431)
Services/Semi-Professions	0.027	0.197	0.101	0.160	0.515
	(0.118)	(0.320)	(0.221)	(0.297)	(0.433)
Commercial & Admin occupations	0.025	0.254	0.346	0.354	0.022
	(0.106)	(0.322)	(0.310)	(0.299)	(0.111)
Technicians/Engineers	0.320	0.250	0.157	0.166	0.106
	(0.370)	(0.303)	(0.245)	(0.291)	(0.229)
Managers/Professions	0.102	0.459	0.255	0.122	0.062
	(0.205)	(0.336)	(0.275)	(0.197)	(0.181)

Notes: Occupational task information on the three-digit level of the classification of occupational titles 1988 is derived from BIBB/IAB data. Occupations are then further aggregated into 6 major occupational groups following Blossfeld (1985). Sample includes West German workers aged 20-60 excluding agricultural and public sector employment.

We now further explore analytically which tasks are most prone to substitution by THS

[18]Looking at the data from a different angle, Table 6.7 in appendix 6.5 lists the ten three-digit occupations with the highest share of THS employment in 2008 and their task content in 1979. Consistent with previous evidence, those occupations are characterized by a high routine and non routine manual task content.

employing the empirical strategy described by equation 5.1. Table 5.2 summarizes the results for the five task measures. The negative albeit insignificant coefficient in column 1 suggests that occupations with a high non routine analytic task content in 1979 witnessed a slower growth of THS employment between 1979 and 2008. This negative relationship holds also true for occupations with a high non routine interactive task content as indicated by the negative an highly significant coefficient in column 2. As those tasks in our data are mainly performed by high-skilled employees, this finding seems to square with the fact that the large majority of temp workers is employed in rather low qualified occupations. While routine cognitive tasks seem to be less prone to substitution by THS employment as well (column 3), a high routine manual task intensity predicts a much faster growth in occupational THS penetration (column 4). The positive and highly significant coefficient suggests that occupations with a high routine manual share in 1979 indeed witnessed a stronger growth in THS employment between 1979 and 2008. Based on the coefficient estimate of 8.574 a one standard deviation higher routine manual task content in 1979 is associated with a 200 percent larger growth in the occupational THS share. In other words, occupations with a one standard higher routine manual share witnessed an increase in THS penetration twice as large compared to the mean occupational THS growth of 225 percent between 1979 and 2008. The coefficient estimate in column 5 predicts a positive but insignificant relationship between non routine manual task intensity and subsequent occupational THS penetration.

Table 5.2: Task content and growth of THS share by occupation 1979-2008

		Dependent variable: 100 x Δ share of occupational employment provided by THS				
Task share	Means (SD)	(1)	(2)	(3)	(4)	(5)
Non routine Analytic	0.090	-6.945				
	(0.142)	(4.843)				
Non routine Interactive	0.198		-10.570***			
	(0.175)		(3.124)			
Routine Cognitive	0.150			-6.692*		
	(0.126)			(3.507)		
Routine Manual	0.305				8.574***	
	(0.225)				(2.564)	
Non routine manual	0.258					2.194
	(0.252)					(1.879)
R^2		0.011	0.059	0.019	0.057	0.007

Notes: N=186 occupations. Occupations are defined according to the three-digit level of the classification of occupational titles 1988. Each cell reports the results from a separate OLS regression and its standard errors in parentheses. Estimates are weighted by the average fraction of national employment in each occupation over the years 1979-2008.

To summarize, occupations that are most likely to being outsourced into THS employment are those characterized by a high routine and non routine manual task content which are mainly low-skilled blue collar occupations. In contrast, analytic and interactive tasks offer less potential for substitution by THS. We therefore expect a positive relationship between

the initial share of manual employment in a region and subsequent THS employment growth and will focus on the relationship in the remainder of our analysis.

5.3.2 Regional distribution of manual tasks and THS employment

We start our empirical description at the regional level by looking at the top 10 regions with the highest manual shares of employment in 1979. Table 5.3 lists these regions, with a separate ranking for the routine manual and the non routine manual share. The two rankings reveal distinct regional distributions: while the top regions in terms of routine manual share in 1979 correspond to industrial strongholds across Germany, the non routine manual share is highest in regions that are characterized by tourism and hospitality.[19]

Table 5.3: Top 10 regions with highest manual shares in 1979

Panel A: Regional ranking in 1979

Region	State †	Routine man.	Region	State †	Non routine man.
Hof	BY	0.3842	Emden	NDS	0.3048
Dingolfing	BY	0.3727	Flensburg	SH	0.3050
Waldshut	BW	0.3636	Waldeck	HE	0.3054
Märkisch	NRW	0.3621	Ansbach	BY	0.3073
Tuttlingen	BW	0.3603	Heidenheim	BW	0.3112
Wolfsburg	NDS	0.3588	Traunstein	BY	0.3120
Lörrach	BW	0.3572	Landshut	BY	0.3166
Deggendorf	BY	0.3558	Cham	BY	0.3172
Rottweil	BW	0.3504	Nordfriesland	SH	0.3180
Coburg	BY	0.3494	Passau	BY	0.3228
Zollernalb	BW	0.3483	Straubing	BY	0.3302

Panel B: Descriptive statistics, manual shares 1979

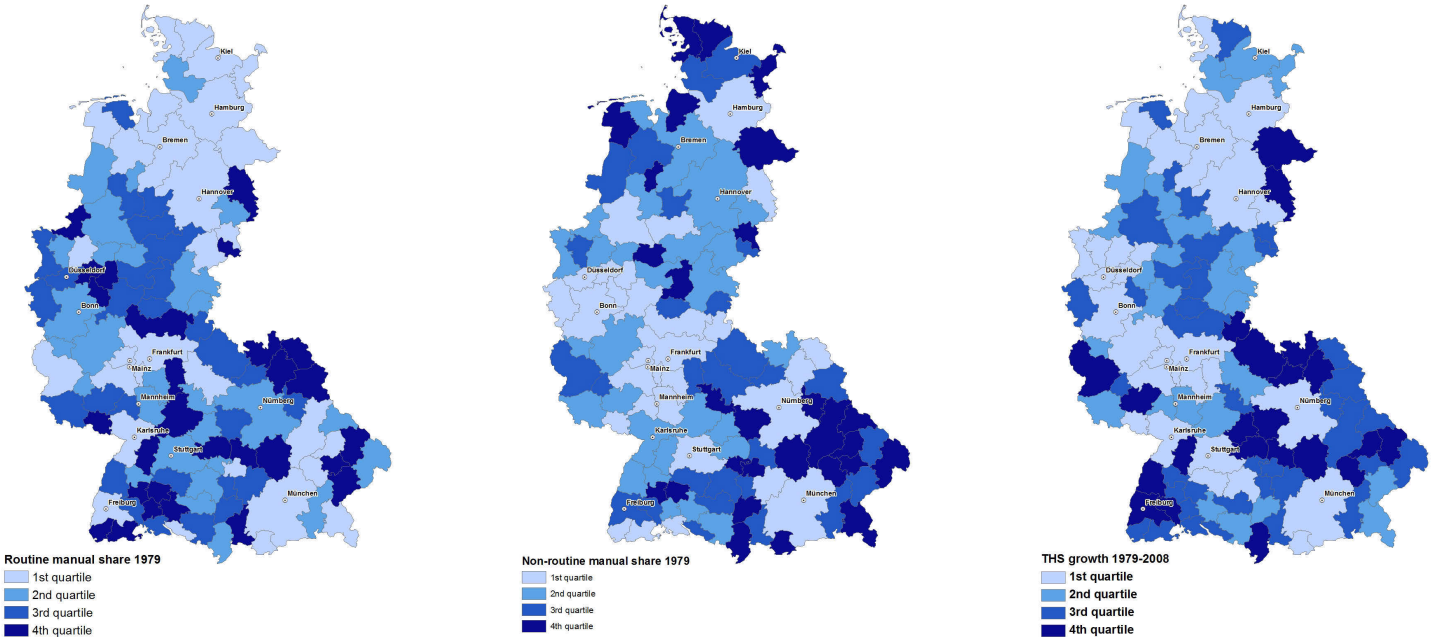
Mean	0.3224	Mean	0.2736
Std. Deviation	0.0204	Std. Deviation	0.0221

Notes: N=111. †The federal states are: Baden-Württemberg (BW), Bavaria (BY), Hessen (HE), Lower-Saxony (NDS), North Rhine-Westphalia (NRW), Schleswig-Holstein (SH).

Figure 5.2 further illustrates the geographic distributions by mapping the two manual shares in 1979. While a small number of regions do figure in the upper quartiles of both the routine manual as well as the non routine manual share distribution, the overall correlation between the two manual shares is negative, with a correlation coefficient of -0.2432.

[19] These regions are mainly located at the coast of the North and Baltic Sea, in the Bavarian Forest, and the Alps.

Figure 5.2: Routine task share and THS growth in West Germany, 1979-2008
left: Routine manual task share *center:* Non routine manual task share *right:* THS employment growth by region, 1979-2008



Source: IAB-BIBB data; own calculations. See text for details.

Table 5.4: Descriptive statistics for regions in West Germany, 1979-2008

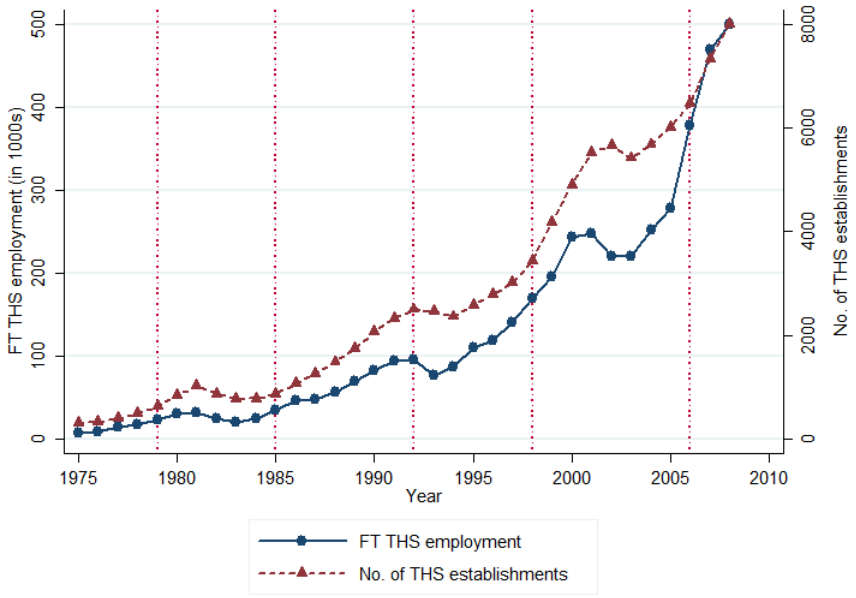
	1979	1994	2002	2008
Full-time THS employment	202	780	1,986	4,491
	(536)	(1,398)	(2,852)	(5,540)
Full-time employment	148,692	152,520	147,654	143,999
	(190,734)	(191,655)	(188,467)	(182,643)
Fraction of THS in total employment	0.0006	0.0035	0.0116	0.0303
	(0.0009)	(0.0029)	(0.0067)	(0.0141)
Number of THS establishments	6	21	51	72
	(13)	(36)	(75)	(95)
Fraction employed/pop.	0.262	0.255	0.242	0.238
	(0.048)	(0.053)	(0.050)	(0.049)
Fraction female employees	0.330	0.330	0.329	0.320
	(0.034)	(0.026)	(0.027)	(0.029)
Fraction young (< 25) employees	0.149	0.112	0.086	0.082
	(0.017)	(0.018)	(0.015)	(0.016)
Fraction high to low skill employees	0.032	0.062	0.086	0.109
	(0.015)	(0.028)	(0.040)	(0.050)
Fraction manufacturing employment	0.120	0.102	0.093	0.089
	(0.040)	(0.031)	(0.033)	(0.034)
Fraction foreign employees	0.084	0.083	0.070	0.066
	(0.044)	(0.036)	(0.032)	(0.030)
Fraction small firms (<25 employees)	0.319	0.348	0.358	0.345
	(0.061)	(0.055)	(0.055)	(0.054)
Fraction medium firms (25-100 employees)	0.211	0.218	0.224	0.227
	(0.036)	(0.028)	(0.029)	(0.029)
Fraction large firms (>100 employees)	0.471	0.433	0.418	0.429
	(0.091)	(0.075)	(0.077)	(0.075)
Average unemployment rate[a]	-	0.086	0.081	0.064
	-	(0.022)	(0.023)	(0.024)
Average region population	539,585	558,864	568,901	570,161
	(625,945)	(608,124)	(618,137)	(626,555)
Population density	245	260	263	262
	(260)	(258)	(255)	(250)
Number of regions	111	111	111	111
Number of regions with THS employment ≥1	61	99	111	111

Notes: [a]The regional unemployment rate is only available from 1985 on. All employment variables are based upon full-time employment subject to social security contributions for a given region. Fractions are computed with respect to total full-time employment.

Table 5.4 summarizes the main characteristics of employment by region for the time period under consideration. The first row shows that full-time employment in THS was at very low levels until the beginning of the 1980's, with only 200 THS employees on average in a given region, and has started to grow especially since the mid 1990's. A similar development can be observed for the fraction of THS in total regional employment and the number of THS establishments, alike. While in 1979 still around 50 percent of the regions did not exhibit any THS employment, all regions had at least some THS employment in 2008. The tremendous growth in THS employment is most visible in Figure 5.3 which depicts aggregate full-time THS employment and the number of THS establishments in West-Germany for the years 1975 to 2008. In accordance with Table 5.4, THS employment increased only little until the early 1990's, but has accelerated afterwards. As noted earlier,

even if the average share of THS employment in total employment seems to be rather small, it can be significantly larger for particular regions. The top five regions had a share of THS employment of around 8 per cent in 2008. The additional covariates which will be used in the regression analysis and that are listed in Table 5.4 are chosen according to Autor and Dorn (2011). Analytically, Figure 5.4 plots the routine manual and routine cognitive task shares in 1979 along with regional THS employment growth and reveals a positive relationship. Taken together, the first descriptive evidence provides encouraging results for the following regression analysis.

Figure 5.3: Aggregate full-time THS employment (in 1000s) and number of THS establishments in West-Germany, 1975-2008



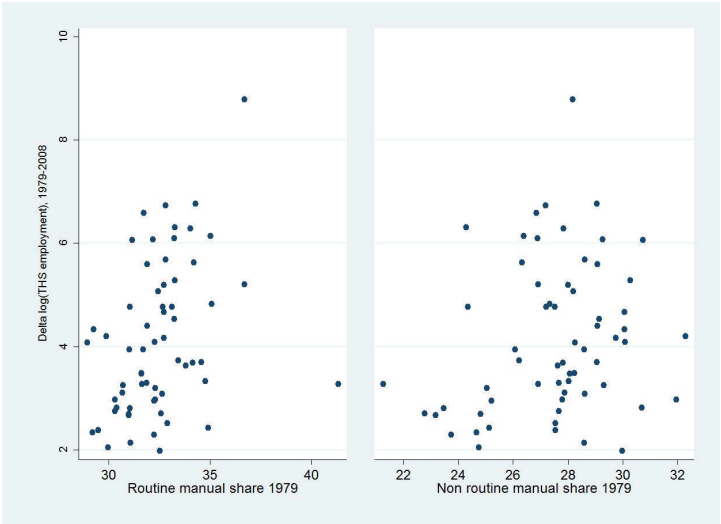
Source: IAB data; own calculations. See text for details.

5.3.3 Regression estimates for relationship between regional manual intensity and THS employment growth

A. Baseline estimates

As a first pass to analyzing the relationship between regional task intensity and THS employment, we relate the long difference in regional log THS employment between 1979 and 2008 to the initial manual shares of employment in 1979. The first column of Table 5.5 reports a coefficient estimate of 0.177 for the routine manual share, indicating that a 1 percentage point higher regional routine share in 1979 would translate into an increase in regional THS employment growth by 17.7 log points (∼17.8 percentage points) relative to the mean THS growth across all regions over 10 years. As to be expected from the earlier analysis on the differential penetration of THS by occupations intensive in non routine manual tasks, the relationship at the regional level also exhibits a significant positive

Figure 5.4: Routine manual task share and regional THS employment growth, 1979-2008



Source: IAB-BIBB data; own calculations. See text for details.

relationship. Apparently, regions with high shares of non routine manual work witnessed a relatively faster expansion of THS employment. Column 2 adds a measure of the population density (number of inhabitants per square kilometer) to control for differences in urbanity between the regions. While the coefficient on the population density enters with a negative sign, the coefficient on the routine manual share remains unchanged compared to a slightly smaller coefficient on the non routine manual share than in the base result. In Column 3, the regression is further augmented with the share of the population active in social security employment. This variable that serves as a proxy for the employment rate is negatively associated with later THS employment growth. Next, columns 4-8 add alternative measures for the initial structure of employment in a region. The relative share of female employment enters with a positive and significant sign. This result seems to be consistent with the idea that THS employment could thrive in regions with a large workforce with relatively lower labor force attachment and less initial human capital. Similarly, the initial share of young workers below 25 years is positively (but insignificantly) related to the subsequent growth of THS, indicating that regions employing a greater regional pool of young workers (who are also less likely to have already obtained a large amount of firm-specific human capital) could be a fertile ground for the spread of THS work. The specification in column 6 aims to evaluate the effect from the variation in the human capital intensity of the regional workforce more directly by including the ratio of high- to low-skilled employment as a measure of formal educational attainment. Surprisingly, a higher relative share of high-skilled workers in a region is related to comparatively faster growth of regional THS employment. However, the coefficient is not statistically different from zero. Column 7 includes the relative share of foreign employees. If THS are used by firms to realize labor cost savings, a larger relative supply of foreigners (that could potentially market their

skills at lower wages compared to natives) could have a moderating effect on THS growth. Indeed, the share of foreign employees exhibits a negative but insignificant coefficient and it leaves the coefficient on the routine manual share almost unaltered while the coefficient on the non routine manual share further declines. We also added the share of employment in large firms (defined as having more than 100 full-time employees) in column 8. Based on the literature on THS, there are two contradictory arguments regarding the expected effect of this explanatory variable: on the one hand, large firms might create relatively more demand for THS if they enjoy economies of scale in the outsourcing process, particularly if the use of THS requires a certain degree of sophistication in the management of HR processes. On the other hand, large firms in Germany are subject to more union influence due to the co-determination rights of works councils. Based on the partial correlation that we can observe in our regression analysis, the latter effect seems to dominate, as the coefficient estimate on this variable is negative but insignificant. Reassuringly, when the full set of explanatory variables is included (column 9), the point estimate on the routine manual share remains robust and still retains almost 98 percent of the size reported in the base specification in column 1. However, the point estimate on the non routine manual share is only 40 percent of its initial size and is no longer significant.

Table 5.5: Estimated impact of manual task shares on regional THS employment growth, 1979-2008

10 x annual Δ log(THS employment)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Routine manual share 1979	0.177***	0.177***	0.171***	0.176***	0.152***	0.181***	0.175***	0.170***	0.173***
	(0.029)	(0.024)	(0.024)	(0.024)	(0.027)	(0.034)	(0.024)	(0.025)	(0.043)
Non-routine manual share 1979	0.150***	0.115***	0.103***	0.106***	0.071**	0.113***	0.093***	0.095***	0.056
	(0.018)	(0.017)	(0.020)	(0.019)	(0.029)	(0.033)	(0.025)	(0.023)	(0.038)
Population density		-0.0005***	-0.0005***	-0.0004***	-0.0005***	-0.0005***	-0.0005***	-0.0005***	-0.0003***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Share employed/pop.			-0.012*	-0.015*	-0.008	-0.013*	-0.008	-0.009	-0.010
			(0.007)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.010)
Share female/empl.				0.033**					0.063***
				(0.016)					(0.023)
Share young (<25 years)/empl.					0.069				0.108**
					(0.043)				(0.046)
High to low-/medium-skilled empl.						0.019			0.015
						(0.046)			(0.062)
Share foreign empl./empl.							-0.012		-0.031**
							(0.012)		(0.015)
Share empl. large firms/empl.								-0.006	0.013
								(0.010)	(0.013)
R^2	0.465	0.568	0.577	0.597	0.591	0.578	0.580	0.580	0.642

Notes: N=111. All models include a constant and are weighted by start of period regional population. Robust standard errors in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

B. Estimates for subperiods

To get a better understanding of the underlying determinants of THS employment growth we further subdivide the long period 1979-2008 into three subperiods 1979-1994, 1994-2002 and 2002-2008. Since the early 1980's temporary agency employment in Germany underwent major deregulation and continuous liberalization, increasing the flexibility of THS use. Jahn and Bentzen (2010) point out that relaxing the regulations will affect the demand for THS employment as it decreases the quasi-fixed cost component of employment. Reforms in 1985, 1994 and 1997 gradually extended the maximum period of assignment from initially 3 months to 12 months and up to two years in 2002. The prohibition of THS employment in the construction sector (introduced in 1982) was abolished in 1994 and the re-employment and synchronization ban were relaxed from 1997 on. In order to improve the labor market situation of temp workers, the most recent reform in 2004 stipulated the equal pay and equal treatment principle between temp workers and the permanent workforce. Nevertheless, the temp agency can circumvent the new regulation by signing a sectoral collective agreement which makes the equal treatment principal obsolete.[20]

Table 5.6: Estimated impact of manual task shares on regional THS employment growth, subperiods

Dependent variable:	Time Period					
10 x annual	1979-1994		1994-2002		2002-2008	
Δlog(THS employment)	(1)	(2)	(3)	(4)	(5)	(6)
Routine manual share 1979	0.191***	0.120	0.178***	0.253**	0.142***	0.113**
	(0.049)	(0.094)	(0.063)	(0.106)	(0.032)	(0.052)
Non-routine manual share 1979	0.151***	-0.022	0.179***	0.149*	0.115***	0.044
	(0.035)	(0.077)	(0.042)	(0.087)	(0.037)	(0.060)
Regional covariates	no	yes	no	yes	no	yes
R^2	0.187	0.326	0.130	0.189	0.225	0.265

Notes: N=112. Each cell reports the results from a separate regression. All regressions include a constant and are weighted by start of period regional population. Robust standard errors in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

For our empirical analysis on the regional growth pattern of THS, these deregulation steps that amended the German national labor laws should only affect the posited secular relationship of the regional routine share if they in some way interacted with regional characteristics. We try to explore this possibility by re-estimating the regression model for three subperiods that each cover one or more of the aforementioned liberalization steps.[21] Table 5.6 presents the results for the base specification (column 1,3 and 5) and a specification including the regional covariates (columns 2,4 and 6) for each of the three

[20] Mitlacher (2005) documents that while this regulation led to an increase in temp worker wages, their average remuneration is still far below the minimum pay stipulated in many industry collective wage agreements, and thus likely has not eradicated potential savings from THS use altogether.

[21] As THS employment is strongly affected by business cycle fluctuations, start and end points of the subperiods should be comparable with respect to their location in the economic cycle. Apart from the base year 1979 we therefore select 1994, 2002 and 2008. Figure 6.4 in the Appendix depicts the evolution of the output gap in Germany between 1979 and 2010 and underlines the appropriateness of the selected subperiods.

subperiods, respectively. The results in Panel A confirm the positive relation between the manual routine share in 1979 and subsequent THS employment growth for all three subperiods, providing some further support for the notion that our previous estimates indeed capture a long-run secular relationship. As previously, a higher non routine manual share in 1979 also seems to predict a faster growth of THS employment throughout all subperiods, as presented in Panel B of Table 5.6. However, consistent with our previous estimates for the long difference from 1979 to 2008, the inclusion of covariates vastly reduces the coefficient estimates on the non routine manual share and renders them insignificant in nearly each period.

C. Robustness checks

As a first step to test the robustness of our results we repeat the OLS estimates using an alternative definition of regional labor markets following a delineation of the "Gemeinschaftsaufgabe *Verbesserung der regionalen Wirtschaftsstruktur*" (Koller and Schwengler, 2000). This classification is based on a less rigorous provision for commuter flows than the one used in our baseline and aggregates the administrative districts into 204 labor market regions for West Germany.[22] It is therefore somewhat finer compared to the definition previously employed, but for administrative reasons does not allow for broader labor market regions that span across state borders. However, as argued before, temp workers are frequently assigned to firms that are not necessarily located in the same district which might lead to an underestimation of the relation between regional routine intensity and subsequent THS employment growth and thus rather favors the use of the broader classification following Eckey et al. (2006) for the purpose of our analysis. In any case, while the coefficient estimates in columns 1 and 2 of Table 5.7 are somewhat smaller, their overall pattern is consistent with the earlier results.

Table 5.7: Estimated impact of manual task shares on regional THS employment growth, 1979-2008

Dependent variable:	Alternative regional		Spatial Error Model			
10 x annual	specification		Contiguity		Inverse Distance	
Δlog(THS employment)	(1)	(2)	(3)	(4)	(5)	(6)
Routine manual share 1979	0.131***	0.115***	0.151***	0.107**	0.153***	0.107**
	(0.017)	(0.034)	(0.028)	(0.041)	(0.027)	(0.042)
Non routine manual share 1979	0.127***	0.064**	0.130***	0.031	0.132***	0.031
	(0.015)	(0.028)	(0.021)	(0.040)	(0.021)	(0.040)
Regional covariates	no	yes	no	yes	no	yes
R^2	0.342	0.458				
Wald-Test			1.410	0.008	0.157	0.000
p-value			(0.235)	(0.927)	(0.692)	(0.999)
N	202	202	111	111	111	111

Notes: Each cell reports the results from a separate regression. Regressions in columns 1 and 2 are weighted by start of period regional population. Robust standard errors in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

[22] As before, we exclude the region "Südwestpfalz" which is part of the outlier region "Pirmasens" and "Garmisch-Partenkirchen", see footnote 10 on page 89 for details.

We also considered the possibility that outcomes in the regional labor market regions in our baseline analysis are potentially still spatially correlated and re-estimated spatial error models with contiguity and inverse distance weighting. While the contiguity matrix only consists of zeros and ones, the inverse-distance weighting matrix assigns weights that are inversely related to the distance between regions. Distance-based weight matrices are in general better suited to account for spatial dependency among regions than contiguity-based matrices as they describe the regional integration more accurately. However, as the results for the base specification in columns 3 and 5 suggest, both weighting methods yield very similar, but somewhat smaller coefficients compared to previous results. The coefficient on the routine manual share for 1979 further decreases with the inclusion of additional control variables (columns 4 and 6) but remains highly significant. The non routine manual share becomes insignificant with the inclusion of the regional covariates just like in the earlier analysis. Moreover, there is no evidence of significant spatial autocorrelation as suggested by the Wald test statistic and the associated p-value.

Many additional test further confirm the robustness of our results. In results not tabulated here, we tested alternative time periods in case our result are sensitive to the chosen end point of 2008. If we repeat our analysis with the time periods 1979-2006 or 1979-2007, the results remain virtually unchanged. We further experimented with different subsamples depending on the size and the region type of the specific labor market. We obtain similar result if we only consider urban or rural regions separately or if we estimate separate models for large (population >500 T in 1979) and small (population < 500 T in 1979) regions. Finally, we also tested specifications that included the initial share of THS employment in 1979 and the total employment growth rate between 1979 and 2008 as additional covariates to ensure that our results are not driven by a mere form of "catch up growth" or general differences in employment growth across regions. While these covariates enter with the expected sign (i.e. the initial share has a negative effect on the subsequent growth rate whereas the total employment growth rate has a positive effect), they are only marginally significant and leave the size and significance of our estimates on the regional manual shares unaltered.

D. Instrumental variable estimates

To shed some more light on the potential origin of the heterogeneous diffusion of THS employment across German regions, we exploit historical cross-regional differences in the production structure similar to the analysis by Autor and Dorn (2011) who relate the growth in local service sector employment since 1980 back to the regional industry structure in the U.S. in 1950. While this is not a true IV estimation in the sense that the instrument is completely exogenous, the approach should mitigate concerns about the endogeneity of the regional differences at the beginning of the sample period by isolating the long-run structural component of the regional employment composition. For this approach we use historical workplace and employee data (Arbeitsstätten- und Berufszählung) published

by the Statistical Federal Office which includes the number of employees and workplaces by industry and region and is available for several years just after the second world war (Statistisches Bundesamt, 1953). Similar to the task shares we generate the fraction of manufacturing in regional employment in 1950. For the share of manufacturing in 1950 to serve as a valid instrument, it must be (at least to some degree) exogenous and correlated with the routine manual share in 1979, conditional on the other covariates. The manufacturing share in 1950 is indeed highly correlated with the routine manual share in 1979 (correlation coefficient of 0.669).[23]

Panel II of Table 5.8 presents first-stage estimates for the IV model.[24] The first-stage coefficients are positive and highly significant and the high partial r-squared and F-statistics in the first-stage regression confirm the validity of the instrument. The first panel of Table 5.8 presents the main estimates for the growth of temporary help service employment using the IV model and reveals a similar pattern. The coefficient estimates are somewhat larger compared to the OLS results but highly significant and robust to the inclusion of additional covariates. In the specification including all regional covariates the coefficient from the IV estimation is 0.172 as against 0.134 in a comparable OLS model.

[23] We also require the instrument to be correlated with our dependent variable, the growth of THS employment, but only via its correlation with the routine manual share 1979. Indeed, the share of manufacturing in 1950 has only little explanatory power for the growth of THS employment between 1979 and 2008.

[24] As before, we leave out the outlier regions "Garmisch-Partenkirchen" and "Pirmasens" in the regressions. In addition, the region "Saarland" is excluded as it was reintegrated into the Federal Republic of Germany only in 1957 and thus is absent from the historical data in 1950.

Table 5.8: Estimated impact of routine manual task share on regional THS employment growth, 1979-2008: IV estimates

10 x annual Δ log(THS employment)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel I: IV estimates								
Manual routine share 1979	0.149**	0.121**	0.146***	0.133***	0.144***	0.113**	0.209***	0.144***	0.172***
	(0.063)	(0.048)	(0.040)	(0.040)	(0.032)	(0.046)	(0.034)	(0.038)	(0.059)
Population density		-0.0008***	-0.0007***	-0.0006***	-0.0006***	-0.0006***	-0.0007***	-0.0005***	-0.0004***
		(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0001)
Share employed/pop.			-0.027***	-0.031***	-0.011*	-0.014*	-0.010	-0.016*	-0.009
			(0.009)	(0.011)	(0.006)	(0.008)	(0.007)	(0.008)	(0.009)
Share female/empl.				0.028					0.074***
				(0.020)					(0.022)
Share young (<25 years)/empl.					0.132***				0.142***
					(0.030)				(0.038)
High to low-/medium-skilled empl.						-0.107***			-0.014
						(0.039)			(0.076)
Share foreign empl./empl.							-0.038***		-0.040**
							(0.012)		(0.019)
Share empl. large firms/empl.								-0.020**	0.016
								(0.010)	(0.014)
R^2	0.212	0.415	0.486	0.494	0.566	0.532	0.519	0.513	0.631
	Panel II: First stage								
Share manufacturing 1950	0.113***	0.115***	0.124***	0.133***	0.123***	0.108***	0.153***	0.124***	0.104***
	(0.020)	(0.020)	(0.016)	(0.015)	(0.014)	(0.013)	(0.013)	(0.016)	(0.016)
F-statistic	32.335	32.726	57.228	73.675	72.180	72.994	141.946	57.384	40.010
Partial R^2	0.437	0.443	0.508	0.565	0.541	0.503	0.550	0.510	0.292

Notes: N=110. All models include a constant and are weighted by start of period regional population. Robust standard errors in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

E. Estimates for East Germany

We complete our empirical analysis on regional THS employment trends by extending our focus to regions in East Germany from 1992 onwards (the first year with data available). The perspective on East Germany is of particular interest as the use of THS employment has also increased tremendously since reunification and, according to some recent reports for the German Federal Labor Agency, the overall average of 2.8 per cent of employment in 2010 even exceeds the West-Germany average (2.6 percent).[25]

Table 5.9: Estimated impact of manual task shares on regional THS employment growth in East Germany, 1992-2008

Dependent variable: Time Period								
10 x annual	1992-2008		1992-1994		1994-2002		2002-2008	
Δlog(THS employment)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R manual share 1992	-0.178	-0.054	-2.254***	-1.141	0.092	0.135	0.160**	0.051
	(0.110)	(0.162)	(0.818)	(0.957)	(0.100)	(0.131)	(0.077)	(0.089)
NR manual share 1992	0.211	0.236	1.239	2.268	0.071	-0.085	0.050	0.057
	(0.146)	(0.255)	(0.933)	(1.503)	(0.121)	(0.134)	(0.054)	(0.093)
Regional covariates	no	yes	no	yes	no	yes	no	yes
R^2	0.107	0.250	0.215	0.294	0.035	0.287	0.205	0.399

Notes: N=36 labor markets in East Germany excluding Berlin. Each cell reports the results from a separate regression. All regressions include a constant and are weighted by start of period regional full-time employment. Due to data availability, population density is replaced by a dummy variable indicating urban or rural areas. Robust standard errors in parentheses. * Significant at 10%, ** at 5%, *** at 1%.

Analogous to our previous analyses, we relate the growth in THS employment for the long difference from 1992 to 2008 as well as separately for the three subperiods 1992-1994, 1994-2002 and 2002-2008 to the initial regional routine and non routine manual share in 1992.[26] Results are presented in Table 5.9. Note that as the sample for East Germany only consists of 36 regions, the coefficients on the manual shares in 1992 are estimated with less precision. At first glance, the results for the long difference 1992-2008 and the first subperiod 1992-1994 in columns 1 to 4 seem to follow a somewhat diverging pattern from the results for West German regions as the coefficient estimates on the routine manual share have a negative sign, but are insignificant, whereas the positive coefficient estimates for the non routine manual share are larger in absolute size. However, it is important to note that our empirical approach suffers from an important caveat for East Germany. Measured in 1992 only two years after reunification, the regional task shares for East German regions are much less likely to capture stable differences in regional production structures as the differences at the time were still strongly affected by the ongoing massive transformation of the entire East German economy.[27] The results for the later subperiods

[25] Cf. Jahn and Wolf (2005); Fuchs (2009b,a). For some regions especially in Thuringia, THS employment is reported to constitute for more than 9 per cent of overall regional employment.

[26] See Figure 6.5 in the Appendix for the graphical mapping of the regional manual shares and the THS growth rates.

[27] See, e.g., Sinn and Sinn (1994) and Burda and Hunt (2001) on the many facets of this process.

from 1994-2002 and 2002-2008 again reveal relationships that seem to be more similar to the pattern observed for West Germany.

5.4 Conclusion

In this study, we have argued that the notion of the routine task intensity of labor that forms a centerpiece of the task and trade-in-task literature also provides a useful framework for the understanding of the secular rise of THS employment, a feature of growing importance for many labor markets in industrialized countries. We start out by confirming that employment in occupations that were initially intensive in routine manual tasks are indeed particularly prone to being outsourced into THS. This positive relationship is observed for non routine manual tasks as well, but with less significance. In our main analysis, we apply this concept in the spirit of Autor and Dorn (2011) to the level of regional labor market differences and find that the growth of employment by temporary help services has been particularly pronounced in regions that initially were specialized in routine manual labor. The economic magnitude of this relationship is large and robust to the inclusion of variables that control for alternative explanations due to regional differences in labor supply and demand conditions as well as alternative model specifications. The initial non routine manual task intensity is also positively related to subsequent THS employment growth, but loses explanatory power after controlling for relevant covariates.

Our study aims to contribute to the emerging literature on the patterns of polarization in regional labor markets. We would like to conclude by stressing that while our results suggest an important role for THS employment in this development, several questions about the fundamental connection between THS use and employment and wage polarization have to be left open at this stage and should deserve further scrutiny in future research. In particular, we think that more empirical evidence at the firm level of the determinants for and the connections between the use of alternative forms of mediated labor market arrangements that include offshoring, (business process) outsourcing as well as temporary help services is highly warranted. At the regional labor market level, it would be interesting to see to what extent the differential penetration of THS also has repercussions on the wage bargaining process and could therefore contribute to regional wage inequality.

6 Appendix

6.1 Data appendix on military forces and base closures in Germany

6.1.1 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.[1] 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

[1] 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.

that could bias the results towards finding a spurious effect.[2]

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 in the local economy.[3]

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[4], 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, n.d.). We can thus attribute the manpower data with great precision and confidence to the districts in which the respective sites are located.

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

[3] 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.

[4] 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.

6.1.2 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, n.d.);

(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;

(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.[5]

6.1.3 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).

[5]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.

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.[6]

6.1.4 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 official 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.

(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.

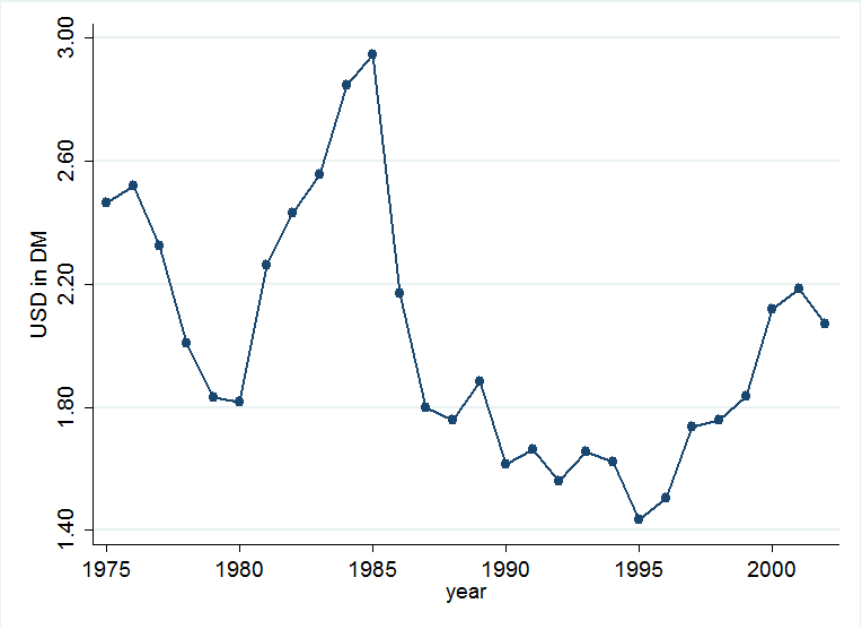
(3) A dedicated entry on Wikipedia (2011) provides a comprehensive list of all (active and closed) foreign military bases in Germany that was used to validate the information obtained from sources (1) and (2).

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.

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

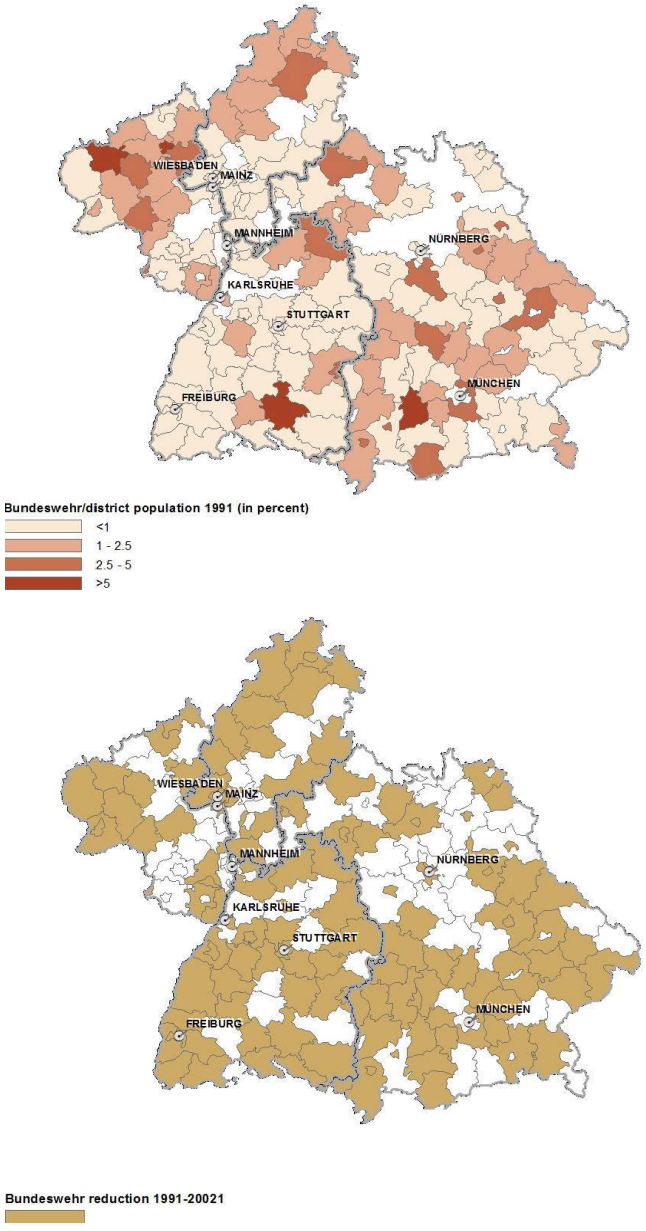
6.2 Appendix to Chapter 2

Figure 6.1: Average yearly USD/DM exchange rate



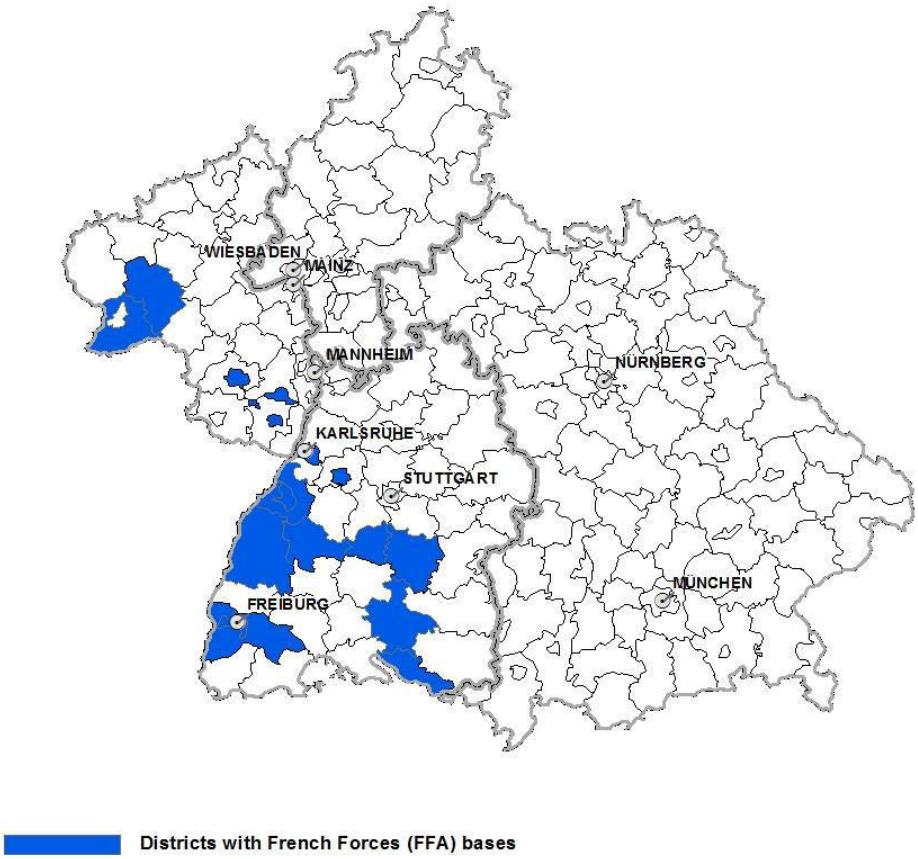
Source: Deutsche Bundesbank (n.d.); own calculations.

Figure 6.2: Districts in southwest Germany with presence of German armed forces
A.(top) Districts with presence of German armed forces (Bundeswehr), 1991
B. (bottom) Districts with reductions of German armed forces, 1991-2002



Source: See appendix 6.1.3 for details on data sources; own calculations.

Figure 6.3: German districts with French force (FFA) bases



Source: See appendix 6.1.4 for details on data sources; own calculations.

Table 6.1: Regional classification scheme based on BBR classification

Structural region type	District type (BBR classification)	Region types in this paper	Description of region type (BBR)
Regions with large agglomerations	BBR 1	Urban	Core cities
	BBR 2	Urban	Highly urbanized districts in regions with large agglomerations
	BBR 3	Rural	Urbanized districts in regions with large agglomerations
	BBR 4	Rural	Rural districts in regions with large agglomerations
Regions with features of conurbation	BBR 5	Urban	Central cities in regions with intermediate agglomerations
	BBR 6	Rural	Urbanized districts in regions with intermediate agglomerations
	BBR 7	Rural	Rural districts in regions with intermediate agglomerations
Regions of rural character	BBR 8	Rural	Urbanized districts in rural regions
	BBR 9	Rural	Rural districts in rural regions

Source: Möller and Lehmer (2010), p. 51, Table 8.

6.3 Appendix to Chapter 3

Table 6.2: Descriptive statistics for control variables at level of individual employees

	1984-1990			1991-2002		
	Treatm.	Contr.	Diff.	Treatm.	Contr.	Diff.
Socio-demographic characteristics						
Age (in years)	39.54	39.03	.503***	38.90	38.48	.422***
	(0.10)	(0.09)	(0.13)	(0.08)	(0.06)	(0.10)
Foreign citizenship	.130	.098	.032***	.139	.113	.026**
	(.010)	(.006)	(.012)	(.009)	(.006)	(.011)
Low education	.175	.193	-.018**	.146	.142	.004
	(.005)	(.005)	(.007)	(.004)	(.004)	(.005)
Medium education	.825	.807	.018**	.854	.858	-.004
	(.005)	(.005)	(.007)	(.004)	(.004)	(.005)
Labor market segment						
Weak worker influence (< 50 employees)	.340	.403	-.064***	.376	.428	-0.05**
	(.015)	(.014)	(.021)	(.016)	(.013)	(.020)
Firm entry regulation (GTCC)	.284	.317	-.033***	.267	.304	-.037***
	(.008)	(.005)	(.009)	(.010)	(.006)	(.011)
Occupational structure						
1 Production, basic materials workers	.414	.464	-.050***	.407	.465	-.058***
	(.017)	(.008)	(.019)	(.019)	(.009)	(.021)
2 Craft/construction workers	.117	.142	-.026***	.102	.129	-.027***
	(.006)	(.005)	(.007)	(.006)	(.005)	(.008)
3 Professionals	.114	.093	.021***	.109	.090	.019***
	(.005)	(.004)	(.006)	(.005)	(.004)	(.006)
4 Associate Professionals/Technicians	.100	.081	.020***	.100	.084	.016***
	(.005)	(.003)	(.006)	(.004)	(.004)	(.006)
5 Clerks and sales workers	.101	.077	.024***	.115	.082	.033***
	(.008)	(.003)	(.009)	(.011)	(.003)	(.011)
6 Transport/Security profs. and workers	.127	.124	.003	.136	.129	.008
	(.003)	(.003)	(.004)	(.004)	(.003)	(.006)
7 Research/Education profs. and artists	.009	.006	.003**	.009	.006	.003*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.002)
8 Elementary services workers	.018	.013	.004*	.021	.016	.006*
	(.002)	(.001)	(.002)	(.003)	(.001)	(.003)
Industry structure						
1 Basic materials	.097	.134	-.037	.089	.119	-.030
	(.011)	(.023)	(.026)	(.009)	(.019)	(.021)
2 Investment goods	.314	.308	.006	.288	.300	-.013
	(.023)	(.020)	(.030)	(.021)	(.018)	(.028)
3 Food and consumption goods	.115	.154	-.039***	.104	.138	-.034***
	(.010)	(.010)	(.014)	(.009)	(.008)	(.012)
4 Construction	.130	.156	-.026***	.118	.145	-.028***
	(.005)	(.005)	(.007)	(.006)	(.005)	(.008)
5 Retail/Repair	.133	.106	.027***	.139	.120	.019**
	(.006)	(.006)	(.008)	(.005)	(.007)	(.008)
6 Transport/Information	.077	.058	.020**	.085	.067	0.02*
	(.009)	(.003)	(.009)	(.009)	(.005)	(.011)
7 Corporate services	.100	.058	.042***	.139	.079	.060***
	(.012)	(.003)	(.012)	(.016)	(.005)	(.017)
8 Private hh. services	.033	.026	.007	.038	.031	.007
	(.005)	(.002)	(.005)	(.005)	(.002)	(.006)

Notes: This table displays means/shares for individual variables used as controls in the regression analysis. The sample includes all full-time male employees with low or medium education aged 25-55, separately for 86 treatment and 96 control districts and time periods 1984-1990 and 1991-2002. Employees in agriculture and fishing, mining and quarrying as well as the public sector are excluded from the analysis. Data in columns 4 and 7 indicate the mean differences between treatment and control district within the two time periods, with stars indicating significant differences from equality-of-means tests: * denotes significance at 10%, ** at 5%, *** at 1%.

Table 6.3: Establishment size distribution across product market groups

Number of employees	Product markets without GTCC entry regulation		Product markets with GTCC entry regulation	
1-4	6.60		10.29	
5-499	63.62		69.31	
5-9		*5.89*		*11.48*
10-49		*20.69*		*27.83*
50-99		*10.93*		*10.25*
100-499		*26.12*		*19.75*
500 or more	29.78		20.40	
500-999		*9.22*		*6.46*
1000 or more		*20.56*		*13.94*

Notes: This table displays the establishment size distribution of the individual observations in product markets with GTCC firm entry regulation and product markets without it. The sample includes all full-time male employees with low or medium education aged 25-55 for the time period 1984-2002. Employees in agriculture and fishing, mining and quarrying as well as the public sector are excluded from the analysis.

Table 6.4: Heterogeneity of wage responses by GTCC entry regulation - subsamples by industry sectors

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
	1 Basic materials			
U.S. withdrawal x GTCC regulation (R)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
	2 Investment goods			
U.S. withdrawal x GTCC regulation (R)	-.001	-.001	-.001	-.001
	(.000)	(.000)	(.000)	(.000)
	3 Food and consumption goods			
U.S. withdrawal x GTCC regulation (R)	-.000	-.000	-.000	-.000
	(.001)	(.001)	(.001)	(.001)
	4 Construction			
U.S. withdrawal x GTCC regulation (R)	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)
	5 Retail/Repair			
U.S. withdrawal x GTCC regulation (R)	.001	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)
	6 Transport/Information			
U.S. withdrawal x GTCC regulation (R)	-.001	-.001	-.001	-.001
	(.001)	(.001)	(.001)	(.001)
	7 Corporate Services			
U.S. withdrawal x GTCC regulation (R)	-.001	-.001	-.000	-.000
	(.001)	(.001)	(.001)	(.001)
	8 Private household services			
U.S. withdrawal x GTCC regulation (R)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
Other covariates:				
GTCC indicator (R) x year effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes

Notes: * denotes significance at 10%, ** at 5%, *** at 1%.

Table 6.5: Heterogeneity of wage responses by interaction of boths regulations - subsamples by industry sectors

Dep. Variable: Real gross daily wage (log)	(1)	(2)	(3)	(4)
	1 Basic materials			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	-.002	-.002	-.002	-.002
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x weak workers' influence (W)	.000	.000	.001	.001
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x GTCC regulation (R)	-.000	-.000	-.000	-.000
	(.001)	(.001)	(.001)	(.001)
	2 Investment goods			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x weak workers' influence (W)	-.001	-.000	-.001	-.001
	(.000)	(.000)	(.000)	(.000)
U.S. withdrawal x GTCC regulation (R)	-.002*	-.001*	-.001*	-.001*
	(.001)	(.001)	(.001)	(.001)
	3 Food and consumption goods			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	-.002*	-.002*	-.002	-.002
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x weak workers' influence (W)	.001	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x GTCC regulation (R)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
	4 Construction			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	-.000	-.000	-.000	-.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x weak workers' influence (W)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x GTCC regulation (R)	-.002*	-.002*	-.002*	-.002*
	(.001)	(.001)	(.001)	(.001)
	5 Retail/Repair			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	.001	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x weak workers' influence (W)	-.000	.000	-.000	-.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x GTCC regulation (R)	.001	.001	.000	.000
	(.001)	(.001)	(.001)	(.001)

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Table 6.5 – ***continued from previous page***

	(1)	(2)	(3)	(4)
	6 Transport/Information			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	-.000	-.000	-.000	-.000
	(.003)	(.003)	(.003)	(.003)
U.S. withdrawal x weak workers' influence (W)	.000	.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
U.S. withdrawal x GTCC regulation (R)	-.002	-.002	-.001	-.001
	(.001)	(.001)	(.001)	(.001)
	7 Corporate Services			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	-.005*	-.005*	-.006*	-.006*
	(.003)	(.003)	(.003)	(.003)
U.S. withdrawal x weak workers' influence (W)	.002	.002	.003	.003
	(.002)	(.002)	(.002)	(.002)
U.S. withdrawal x GTCC regulation (R)	.000	.000	.001	.001
	(.001)	(.001)	(.001)	(.001)
	8 Private household services			
U.S. withdrawal x weak workers' influence (W) x GTCC (R)	-.006**	-.006**	-.007**	-.007**
	(.003)	(.003)	(.003)	(.003)
U.S. withdrawal x weak workers' influence (W)	.003**	.003**	.003**	.003**
	(.001)	(.002)	(.001)	(.001)
U.S. withdrawal x GTCC regulation (R)	.002	.002	.003	.003*
	(.002)	(.002)	(.002)	(.002)
Other covariates:				
(Weak influence (W) x GTCC (R)) x year effects	Yes	Yes	Yes	Yes
(Weak influence (W) x GTCC (R)) x district effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x year effects	Yes	Yes	Yes	Yes
Weak influence indicator (W) x district effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x year effects	Yes	Yes	Yes	Yes
GTCC indicator (R) x district effects	Yes	Yes	Yes	Yes
Industry x year effects	Yes	Yes	Yes	Yes
Education x year effects	Yes	Yes	Yes	Yes
State by year effects	No	Yes	Yes	Yes
District x time trends	No	No	Yes	Yes
District x time^2 trends	No	No	No	Yes

6.4 Appendix to Chapter 4

6.4.1 Description of crime data

Crime data at district level for each federal state is collected by the State Criminal Police Offices. The police crime statistics (*Polizeiliche Kriminalstatistik, PKS*) in Germany report all criminal actions and punishable attempts that are classified as a crime by the list of offenses. An offense is recorded in a reporting period after the police investigations have ended and before the case is handled over to the Public Prosecutor's Office or the criminal court. Crime categories used in this analysis are measured according to the official list of offenses of the Bundeskriminalamt (1990):

Total offenses refers to the sum of the total number of crime incidences that are known to the police.

Incidences of **drug offenses** capture the number of recorded crimes that represent violations under the Narcotics Act and include general violations, the trafficking in and smuggling of drugs as well as the illegal importation and cultivation of drugs.

Sex offenses (including rape) refers to offenses against the sexual self-determination with use of violence or exploiting a state of dependence. Sex offenses include incidences of sexual abuse, the exploitation of sexual inclinations and rape. **Rape** is described as the coercion of a person into sexual activities with the perpetrator or a third party.

Total theft is defined as all cases of theft with and without aggravating circumstances and in this analysis includes all incidences of robbery and violent theft, breaking and entering and theft of motor cars that are known to the police.

Robbery and violent theft measures incidences of robbery and of extortion accompanied by violence. Incidences of assault on motorists with the intent to rob are also counted as robbery and violent theft.

Breaking and entering is defined as the theft in and from dwellings.

Theft of motor cars refers to the number of incidences known to the police that are defined as the theft of motor vehicles (including taking without consent).

Bodily injury includes bodily injury resulting in death, serious assault, mistreatment of persons under offender's care, (intentional slight) bodily injury and negligent bodily injury. **Serious assault** refers to the number of incidences of dangerous and serious bodily injury that are reported to the police.

Damage to property captures general incidences such as damage to property by fire as well as damage to motor vehicles, alteration of data, computer sabotage, other damage to property committed in streets, lanes or public places and the destruction of important equipment.

Misdemeanors against the public order describes the resistance to public authority and offenses against public order which includes trespassing on the premises of another, the breach of the public peace, the feigning commission of a crime and the glorification of violence.

Offenses against life is described by crime incidences of murder, manslaughter and the killing of another person at his own request as well as by cases of homicide by negligence.

6.4.2 Table appendix to chapter 4

Table 6.6: Selected district characteristics by treatment status

	1984-1989		1990-2002	
	Treatment	Control	Treatment	Control
	(1)	(2)	(3)	(4)
Incidence of Crime				
Total crime rate	5,724	4,460	6,180	5,090
	(3,264)	(2,103)	(3,110)	(2,351)
	[2,156;23,251]	[1,684;13,863]	[2,255;21,345]	[404;14,894]
Drug offenses rate	108	74	228	203
	(80)	(68)	(180)	(166)
	[7;534]	[3;468]	[17;1,055]	[5;1,223]
Sex offenses rate	55	47	55	48
	(30)	(29)	(28)	(24)
	[8;170]	[8;202]	[15;192]	[6;182]
Rape rate	9	6	8	7
	(5)	(4)	(5)	(5)
	[0;30]	[0;37]	[0;42]	[0;41]
Theft rate (total)	3,204	2,320	3,042	2,280
	(2,298)	(1,400)	(1,892)	(1,328)
	[898;16,354]	[644;9,582]	[840;13,077]	[692;8,985]
Robbery rate	34	21	44	28
	(36)	(20)	(46)	(26)
	[1;219]	[1;132]	[4;374]	[2;205]
Motor cars theft rate	81	50	74	48
	(72)	(32)	(70)	(36)
	[8;456]	[4;205]	[10;563]	[4;402]
Demographics				
Population	195,315	120,882	210,279	134,027
	(174,894)	(62,017)	(178,440)	(68,973)
	[33,703;1,274,716]	[37,284;373,065]	[35,514;1,256,638]	[39,333;415,764]
Population density	635	292	669	317
(inhabitant\sqkm)	(770)	(342)	(787)	(363)
	(66;4,106)	(66;1,795)	(70;4,048)	(71;1,893)
Share young male	0.093	0.098	0.062	0.065
population (age 15-25)	(0.030)	(0.034)	(0.015)	(0.020)
	[0.060;0.192]	[0.061;0.202]	[0.047;0.147]	[0.043;0.168]
Share foreign	0.077	0.057	0.105	0.081
population	(0.045)	(0.031)	(0.056)	(0.039)
	[0.007;0.229]	[0.008;0.140]	[0.022;0.280]	[0.015;0.289]

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Table 6.6 – *continued from previous page*

	1984-1989		1990-2002	
	Treatment	Control	Treatment	Control
	(1)	(2)	(3)	(4)
Socio-economic indicators				
GDP per capita†	38,423	32,988	45,217	38,470
(in national currency)	(17,441)	(10,721)	(19,648)	(12,429)
	[12,671;115,366]	[14,033;73,236]	[14,686;127,542]	[17,416;89,604]
Unemployment Rate	6.6	6.5	6.8	5.9
	(2.3)	(2.8)	(2.4)	(2.3)
	[2.6;23.6]	[2.6;22.2]	[2.1;16.8]	[1.8;18.9]
Area type				
Urban	0.364	0.122	0.364	0.122
Border districts	0.152	0.402	0.152	0.402
Geographic distribution				
Hesse	0.288	0.049	0.288	0.049
Baden-Württemberg	0.273	0.305	0.273	0.305
Bavaria	0.439	0.646	0.439	0.646
N	66	82	66	82

Notes: This table display means/shares for the sample of treatment and control districts used in the analysis. Values in round brackets report standard deviations, whereas values in squared brackets are [Min; Max]. Crime rates are defined as the numbers of crimes known to the police per 100,000 residents.
†Due to data limitations, GDP per capita are reported only until 1997.

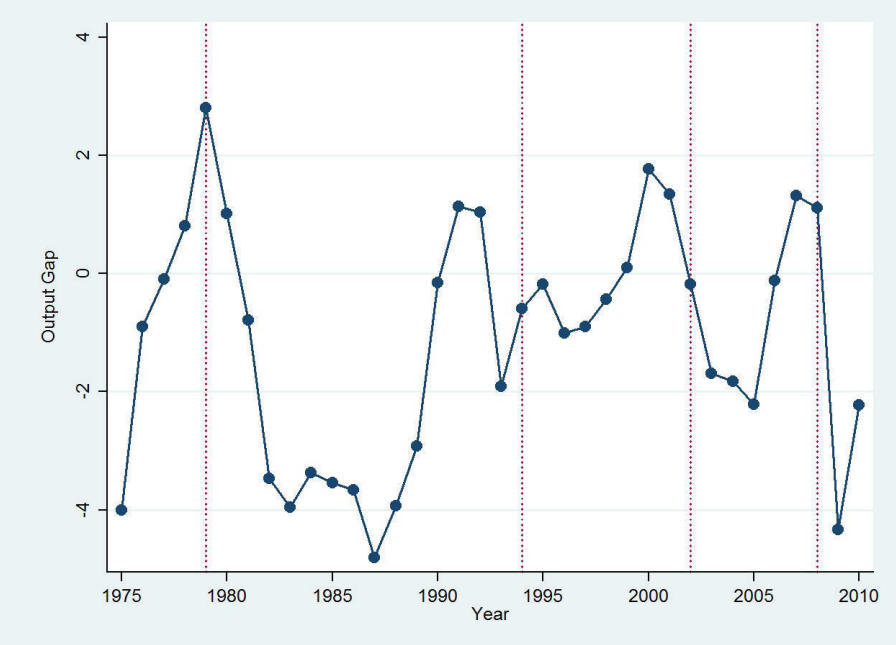
6.5 Appendix to Chapter 5

Table 6.7: Top 10 occupations with highest THS share in 2008 and their task content in 1979

Occupation	THS Share 2008	Δ THS Share 1979-2008	non-routine analytic	non-routine interactive	routine cognitive	routine manual	non-routine manual
Laborers	0.545	0.510	0.009	0.035	0.060	0.600	0.296
Welders	0.213	0.196	0.002	0.084	0.076	0.441	0.397
Transport equipment drivers	0.195	0.194	0.000	0.058	0.091	0.522	0.329
Storekeeper	0.166	0.158	0.007	0.129	0.150	0.380	0.334
Building fitters	0.148	0.131	0.028	0.049	0.057	0.250	0.616
Painters	0.145	0.138	0.028	0.150	0.030	0.293	0.499
Clothing sewers	0.135	0.135	0.040	0.132	0.064	0.295	0.469
Data typists	0.124	0.119	0.000	0.042	0.165	0.761	0.031
Electrical fitters	0.096	0.087	0.080	0.138	0.070	0.145	0.568
Telephonists	0.093	0.091	0.031	0.155	0.190	0.624	0.000

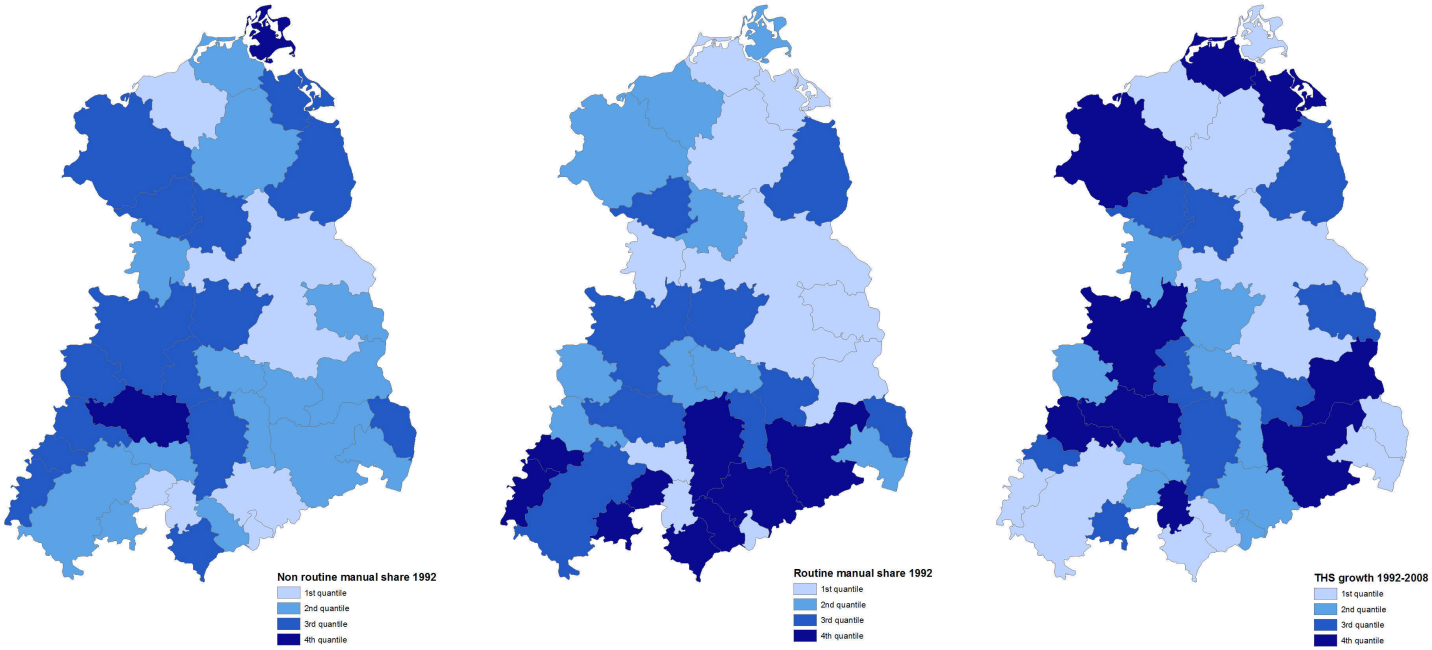
Notes: Occupations are defined according to the three-digit level of the classification of occupational titles 1988. Occupational task information derived from BIBB/IAB data, share of THS employment derived from SIAB. Sample includes West German workers aged 20-60 excluding agricultural and public sector employment.

Figure 6.4: Output Gap, 1975-2010



Source: International Monetary Fund, World Economic Outlook Database, September 1999, October 2010.
Notes: Output gap in percent of potential GDP. Dotted vertical lines indicate years used for the analysis.

Figure 6.5: Routine task share and THS growth in East Germany, 1992-2008
left: Routine manual task share *center:* Routine cognitive task share *right:* THS employment growth by region, 1992-2008



Source: IAB-BIBB data; own calculations. See text for details.

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Selbständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.
Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den 29.05.2012 Jan Peter aus dem Moore