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# The Regional Distribution of Income and Wages: Causes and Consequences

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# Preface

Lea Immel prepared this study while she was working at the Research Group Taxation and Fiscal Policy at the ifo Institute. The study was completed in September 2020 and accepted as doctoral thesis by the Department of Economics at the University of Munich. It consists of four distinct empirical essays investigating the causes and consequences of regional differences in the distribution of income and wages using county-level data from Germany. Chapter 2 analyzes the impact of the so-called Hartz reforms on income inequality in German regions. Chapter 3 investigates the long-term effect of university openings on the wage distribution in the surrounding area. Chapters 4 and 5 turn to the political consequences of regional income dispersion and examine the effect of relative economic deprivation on the support for radical right- and left-wing parties as well as on local fiscal policy. The chapters employ difference-in-differences, event study, and instrumental variable estimations to answer the respective research questions.

Keywords: Labor market policy, income inequality, wages, Germany, Hartz reforms, universities, knowledge spillovers, local labor markets, economic deprivation, political polarization, radical voting, poverty, taxation, fiscal policy, public services, difference-in-differences, event study, instrumental variables

JEL-No: D31, D73, H41, H71, H72, I23, I25, I32, J08, J31, J68, R23, R58

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Furthermore, I thank Andreas Peichl for co-supervising my thesis, for helpful comments and feedback—especially on Chapter 2 of this thesis—as well as for great collaboration and leadership on joint policy projects. Many thanks to Dominik Sachs for joining my committee.

I am deeply indebted to Florian Neumeier for being my mentor, unofficial boss, and much valued co-author. Not only could I always rely on him for great advice, guidance, feedback, and support, he also put tremendous efforts into the development and running of our research group: Coming up with new research ideas to shape the profile of our group, winning our first policy projects, and always making sure I have enough time to focus on my thesis.

I thank my colleagues at the ifo Research Group on Taxation and Fiscal Policy, especially my office neighbor Daniel Stöhlker as well as our fellow doctoral students at CES Felix Hugger and Samina Sultan for a great working atmosphere, coffee breaks, skiing trips, great discussions, and productive co-authorship. Interns, the administrative staff at the ifo Institute, as well as colleagues from other ifo centers also provided me with helpful support during the entire time. Special mention to Franca Fogli for always finding a spot in Clemens Fuest's schedule to squeeze me in.

I am immensely grateful to Elisabeth Grewenig, Stefanie Gäbler, and Feodora Teti for incredible support in- and outside of ifo throughout the last four years and particularly in the final stages of my dissertation.

Most of all, I am grateful to my family and friends for all the support and encouragement you have given me, not just in the last four years but throughout my life. Thank you!

Lea Immel
September 2020

# The Regional Distribution of Income and Wages: Causes and Consequences

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Lea Immel

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| | Prof. Dr. Andreas Peichl |
| | Prof. Dr. Dominik Sachs |

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# 1 Introduction

Since Thomas Piketty's 'Capital of the $21^{st}$ Century' the unequal distribution of income and wages has been at the forefront of academic and policy debates as at no other time in recent history. Former U.S. president Barack Obama even called the widening of the income distribution the 'defining challenge of our time' (Guardian, 2013). While most of the political and academic debates about inequality have focused on the extent of inequality at the global or national level, recently the sub-national dimension of inequality, i.e., inequality *between* and *within* regions, has gained importance. This has mostly been due to the realization that the wave of populism, many developed countries have been experiencing, has strong regional rather than social foundations (Rodríguez-Pose, 2018). The Brexit vote, the election of Donald Trump, as well as the strong performance of the far right National Rally (*Rassemblement National*) in French elections, for example, all follow regional patterns, where the success of populist politicians, parties, and/or positions is greater in declining and 'lagging-behind' regions (Hüther et al., 2019).

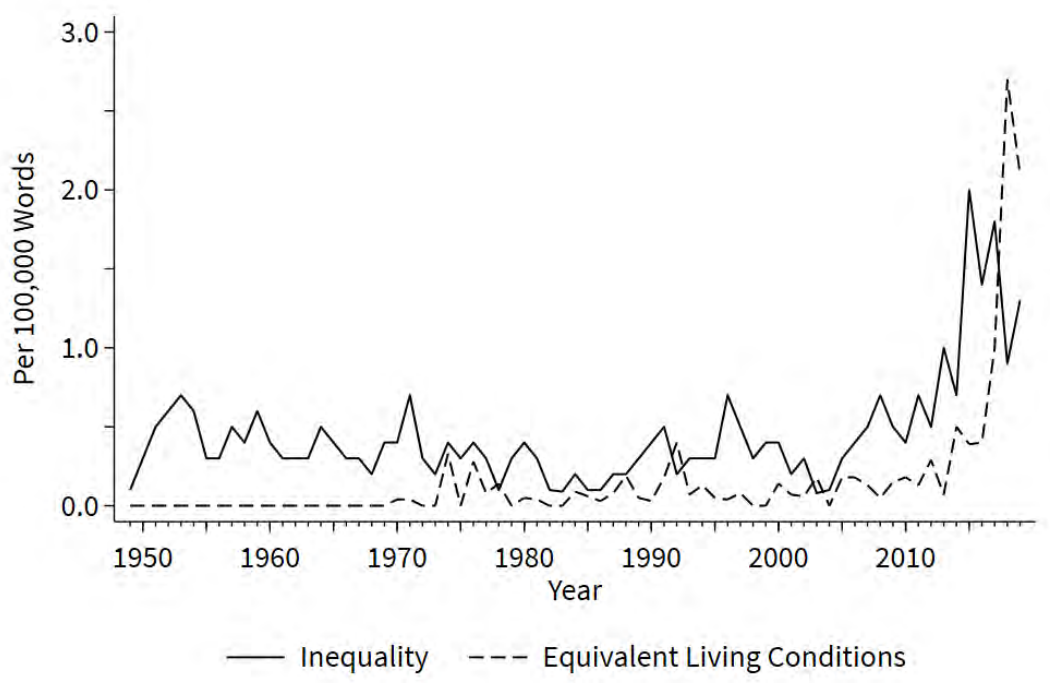
So far, the economic literature investigating regional inequalities has focused on documenting and explaining wage discrepancies between regions. In particular, a body of studies in the agglomeration literature linking location to wages find that differences in regional wages are at least partly associated with city size (the so-called urban wage gap) and geographic density (see Duranton and Puga, 2014; Glaeser and Mare, 2001; Rosenthal and Strange, 2004, for a general discussion). Recently, a new literature has emerged which moves beyond agglomeration economics and the urban wage gap and investigates the differential impact of global trends such as globalization (Autor et al., 2013; Dauth and Suedekum, 2016) or automation (Acemoglu and Restrepo, 2020; Dauth et al., 2019) on local labor markets. In contrast to wages, studies investigating income inequality between regions are scarce, and even less is known about wage and income inequality within regions. With this thesis, I contribute to the literature on regional inequalities by investigating causes and consequences of the regional distribution of income and wages in Germany.

Germany provides an interesting setting to study the regional distribution of income and wages for several reasons—both political and academic. First, Germany is a country with great historical, cultural, political, economic, socio-cultural, and administrative differences. While Germany has a multitude of prospering regions and economic centers, at the same time there are also economically weak cities and stagnating rural regions, particularly in East Germany (Hüther et al., 2019).

Second, like in many other developed countries, regional inequalities are increasingly on the political agenda. They are seen as a violation of the principle of 'equivalent living conditions' as enshrined in the German constitution and calls for an expansion of political measures to

contain regional differences are getting louder. The growing importance of regional inequality in the political debate in Germany can also been seen in Figure 1.1.

Figure 1.1: Importance of Inequality and Equivalent Living Conditions in the German Parliament




*Source:* ZEIT Online (2020).
*Notes:* This figure shows the appearance of the words 'Ungleichheit' (inequality) and 'gleichwertige Lebensverhältnisse' and 'gleichwertiger Lebensverhältnisse' (equivalent living conditions) per 100,000 words in debates of the German Parliament (*Deutscher Bundestag*). It is based on an analysis of 4.216 plenary sessions from the German Parliament between September 7, 1949 and July 24, 2019.

The figure depicts the appearances of the words 'inequality' and 'equivalent living conditions' per 100,000 words in debates of the German Parliament (*Deutscher Bundestag*) from 1949 to 2019 and clearly shows that over the last years inequality within and between regions has become an important policy concern.

Third, due to the federal structure of Germany, power is divided between the federal government, the 16 German states, as well as local authorities. Local authorities in Germany consist of the 401 counties (*Kreise*) and city districts (*Kreisfreie Städte*), and over 11.000 municipalities. As local authorities play an important role in shaping regional development, this gives researchers ample local variation to study regional inequalities.

Fourth, analyzing the regional distribution of income and wages is only possible because in Germany there exist some high-quality data sets providing information on household income and individual wages that can be analyzed at the regional level. In this thesis, I mainly rely on two data sources, the German Microcensus and the Sample of Integrated Labor Market

Institutions (*Stichprobe der integrierten Arbeitsmarktbiographien*, SIAB). Both data sets are available at the county- and city district-level, corresponding to NUTS-3.

The German Microcensus is an annual household survey administered by the Federal Statistical Office (*Statistisches Bundesamt*). It is a representative one percent sample of the German population which is also representative at the regional level. The Microcensus contains information on a large number of demographic characteristics, including net household income. Besides the large sample size and number of variables, one advantage of the Microcensus over other survey data is that there is a legal obligation to answer truthfully and complete, ensuring the quality of the data. Moreover, in contrast to administrative tax data, the Microcensus covers the whole population, including households at the bottom of the income distribution that do not pay taxes. All of these characteristics make the Microcensus particularly well-suited to study regional income inequality. I thus use the Microcensus to construct a new and unique panel data set of county-level income inequality measures as well as indicators of economic deprivation which I use in Chapters 2, 4, and 5 of this thesis.

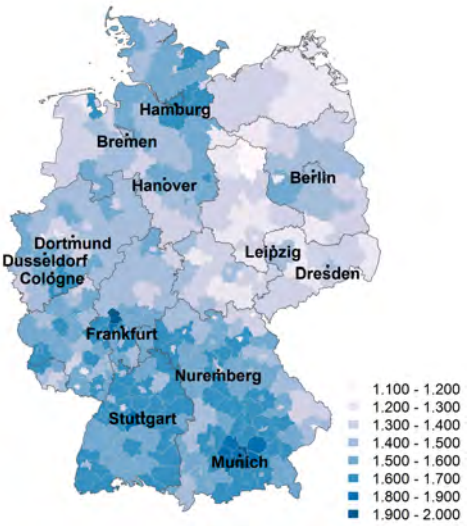
To compute wage inequality measures within and between German counties, I use information on individual daily wages from the Sample of Integrated Labor Market Biographies of the Institute of Employment Research (*Institut für Arbeitsmarktforschung*, IAB). The SIAB is a two percent random sample drawn from administrative social security records. It is representative of all individuals covered by the social security system and thus covers about 80 percent of the German workforce. In the weakly anonymous version of the SIAB the individual's county of residence and place of work is available to the researcher. Therefore, the data are ideal to compute wage inequality measures at the regional level. I use these measures in Chapters 2 and 3.

Figures 1.2 and 1.3 display some of the measures of regional inequality that I employ in the following four chapters of this thesis. Figure 1.2 illustrates income and wage inequality between German counties in 2017. Figure 1.2a presents county medians of disposable household income, Figure 1.2b presents county medians of daily wages. The darker the shade, the higher the median income or wage. The figures reveal that the largest regional differences in Germany still exist between East and West German counties. However, they also show discrepancies between North and South as well as between urban and rural regions.

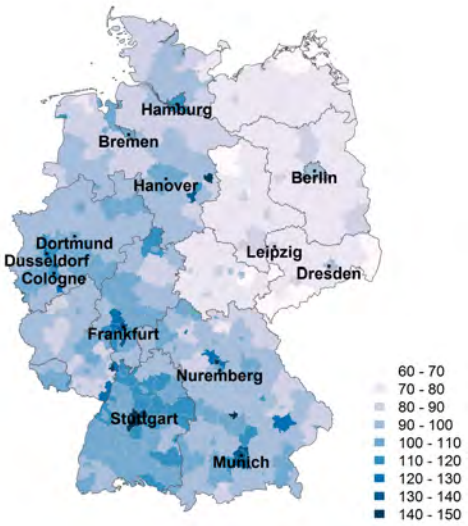
Figure 1.3 illustrates that income and wages do not only vary between but also within German regions. The figure depicts county-level Gini coefficients of disposable household income (Figure 1.3a) and individual daily wages (Figure 1.3b). Again, the darker the shade, the higher the level of inequality within the county. The figures show, that differences in inequality within regions exist between East and West. Gini coefficients of both income and wages are slightly lower in East German counties than in West German counties. Moreover, wage inequality, on average, is higher within urban than within rural counties.

## Figure 1.2: Inequality between German Regions in 2017

(a) Median Household Income



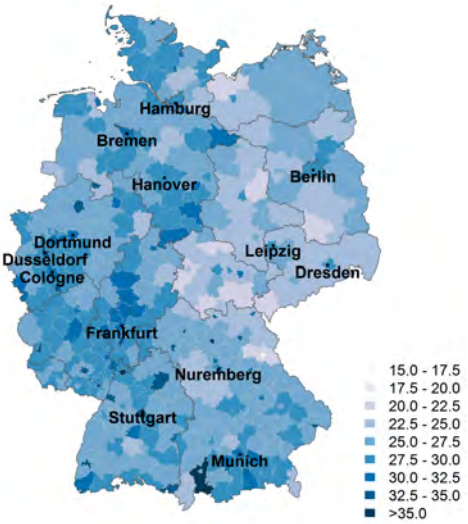

(b) Median Daily Wage



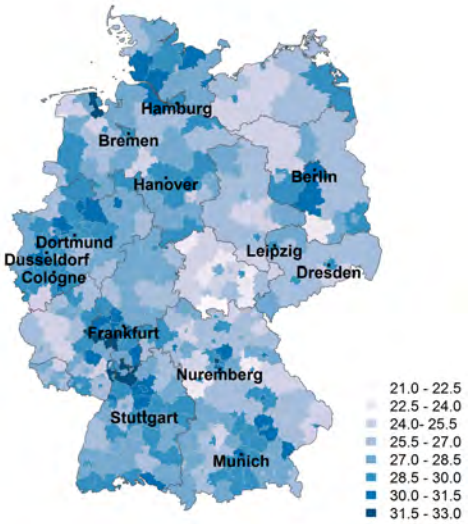

*Notes:* The figures display median net household income and median daily full-time wages across German counties in 2017. Median net household income and median daily full-time wages are measured in Euros.

## Figure 1.3: Inequality within German Regions in 2017

(a) Gini Coefficient of Household Incomes




(b) Gini Coefficient of Daily Wages




*Notes:* The figures display Gini coefficients of net household income and daily full-time wages across German counties in 2017. Gini coefficients are measured between 0 and 100.

In this thesis, I investigate some of the causes and consequences of these regional differences in the distribution of income and wages. In particular, I study the impact of a comprehensive labor market reform on income inequality within German regions (Chapter 2) and analyze

the long-term effect of university openings on the wage distribution in the surrounding area (Chaper 3). In Chapters 4 and 5, I turn to indicators of relative economic deprivation and investigate the political consequences of rising economic deprivation. In the following, I give a brief summary of Chapters 2 to 5. Each chapter is self-contained and can be read independently. A combined bibliography can be found after Chapter 5.

In Chapter 2, I analyze the impact of the so-called Hartz reforms on disposable household income inequality within German counties. The Hartz reforms are a set of four comprehensive labor market reforms that were introduced in Germany between 2003 and 2005 in order to tackle high unemployment and stagnating growth. Inter alia, the reforms deregulated non-standard work, modernized the employment agencies, and cut unemployment benefits for the long-term unemployed. The reforms were highly controversial, sparking street protests across the whole country and ultimately splitting Germany's political left. Among the opponents of the reforms, the Hartz reforms are seen as socially unfair and are believed to have increased inequality in Germany. However, there is very little empirical evidence on the reforms distributional consequences.

I thus ask whether and how the Hartz reforms have increased income inequality in Germany. In order to answer this question, I use county-level inequality measures from the German Microcensus and exploit variation in the intensity German counties were affected by the reforms in a difference-in-differences (DiD) framework. Here, I define a county's treatment intensity according to its labor market performance prior to the introduction of the reforms.

Being the first to provide causal evidence on the distributional impact of the Hartz reforms, I find that the Hartz reforms have led to a small increase in income inequality. Testing possible transmission channels, my results suggest that this increase is partly driven by a mechanical effect of the last Hartz reform, Hartz IV, on the income of households relying on government transfers. Another part of the increase in inequality comes from a rise in the share of households dependent on government transfers. In contrast, I find that neither a rise in female labor supply, in part-time work, or in the number of income earners per household, nor changes in the distribution of full-time wages can account for the increase in income inequality. While not being able to test it directly, I argue that an increase in labor market dualization as a consequence of the Hartz reforms may be able to explain the remaining part of the increase in income inequality.

In Chapter 3, Clemens Fuest and I analyze the long-term effect of university openings on regional wages in West Germany. In many developed countries, universities are increasingly attributed a central role in regional development. This is because the last two decades saw a shift in regional policy thinking from place-based policies offering incentives to individual firms to locate in less favored regions to supporting the endogenous growth of regions by mobilizing its institutional capacities. Yet, there is surprisingly little systematic evidence on the causal impact of universities on regional economic development.

We fill this gap in the literature by exploiting variation in the availability of universities over time and across regions in Germany to analyze the impact of university openings on different percentiles of the regional wage distribution. In order to do so, we combine geo-coded data on German universities with administrative data on individual daily wages from the SIAB and estimate a generalized event study with multiple events. To identify causal effects, we compare counties in whose close vicinity a new university has been opened to counties in whose vicinity no university has been opened.

Our findings suggest that in the long-run, i.e., up to 30 years after a university opening, establishing a new university has a positive effect on wages in the surrounding counties. This effect differs in terms of size and timing along the wage distribution as well as between different employee subgroups. In the lower part of the distribution, establishing a new university takes longer to show effect and wage effects are smaller. In the upper part of the distribution, wage increases occur earlier and are markedly larger. Similarly, while opening a university is positively associated with wages for all employee subgroups, effects on median wages are largest for employees with post-secondary education as well as for employees providing commercial and business-related services.

Chapters 4 and 5 study the political consequences of differences in the regional distribution of income. Chapter 4 is joint work with Florian Dorn, Clemens Fuest, and Florian Neumeier. We turn to indicators of regional economic deprivation and investigate whether relative economic deprivation has an effect on the support for radical right-wing and left-wing parties. The concept of relative economic deprivation suggests that people tend to be more concerned about their relative standing in a society's income distribution rather than their absolute level of income. An unfavorable social comparison is believed to trigger feelings of anxiety and frustration which in turn can foster resentments against the political mainstream as well as the political order itself.

In order to analyze the effect of relative economic deprivation, we compute indicators of economic deprivation that measure the economic deprivation of county's citizens relative to the national average, i.e., the average shortfall from the national median, the poverty line, as well as the poverty rate, using the German Microcensus. We link these indicators to county-level vote shares of radical right- and left-wing parties at German federal elections. Arguably, studying the effect of economic deprivation on voting behavior in Germany constitutes an important advantage over related studies that focus on countries like the U.K. or the U.S. where only a few parties run for election, as Germany's multi-party system facilitates the measurement of political polarization. Moreover, by using data on election outcomes we observe the electorate's revealed support for radical parties rather than stated preferences. This is an advantage over studies relying on survey data.

Using an instrumental variable approach to draw conclusions about causal effects, we find that economic deprivation has a statistically and economically significant effect on the vote share of radical parties. The higher the intensity of economic deprivation in a county, the more

successful are radical parties at the polls. The effect becomes even more pronounced when focusing on the far right Alternative for Germany's (*Alternative für Deutschland*, AfD) votes at the 2017 federal election. Our results thus provide evidence that the prevalence of relative economic deprivation is an important driver of radical voting behavior and may undermine moderate political forces, posing a threat to political stability.

How do local policy makers react to an increase in economic deprivation? This is the question Florian Neumeier, Samina Sultan, and I aim to answer in Chapter 5 by studying the impact of economic deprivation on local fiscal policy. The nature of the relationship between economic deprivation and fiscal policy is a priori ambiguous. On the one hand, as societies become more heterogeneous in terms of income, it also becomes more difficult to agree on the provision of public services and redistributive policies, possibly resulting in a negative relationship between fiscal policy and economic deprivation. On the other hand, an increase in economic deprivation may change redistributional preferences and thus raise the demand for redistribution. This would imply a positive relationship.

In our study, we thus aim to determine the sign of the relationship. In order to estimate the effect of economic deprivation on fiscal policy outcomes at the local level, we exploit the specific institutional setting in Germany which grants local authorities a high degree of fiscal autonomy. Making use of this rich level in local variation, we combine administrative fiscal data on the universe of German city districts (*Kreisfreie Städte*) with our measures of economic deprivation from the German Microcensus. To identify the causal effect of economic deprivation on fiscal policy, we again employ instrumental variable estimation.

Our findings are ambiguous regarding the distributional consequences of economic deprivation. On the one hand, we find that increasing economic deprivation causes local policy makers to increase the local business tax rate and find no effect on the local property tax. Given the fact that the local business tax is generally perceived as progressive, whereas the perception regarding the progressivity of the property tax is ambivalent, we interpret this finding as an attempt of local policy makers to make the tax system more progressive. On the other hand, we find that aggregate spending on local public services is cut as a response to an increase in economic deprivation. Since this effect is driven by spending cuts on welfare, schooling, and sport facilities which mostly benefit lower income groups, our results suggest a negative relationship between economic deprivation and fiscal policies on the expenditure side.

Taken together, my thesis thus adds to our understanding of the causes and consequences of the regional distribution of income and wages. I demonstrate that labor market reforms at the national level can lead to an increase in income inequality within regions and that, in the long-run, establishing a new university has the potential to increase wages along the entire wage distribution in the surrounding area. Moreover, I show that differences in the regional distribution of income can have adverse political consequences. In particular, an increase in economic deprivation relative to the national average can foster resentments against the

political order and lead to growing support of populist and radical movements. In a time when global trends such as digitization, automation, globalization, climate change, migration, or aging are very likely to have highly diverse consequences not only across countries, but also across regions within a country (OECD, 2018), such insights on the causes and consequences of regional inequality are becoming increasingly important.

# 2 The Impact of Labor Market Reforms on Income Inequality: Evidence from the German Hartz Reforms*


## Abstract

In this chapter, I study the distributional consequences of the German Hartz reforms, a set of four comprehensive labor market reforms implemented in Germany between 2003 and 2005. I exploit regional variation in the intensity German counties were affected by the reforms to estimate the causal effect of the Hartz reforms on income inequality. My results suggest that the Hartz reforms have led to a small increase in income inequality. Testing for potential transmission channels, I find that the increase in inequality is partly due to a direct monetary impact of the last Hartz reform, Hartz IV, on the income of households relying on government transfers. Another part can be explained by a rise in the share of transfer recipients. The Hartz reforms did not increase income inequality via an increase in wage inequality nor via an increase in the number of income earners per household.



* I gratefully acknowledge financial support by the Federal Ministry of Education and Research (*Bundesministerium für Bildung und Forschung*, BMBF) through the project 'DFINEQ - Entwicklung von Armut und Ungleichheit in Deutschland und Frankreich in den vergangenen 25 Jahren'.

## 2.1 Introduction

Between 2003 and 2005, Germany introduced a set of four comprehensive labor market reforms, the so-called Hartz reforms, to tackle its high unemployment and stagnating growth. Being considered the most far-reaching reform endeavor in the history of the German welfare state (Jacobi and Kluve, 2007), the Hartz reforms have gained great importance both nationally and internationally. Opinions on the reforms differ widely, though. For some, mostly international observers, the Hartz reforms are one if not the main reason why Germany in under a decade transformed from 'the sick man of Europe' to 'economic superstar'.[1][2] Within Germany, the reforms sparked street protests across the whole country and caused a rift in the Social Democratic Party (*Sozialdemokratische Partei Deutschland*, SPD), which ultimately split Germany's left into the center-left SPD and the more radical and populist Left Party (*Die Linke*). For the protesters and opponents the reforms are socially unfair, put an end to Germany's social market economy, and increased inequality.[3] Put simply, the controversy surrounding the reforms boils down to efficiency vs. equity arguments.

This chapter contributes to this debate by answering the question whether the Hartz reforms have increased income inequality in Germany. While a large literature exists investigating the Hartz reforms' impact on unemployment,[4] so far, little is known about the reforms' distributional consequences. Considering the controversy surrounding the reforms where proponents stress the success of the reforms in reducing unemployment and opponents lament the distributional consequences, reliable evidence on both efficiency and equity is needed.

To answer my research question, I follow Card (1992) and exploit the variation in the intensity German regions were affected by the reforms in a difference-in-differences (DiD) type framework. I define a region's treatment intensity according to its labor market performance before the implementation of the Hartz reforms. Conceptionally, one can consider regions with a relatively poor labor market performance prior to the reforms as the treatment group while regions with a relatively good labor market performance form the control group. For the analysis, I construct a panel data set which combines novel county-level income inequality measures with county-level labor market characteristics. The sample covers the years 1999 to 2014.

I find that the Hartz reforms had a small positive effect on income inequality. This effect is robust to the way treatment is defined (continuously or binarily) and to the labor market indicator used to measure treatment intensity (unemployment rate, long-term unemployment

[1] The sick man of Europe is a term used since the mid-$19^{th}$ century to label a European country experiencing a time of economic difficulty.

[2] See, for instance, Economist (2004), Economist (2013), Dustmann et al. (2014), or Neue Zürcher Zeitung (2013), among others.

[3] See, for example, Frankfurter Rundschau (2014).

[4] See, for example, Krause and Uhlig (2012), Launov and Wälde (2013), or Bradley and Kügler (2019), among others.

rate, or share of social assistance recipients). In the baseline specification, an increase in treatment intensity by one percentage point leads to an increase in the county's Gini coefficient by 0.11 percentage points. In relation to the sample mean, this translates into an increase in the Gini coefficient by 0.40 percent. Looking at the income distribution more closely, I find that it is the second to sixth income deciles which lose in terms of income shares while the highest three deciles gain.

Testing possible transmission channels, my results suggest that the increase in income inequality is partly due to a mechanical effect of the last Hartz reform on the income of households relying on government transfers. Another small part is due to a rise in the share of households relying on transfer payments. In contrast, I find that neither a rise in female labor supply, in part-time work, in the number of earners per household nor changes in the distribution of full-time wages can account for the increase in income inequality. While not being able to test it directly, I argue that an increase in wage inequality between non-standard workers due to increasing labor market dualization may be able to explain the remaining part of the observed increase in income inequality.

This chapter relates to two strands of the literature. First, it relates to a well established literature in labor economics studying the rising inequality in wages and incomes in developed countries.[5] For Germany, these studies find that wage inequality has risen substantially since the 1980s and 1990s. Opinions are divided over whether this rise is attributable to increasing firm heterogeneity and assortativeness in the assignment of workers to firms (Antonczyk et al., 2010; Card et al., 2013), to trade and technological changes, or to changes in labor market institutions including labor market reforms, unionization, and wage setting institutions (Dustmann et al., 2014, 2009).

In contrast to wage inequality, systematic analyses of the evolution and determinants of the income distribution in Germany are rare. A number of studies have documented an increase in income inequality since the late 1990s (e.g. Battisti et al., 2016; Corneo, 2015; Feld and Schmidt, 2016) but only a few contributions adopt a systematic approach to analyze the underlying causes. Using decomposition analyses, Biewen and Juhasz (2012) suggest that 40 to 50 percent of the rise in income inequality between 1999 and 2005 can be accounted for by changes in wages. Building on these results, Biewen et al. (2019) suggest a break in the trend of inequality in 2005, where before 2005 the rise in income inequality can be explained by rising wage inequality while after 2005 an increase in within-year employment opportunities compensated for the rise in wage inequality and did not lead to a further rise in annual labor income inequality. Peichl et al. (2012), on the other hand, attribute the rise in income inequality to changes in household structures and employment behavior rather than wages. Their results suggest that changes in household structures can explain 78 percent of

[5] For Germany see Dustmann et al. (2009), Antonczyk et al. (2010), Fuchs-Schündeln et al. (2010), Peichl et al. (2012), Card et al. (2013), Dustmann et al. (2014), Biewen et al. (2019), Bartels (2019), among others.

the rise in gross income inequality between 1991 and 2007 and 22 percent of the rise in net income inequality.

Second, this chapter relates to studies evaluating the effect of the Hartz reforms. These studies mainly focus on the effect of the Hartz reforms on unemployment and wages and can be categorized into three groups. The first group uses simulations of macroeconomic models calibrated or estimated with pre-reform data (e.g. Bradley and Kügler, 2019; Hartung et al., 2018; Hochmuth et al., 2019; Krause and Uhlig, 2012; Krebs and Scheffel, 2013; Launov and Wälde, 2013, 2016; Scheffel and Krebs, 2017). These papers usually model specific reform features and find declines of unemployment between 0.1 (Launov and Wälde, 2013) and 2.8 (Krause and Uhlig, 2012) percentage points and mixed effects on wages. Launov and Wälde (2013), for instance, employ an equilibrium matching framework and find that the net wage for most skill and regional groups increased. On the other hand, Bradley and Kügler (2019) use a structural labor market model with forward-looking agents and conclude that the Hartz reforms caused a decline in wages which disproportionally affected low-skilled workers.

A second group of papers uses discontinuities or structural breaks to analyze specific reform policies (e.g. Fahr and Sunde, 2009; Hertweck and Sigrist, 2015; Klinger and Rothe, 2012; Price, 2016). These studies indicate small declines in unemployment in response to each of the Hartz policies. Finally, a number of descriptive studies using flow analyses show that labor market stocks and flows changed significantly after the implementation of the Hartz reforms (see Carrillo-Tudela et al., 2018; Rothe and Wälde, 2017).

Carrillo-Tudela et al. (2018), for instance, find that unemployment fell after the introduction of the Hartz reforms because many long-term unemployed deregistered as job seekers. At the same time, many unregistered unemployed workers accepted low-paid part-time work, leading to a rise in labor market participation and part-time work. Part-time work also increased because workers top-up their labor income by taking on mini or midi jobs, introduced by the second Hartz reform, as secondary employment. Overall, Carrillo-Tudela et al. (2018) conclude that the rise in part-time work led to an increase in inequality at the lower end of the distribution, not only in individual earnings inequality but also when considering net and gross household income. To the best of my knowledge, this is the only paper which specifically investigates the development of inequality in the context of the Hartz reforms.

I contribute to this literature in several ways. First, this study is the first to provide causal evidence on the impact of the Hartz reforms on income inequality. While most of the literature evaluating the Hartz reforms focus on unemployment and sometimes wages, I specifically look at disposable household income. Since the most controversial of the four Hartz reforms, Hartz IV, targeted government transfers at the household-level, analyzing the distributional effects on disposable household income (i.e., after government intervention) may help to better understand the heavy opposition against the reforms in the German population.

Second, I test various transmission channels linking the Hartz reforms to inequality. Testing the mechanisms behind the distributional effects of the reforms empirically adds to a literature which, by mostly relying on structural models to evaluate the effects of the reforms, predetermines the transmission channels via the assumptions made.

Third, I contribute methodologically to the literature. Since the Hartz reforms were uniformly and simultaneously introduced across the country, estimating their causal impact is challenging. By using the regional variation in treatment intensity within a DiD framework, I propose an approach which has not been used in the context of the Hartz reforms before but may prove interesting for other researchers.

Fourth, exploiting regional variation in treatment intensity to estimate the causal impact of the Hartz reforms on income inequality is only possible because I construct a new and unique panel data set of county-level income inequality measures using the German Microcensus. I am not aware of any other German data set providing income inequality measures at such a disaggregated level.

The rest of the chapter is organized as follows. In Section 2.2, I describe the institutional background and give an overview of the four Hartz reforms. Section 2.3 explains the empirical strategy and describes the data. Descriptive evidence is presented in Section 2.4. The empirical results are presented in Section 2.5. I test and discuss various transmission channels in Section 2.6. Section 2.7 concludes.

## 2.2 Institutional Background

In spring 2002, after years of rising unemployment, the social-democratic-green government coalition under chancellor Gerhard Schröder appointed a commission composed of 15 experts from industry, politics, and academia to prepare a report on policy reform proposals that would lead to less unemployment. This so-called 'Hartz-commission', named after its chairman Peter Hartz, personnel director at Volkswagen, worked out a program consisting of 13 innovative modules (Hartz, 2002), serving as a blueprint for the four Hartz reform packages or 'Laws for Modern Services in the Labor Market'. The Hartz reforms were implemented in three waves. The first two Hartz Laws (Hartz I and II) were implemented on January $1^{st}$, 2003. Hartz III was implemented on January $1^{st}$, 2004, and Hartz IV came into effect on January $1^{st}$, 2005.

In order to reach their main objectives, that is, accelerating labor market flows and reducing unemployment, the reforms included a modification of active labor market policies (Hartz I and II), the modernization and reorganization of public employment services (Hartz III), as well as a comprehensive reform of the unemployment benefit and social assistance schemes (Hartz IV). Table 2.1 gives an overview of the main policy changes.

Table 2.1: The Four Hartz Laws

| | |
|---|---|
| ***Hartz I and II*** | |
| Implementation: | January $1^{st}$, 2003 |
| Target: | Labor Demand |
| Measures: | • Foundation of 'Staff Service Agencies' (*Personal-Service-Agenturen, PSA*) acting as temporary work agencies for the unemployed<br>• Deregulation of the temporary work sector<br>• Raising of the threshold for incomes exempt from social security contributions ('Minijobs') to 400 Euros per month<br>• Introduction of 'Midijobs' with reduced social security contributions for incomes between 400.01 and 800 Euros<br>• Introduction of 'Me, Inc' (*Ich-AG*), a start-up subsidy for the unemployed |
| ***Hartz III*** | |
| Implementation: | January $1^{st}$, 2004 |
| Target: | Market Efficiency |
| Measures: | • Modernization and reorganization of the public employment agencies, establishing result-based accountability and controlling of local employment offices<br>• Conversion of local employment offices into customer-oriented one-stop-centers<br>• Introduction of a voucher system for placement services (*Vermittlungsgutschein*) and training measures (*Bildungsgutschein*)<br>• Introduction of a standardized profiling process to improve targeting active measures and the allocation of measures and resources |
| ***Hartz IV*** | |
| Implementation: | January $1^{st}$, 2005 |
| Target: | Labor Supply |
| Measures: | • Shortening of the maximum period the unemployed receive earnings-based unemployment insurance benefits, now called unemployment benefits I (*Arbeitslosengeld I*), from 32 month to 12 month<br>• Pooling of the unemployment assistance payments and social assistance payments into the new flat rate unemployment benefits II (*Arbeitslosengeld II*)<br>• Introduction of one-Euro-Jobs (*Ein-Euro-Jobs*) for unemployment benefits II recipients to increase their income with workfare measures in the public sector |

*Notes:* This is a summary of the main policy changes introduced by the Hartz reforms, I do not claim completeness. For further details see Jacobi and Kluve (2007) among others.

In short, Hartz I facilitated temporary employment and introduced new training subsidies while Hartz II regulated marginal employment (so-called 'mini and midi jobs' (*Mini- und Midijobs*)) and sponsored business start-ups by the unemployed (so-called 'Me, Incs' (*Ich-AGs*)). Hartz III restructured the Federal Employment Agency (*Bundesagentur für Arbeit*) and its local employment offices (*Arbeitsagenturen*) with the objective of making them modern and client-oriented service providers (Weise, 2011).

Hartz IV was the most debated and controversial reform package, changing the structure and generosity of unemployment benefits in order to increase work incentives for the unemployed. Before the reform, those who became unemployed received unemployment insurance payments (*Arbeitslosengeld*) covering 60 percent of previous net wages (67 percent for unemployed workers with dependent children) for a maximum duration of 32 months. After having exhausted the short-term benefits, long-term unemployed workers were eligible to

time-unlimited unemployment assistance at a replacement rate of 53 percent (57 percent for workers with dependent children) (*Arbeitslosenhilfe*). Individuals who never contributed to the unemployment insurance scheme received social assistance (*Sozialhilfe*) (Bradley and Kügler, 2019).

The Hartz IV reform shortened the period the person receives unemployment insurance payments, now called unemployment benefit I (*Arbeitslosengeld I*), to six to twelve months, depending on the employment history, and further combined unemployment assistance with social assistance. After the six to twelve months of receiving unemployment benefit I (*Arbeitslosengeld I*), the unemployed person receives a flat-rate unemployment benefit II (*Arbeitslosengeld II*) which is no longer indexed to previous earnings. Individuals deemed capable of working, but who have never contributed to social security receive unemployment benefits II from the beginning. Only those individuals unable to work receive the more generous social assistance (Jacobi and Kluve, 2007).[6]

Given the extensive nature of the Hartz reforms, the reforms have the potential to affect the distribution of income and income inequality via various channels with the direction of the effect being a priori unclear. On the one hand, the reforms may have led to a decrease in income inequality by reducing unemployment. However, the effect also depends on the type of employment the formerly unemployed are able to find. Since the Hartz reforms deregulated non-standard work, the reduction in unemployment may mainly come from an expansion of the low wage sector and an increase in temporary, marginal, and part-time work. If this is the case, it is more probable that the Hartz reforms have led to an increase in income inequality by increasing wage inequality.

Additionally, Hartz IV affected the income of the unemployed directly by combining earnings-based unemployment assistance with social assistance. But again the effect on income inequality is a priori ambiguous as Hartz IV had heterogeneous effects on household income. Some former unemployment assistance recipients are no longer entitled to benefits as benefits are offset by family income. Some receive higher transfers (those with relatively low previous earnings) while others receive lower transfers (those with relatively high previous earnings). Therefore, the question of whether and how the Hartz reforms have affected income inequality can only be answered empirically.

## 2.3 Empirical Strategy

This section describes the empirical strategy to estimate the impact of the Hartz reforms on income inequality. First, I explain the strategy used to identify causal effects. Next, I discuss the empirical model and present the data used for estimation.

[6] For a more detailed description of the individual reform packages see Jacobi and Kluve (2007).

### 2.3.1 Identification Strategy

As the previous section has shown, the Hartz reforms were uniformly and simultaneously introduced in Germany. This absence of legislative variations makes it difficult to estimate the causal impact of the reforms in a quasi-natural experimental set-up. So far, most studies evaluating the reforms have therefore either used simulations of different variants of search and matching models[7] or have relied on reduced form approaches using discontinuities or structural breaks of specific reform policies.[8]

In contrast, I follow Card (1992) and exploit the regional variation in the intensity German counties were affected by the reforms in order to evaluate the overall impact of the four reform packages on income inequality within a DiD framework. Card (1992) was the first to exploit regional variation in a DiD-type set-up. He used regional variation in the fraction of workers affected to measure the effects of an increase in the U.S. federal minimum wage without having to rely on differences in legislation.[9] I adapt his approach to fit the setting of the German Hartz reforms and use the labor market performance of German counties prior to the introduction of the reforms to indicate treatment intensity.

More precisely, I use variation in the county-level unemployment rate of 2002 as treatment indicator. While prior to the reforms unemployment was high in Germany as a whole, there were also considerable regional differences in unemployment rates. Figure 2.1 depicts the unemployment rate across German counties in 2002, i.e., one year before the first reform was introduced. Table 2.2 presents summary statistics.

Table 2.2: County Unemployment Rates in 2002—Summary Statistics

| | (1) All Counties | (2) West Germany | (3) East Germany |
|---|---|---|---|
| N | 365 | 321 | 44 |
| Mean | 8.6 | 7.5 | 16.7 |
| Sd | 3.9 | 2.5 | 3.1 |
| Min | 3.3 | 3.3 | 11 |
| P25 | 5.9 | 5.7 | 14.1 |
| P50 | 7.6 | 7.2 | 16.8 |
| P75 | 10.3 | 8.8 | 19 |
| Max | 22.7 | 16.9 | 22.7 |

*Notes:* The unemployment rate is measured in percent.

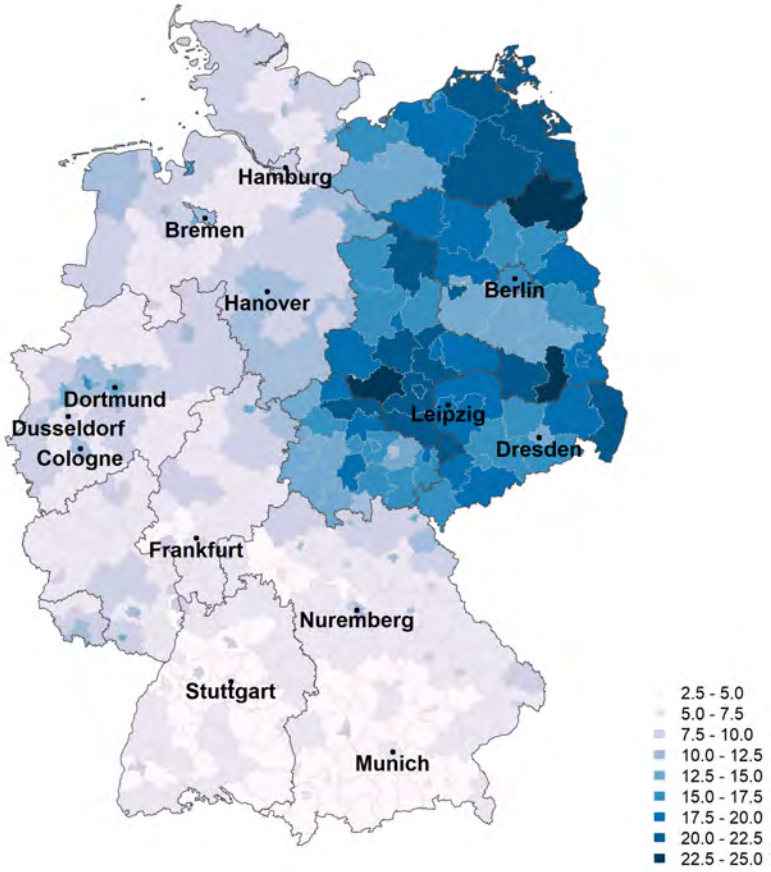
Striking differences in unemployment rates exist between counties. As Figure 2.1 reveals, the largest differences exist between East and West German counties, but there is also substantial

[7] See, for instance, Krause and Uhlig (2012), Krebs and Scheffel (2013), or Launov and Wälde (2013).

[8] See Fahr and Sunde (2009), Hertweck and Sigrist (2015), or Price (2016), among others.

[9] Since Card (1992), the approach has been used in a number of empirical studies, mainly in the minimum wage literature (Caliendo et al., 2018; Dolton et al., 2010; Stewart, 2002, among others) but also to evaluate the effects of health care reforms (Cooper et al., 2011; Gaynor et al., 2013; Propper et al., 2008) or of changes in immigration policies (Clemens et al., 2018).

Figure 2.1: Unemployment Rates across German Counties in 2002




*Notes:* The figure shows the unemployment rate across German counties in 2002. Unemployment rates are measured in percent.

variation within West and East Germany. County unemployment rates in 2002 range from 3.3 percent to 16.9 percent in West Germany and from 11.0 to 22.7 percent in East Germany, the respective standard deviations are 2.5 and 3.1.

I argue that this variation in unemployment rates implies that the Hartz reforms affected counties with different intensities. The higher a county's unemployment rate prior to the introduction of the reforms, the stronger the county is affected. Conceptionally, within a DiD framework, one can think of counties with high unemployment rates in 2002 as 'treated' and of counties with low unemployment as 'untreated'. Since the definition of the treatment indicator is crucial to the identification strategy, I perform several robustness tests, where instead of using the unemployment rate of 2002, I use a county's long-term unemployment rate or its share of social assistance recipients (*Sozialhilfeempfänger*) to define treatment intensity. Figure A2.1 in the appendix shows the variables' regional distribution in 2002, Tables A2.1 and A2.2 in the appendix present summary statistics.

Estimating the effects of the Hartz reforms on income inequality using a DiD approach gives the causal treatment effect only if the identifying assumption, i.e., the parallel trend assumption, holds. The parallel trend assumption requires that the untreated observations provide an appropriate counterfactual of the trend the treated observations would have followed in the absence of treatment. In the context of the Hartz reforms, the parallel trend assumption would be violated if trends in income inequality differed between counties with high and low unemployment rates in 2002.

One concern might be that the different trends in economic outcomes East and West Germany experienced after the German reunification may pose a threat to the validity of the identification strategy. I address this concern by including separate year fixed effects for East and West German counties in my empirical model. I also estimate heterogeneous treatment effects for East and West Germany and restrict my sample to West German counties in a robustness test. Moreover, to assess the plausibility of the parallel trend assumption, I test for differences in trends between treated and untreated counties prior to the introduction of the Hartz reforms, both descriptively (see Section 2.4) and formally (see Section 2.5).

### 2.3.2 Empirical Approach

Stated formally, I estimate the following DiD model in order to identify the impact of the Hartz reforms on income inequality:

$$Y_{ct} = \alpha_c + \beta Treatment_{ct} + \gamma' X_{ct} + \delta_{Region,t} + \epsilon_{ct} \tag{2.1}$$

The index $c$ refers to the county and index $t$ to the year. $\alpha_c$ denotes county fixed effects, $\delta_{Region,t}$ denotes the separate year fixed effects for East and West German counties. $Y_{ct}$ is the outcome variable of interest, i.e., a measure of income inequality. For the main part of the analysis, I use the county's Gini coefficient but also employ income shares by deciles of the income distribution. The vector $X_{ct}$ includes a set of demographic control variables at the county-level, namely the county's age structure, the share of females, the share of foreigners, as well as population density. $Treatment_{ct}$ is the continuous treatment indicator and is defined as

$$Treatment_{ct} = Post_t \times Treatment\ Indicator_{c,2002} \tag{2.2}$$

where $Treatment\ Indicator_{c,2002}$ is either the unemployment rate, the long-term unemployment rate, or the share of social assistance recipients in 2002 and $Post_t$ is a dummy variable equal to one if $t \geq 2006$ and equal to zero if $t \leq 2002$. The years 2003 to 2005 are excluded from the DiD analysis.

I am interested in the size of $\beta$, the treatment effect. As already mentioned, $\beta$ only has a causal interpretation if the parallel trend assumption holds.[10]

In order to asses the plausibility of the parallel trend assumption, I augment Equation 2.1 with leading values of the treatment indicator, i.e., $Treatment^{j}_{ct}$ for $j < 2003$, to test for pre-treatment trends. In addition, I also include a number of lagged treatment variables to study potentially time-varying effects, $\tilde{\beta}_j$. Following Schmidheiny and Siegloch (2019), I thus set up the following event-study model:

$$Y_{ct} = \tilde{\alpha}_c + \sum_{j=1999}^{2014} \tilde{\beta}_j Treatment^{j}_{ct} + \tilde{\gamma}' X_{ct} + \tilde{\delta}_{Region,t} + \tilde{\epsilon}_{ct} \tag{2.3}$$

where in order to standardize $\tilde{\beta}_{2003}$ to zero, I drop $Treatment^{2003}_{ct}$ from the regression.

### 2.3.3 Data Description

To estimate the models described above, I construct a panel data set which combines county-level inequality measures with labor market characteristics as well as demographic control variables. My data cover the years 1999 to 2014.[11] To construct the data, I mainly rely on information on household income from the German Microcensus. Labor market characteristics as well as further control variables come from the Statistical Offices of the Federation and the Länder (*Statistische Ämter des Bundes und der Länder*) or from the Federal Institute for Research on Building, Urban Affairs and Spatial Developments (*Bundesinstitut für Bau-, Stadt-, und Raumforschung, BBSR*).

#### Income Inequality

I compute county-level income inequality measures using information on monthly net household income available in the German Microcensus. The Microcensus is a representative household survey of one percent of the German population. It is carried out annually by the statistical offices of the German states (*Statistische Landesämter*) and administered by the Federal Statistical Office (*Statistisches Bundesamt*). It contains information on various demographic characteristics including the county of residence, employment status, household size, the

[10] Note that Germany introduced a comprehensive tax reform in 2001, which was phased in between 2001 and 2005. If the regional impact of the tax reform is correlated with both, county inequality and labor market performance in 2002, $\beta$ is biased. Unfortunately, I am not able to disentangle the effect of the Hartz reform and the effect of the tax reform on regional income distributions. Since the tax reforms only affected taxpayers, county unemployment rates and the regional impact of the tax reforms may be negatively correlated. If so, I most likely underestimate the size of $\beta$.

[11] Germany introduced a federal minimum wage in 2015, mainly as a response to the Hartz reforms. I therefore stop the sample in 2014 to ensure the introduction does not bias my results.

age of all household members, and monthly net household income, among others. Its large sample size allows to compute income inequality measures at the county-level.

Since the income variable in the Microcensus is interval-censored, i.e., respondents only indicate the income class they are in rather than their precise income, I impute a continuous income figure for each household via interval regression. Including the information on a household's income class and making use of various socio-demographic characteristics, this imputation technique ensures that the empirical distribution of the continuous income variable fits the shape of the distribution of the income classes. As a result, I obtain a single income figure for each household that is consistent with the observed income limits (Royston, 2008).

Having obtained continuous household income, I compute several measures of income inequality at the county-level, namely the Gini coefficient as well as income shares per decile of the income distribution. Note that household incomes are equivalized according to the new OECD equivalence scale to account for differences in household size and adjusted for price changes using the German consumer price index.

Due to several territorial reforms where neighboring counties were merged, the number of counties in the German Microcensus varies across years, resulting in a slightly unbalanced panel data set. The most comprehensive territorial reforms were implemented in East German states where county unemployment rates and thus treatment intensities are higher (see Figure 2.1). Consequently, the number of 'treated' counties declines over time when using the unbalanced panel. For this reason, I restrict the sample to the 365 German counties unaffected by territorial reforms in the main analysis, ensuring that the number of 'treated' and 'untreated' counties remains constant. I use the unbalanced sample in a robustness test (see Section 2.5.2).

**Labor Market Characteristics and Control Variables**

For my main analysis, I use a county's unemployment rate of 2002 as a measure of the intensity a county was affected by the Hartz reforms. As a robustness test, I also look at a county's long-term unemployment rate, calculated as the product of the unemployment rate and the share of the unemployed which have been unemployed for more than a year, as well as a county's share of social assistance recipients in the population. Data on county-level unemployment rates are collected by the BBSR and made available via its online database. The number of social assistance recipients as well as population by county are made available by the Statistical Offices of the Federation and the Länder (*Statistische Ämter des Bundes und der Länder*).

Furthermore, I include several control variables depicting the demographic situation in a county. I control for a county's age structure, the share of females, the share of foreigners, as well as a county's population density. County-level age shares are calculated using the

German Microcensus. The share of females and the share of foreigners is made available by the Statistical Offices of the Federation and the Länder, population densities come from BBSR.

## 2.4 Descriptive Statistics

Before presenting the empirical results in the next section, I first take a closer look at the composition of the 'treatment' and 'control' group. Since I use a continuous treatment indicator, I have no treatment and control group in the traditional sense. Therefore, I sort all considered counties into one of three groups—low, medium, and high treatment intensity—according to their unemployment rate in 2002. To have three equally sized groups, I set the cutoff at the $33^{rd}$ and the $67^{th}$ percentile. Table 2.3 shows the distribution of covariates one year prior to the first reform, i.e., in 2002.

Table 2.3: The Distribution of Covariates in 2002

| | Low mean | Medium mean | High mean |
|---|---|---|---|
| ***Treatment Indicator:*** | | | |
| Unemployment Rate | 5.2 | 7.6 | 13.1 |
| ***Inequality Measures:*** | | | |
| Gini | 27.9 | 28.0 | 26.7 |
| Income Share 1. Decile | 3.5 | 3.5 | 3.7 |
| Income Share 2. Decile | 5.4 | 5.3 | 5.5 |
| Income Share 3. Decile | 6.5 | 6.4 | 6.6 |
| Income Share 4. Decile | 7.4 | 7.4 | 7.6 |
| Income Share 5. Decile | 8.4 | 8.4 | 8.6 |
| Income Share 6. Decile | 9.4 | 9.4 | 9.5 |
| Income Share 7. Decile | 10.6 | 10.6 | 10.7 |
| Income Share 8. Decile | 12.1 | 12.1 | 12.1 |
| Income Share 9. Decile. | 14.6 | 14.6 | 14.3 |
| Income Share 10. Decile | 22.3 | 22.3 | 21.5 |
| ***Control Variables:*** | | | |
| Share of Females | 50.8 | 51.2 | 51.2 |
| Share of Foreigners | 8.3 | 8.0 | 7.2 |
| Age Share 25 to 34 | 12.4 | 12.4 | 12.1 |
| Age Share 35 to 44 | 16.4 | 16.1 | 16.0 |
| Age Share 45 to 54 | 13.7 | 13.4 | 14.0 |
| Age Share 55 to 64 | 12.6 | 12.8 | 13.5 |
| Age Share over 65 | 17.1 | 18.6 | 19.0 |
| Population Density | 313.0 | 501.3 | 817.8 |
| East German | 0.0 | 0.0 | 36.4 |
| Observations | 124 | 120 | 121 |

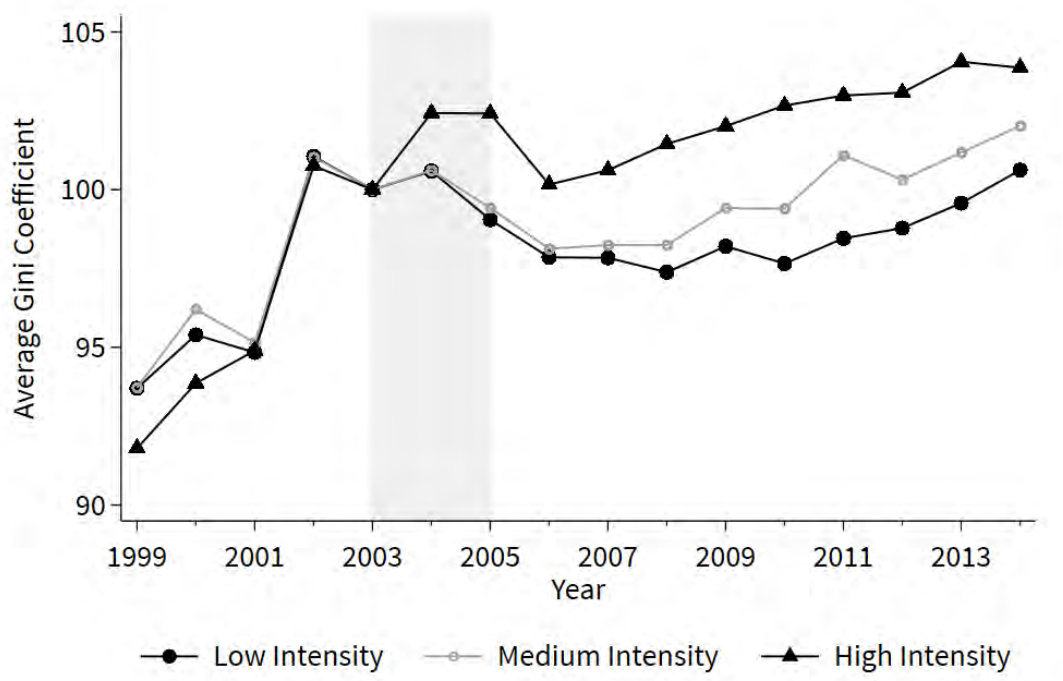
*Notes:* The unemployment rate, all population shares and the share of East German counties are measured in percent, the Gini coefficient lies between 0 and 100, population density is defines as population/$km^2$.

Counties with high treatment intensity have on average a lower level of income inequality, a lower share of foreigners, as well as a lower share of the population aged below 45 and a higher share of the population aged above 45. These differences are due to the fact that all of the 44 East German counties in the sample are in the high treatment intensity group. Generally, incomes in East Germany are more equally distributed. Similarly, the share of

foreigners is lower and the share of the older population is higher in the East. This highlights the importance to control for region-year fixed effects in the upcoming analysis as well as to analyze heterogeneous effects.

While looking at the composition of the different groups gives important insights, what matters for the validity of the DiD design is that in absence of treatment, the groups would have followed a parallel trend. To examine the parallel trend assumption visually, I plot the trend in average Gini coefficient by treatment intensity group, normalizing Gini coefficients (2003=100). Figure 2.2 shows that the identifying assumption is likely to hold as the average Gini coefficients of the three groups move in parallel before 2003.[12]

Figure 2.2: Visualization of the Parallel Trend Assumption




*Notes:* The figure shows the trend of the average Gini coefficients by treatment intensity group from 1999 to 2014. Gini coefficients are normalized to 2003=100.

Besides visualizing the parallel trend assumption, Figure 2.2 also provides first descriptive evidence that the Hartz reforms have increased income inequality. After the introduction of the reforms, the trends in Gini coefficients of the three treatment intensity groups begin to

[12] The jump in the average Gini coefficients between 2001 and 2002 is quite surprising. Potential explanations include a tax reform introduced in 2001 as well as the fact that between 2001 and 2002 income classes in the German Microcensus were adapted, due to the introduction of the Euro. I am not aware of any other structural break in the Microcensus data between 2001 and 2002. At any rate, I observe this jump in inequality for both regional and national inequality measures. Therefore, is is accounted for in the upcoming analyses via the year fixed effects.

diverge. Hereby, the increase in average Gini coefficients is largest for the high treatment intensity group. Figure A2.2 in the appendix visualizes the parallel trend assumption using the long-term unemployment rate and the share of social assistance recipients in 2002 as treatment indicator. The conclusions drawn remain the same.

## 2.5 Empirical Results

In this section, I present the empirical results. I start by presenting my main results, i.e., the impact of the Hartz reforms on different county-level income inequality measures, namely the Gini coefficient as well as income shares by decile of the income distribution. Next, I test the robustness of the results with respect to the definition of treatment as well as sample selection. I extend the analysis by estimating heterogeneous treatment effects between East and West German counties as well as between rural and urban counties. Besides presenting the pooled DiD treatment effects, I provide event study results for every specification in order to ascertain the plausibility of the identifying assumption.

### 2.5.1 Main Results

Table 2.4 presents the pooled DiD treatment effect on county Gini coefficients using the continuous unemployment rate of 2002 as treatment indicator. Columns (1) to (3) present the estimates for different model specifications. The models in columns (1) and (2) include only year or region-year fixed effects. Column (3) presents the estimates of the fully-specified model, which in addition to region-year fixed effects also controls for demographic characteristics at the county-level.

The results suggest that the reforms have increased income inequality in Germany. Independent of the specification, albeit small, the treatment effect is positive and statistically significant at the one percent level. Coefficients become smaller once I include region-year fixed effects and further decrease when adding demographic control variables to the regression. Overall, the coefficients are rather stable, however, ranging between 0.11 to 0.15. In the fully-specified model, the treatment effect is 0.11. The estimate indicates that an increase in treatment intensity by one percentage point is associated with an increase in the Gini coefficient by 0.11 percentage points. This implies an increase by 4.2 percent in terms of its standard deviation. In relation to the sample mean, the effect translate into an increase of the Gini coefficient by 0.40 percent.

Figure 2.3 illustrates the event study results for the same three specifications, taking 2003 as reference point. Table A2.3 in the appendix presents the same results in table format. The figure reveals that, as in the DiD model, coefficients become smaller once I include region-year fixed effects. Coefficients are also rather stable over time. Most importantly though, Figure 2.3 illustrates that all pre-treatment effects are statistically insignificant, independent of the model specification. Prior to the Hartz reforms, counties with low and high unemployment

Table 2.4: The Hartz Reforms and Inequality—DiD

| | (1) Gini | (2) Gini | (3) Gini |
|---|---|---|---|
| Treatment | 0.148*** | 0.125*** | 0.106*** |
| | [0.000] | [0.000] | [0.001] |
| Share of Females | | | -0.023 |
| | | | [0.889] |
| Share of Foreigners | | | -0.091* |
| | | | [0.079] |
| Age Share 25 to 34 | | | -0.018 |
| | | | [0.591] |
| Age Share 35 to 44 | | | -0.069** |
| | | | [0.034] |
| Age Share 45 to 54 | | | -0.101*** |
| | | | [0.000] |
| Age Share 55 to 64 | | | -0.057** |
| | | | [0.036] |
| Age Share over 65 | | | -0.087*** |
| | | | [0.000] |
| Population Density | | | -0.001 |
| | | | [0.546] |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | No | No |
| Region-Year FE | No | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.98 | 26.98 |
| SD Dep. Variable | 2.51 | 2.51 | 2.51 |
| Mean Treatment Ind. | 8.60 | 8.60 | 8.60 |
| $R^2$ | 0.306 | 0.310 | 0.323 |
| N | 4745 | 4745 | 4745 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

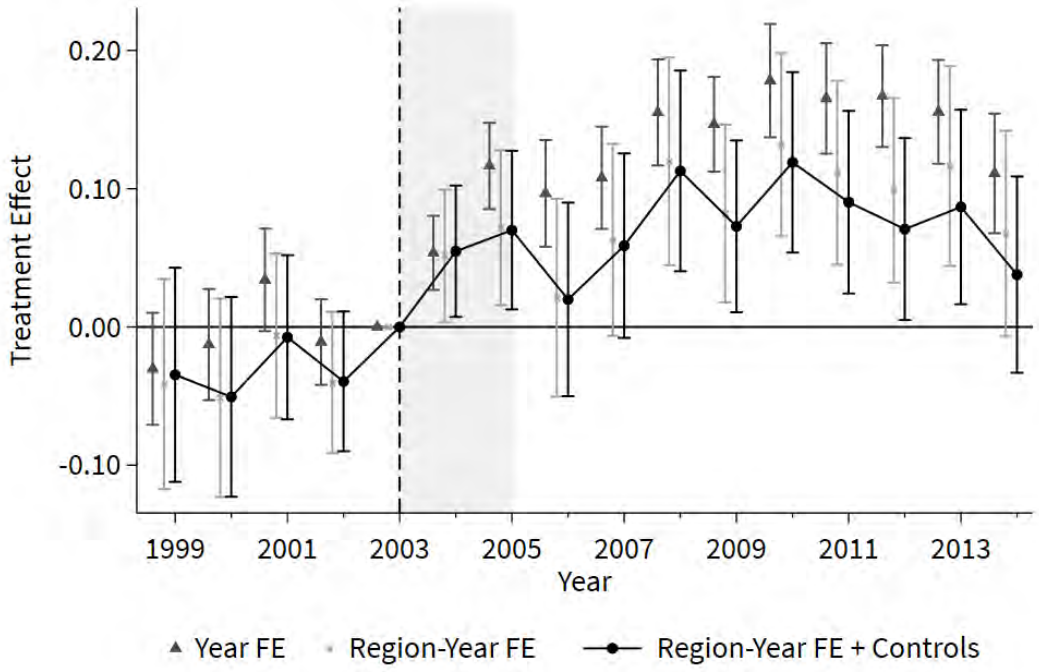
rates in 2002 followed the same trend. I therefore conclude that the parallel trend assumption holds and that the identification strategy and my research design are valid.

The Gini coefficient is the most common summary measure of inequality. It summarizes the whole income distribution into one single number and therefore has an intuitive interpretation. Nevertheless, it also has its drawbacks since summarizing the whole income distribution loses information. To get a more detailed picture of the effects of the Hartz reforms on the income distribution, I complement my analysis by estimating the treatment effects on income shares per decile of the income distribution. Figure 2.4 summarizes the results.[13]

The figure reveals that the overall effect on income inequality comes from the Hartz reforms' adverse effects on lower and middle incomes. While the treatment effect on the income share of the first decile is statistically insignificant, estimates are negative and statistically different from zero for deciles two to six. Thereby, the third decile incurs the highest loss in its income share. As a consequence of the decrease in income shares in the lower and middle part of the distribution, income shares of the upper three deciles increase.

[13] See Table A2.4 in the appendix presents the results in table format. Event study results are provided in Figure A2.3.

Figure 2.3: The Hartz Reforms and Inequality—Event Study




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

### 2.5.2 Robustness Test

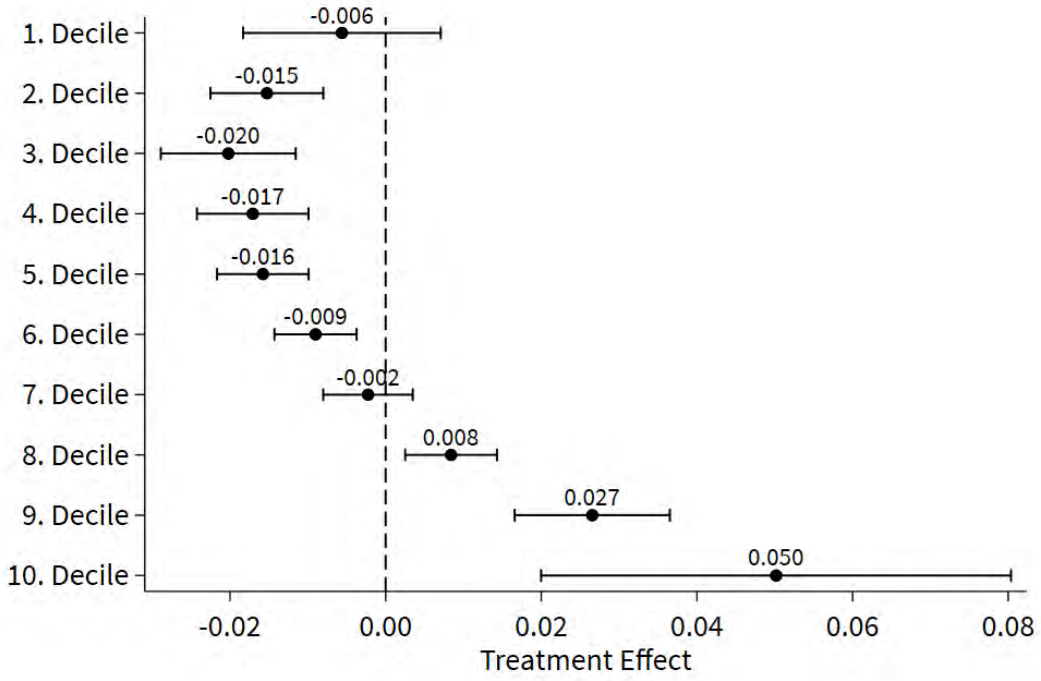
To test the robustness of my results, I undertake a wide set of additional analyses. Since the definition of treatment is crucial to my identification strategy, I modify the empirical specification by using different definitions of treatment (continuous and binary) as well as different treatment indicators (long-term unemployment rate and share of social assistance recipients). In addition, I test how results are affected by sample selection. Note, that for all robustness tests I estimate the effects of the Hartz reform on the Gini coefficient and use the fully specified model, controlling for region-year fixed effects and demographic characteristics.

#### Alternative Treatment Definitions

Treatment and treatment intensity of the Hartz reforms can be defined in multiple ways. So far, I have used the continuous unemployment rate of 2002 as treatment indicator. In the following, I test whether and how the results change, when I alter the definition of treatment. I do this in two ways.

First, instead of using the continuous unemployment rate of 2002, I use the winsorized continuous unemployment rate as treatment indicator to ensure that my results are not driven by outliers. In addition to winsorizing, I also derive three binary treatment indicators by

Figure 2.4: Income Shares by Decile




*Notes:* The figure plots the DiD results for the ten income shares. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

sorting counties into a treatment and control group according to the distribution of the unemployment rate in 2002. The *Binary 50%* treatment variable uses the median of the 2002 unemployment rate distribution as the cut-off and classifies the upper half as the treatment group and the lower half as the control group. The *Binary 33%* and *Binary 25%* treatment variables change the cut-off to the upper and lower third or quarter of the distribution, respectively. Consequently, a county's exact level of the unemployment rate is no longer decisive for the estimation, but rather the distinction between low- and high unemployment counties.[14]

Second, I test the robustness of my results with respect to the choice of treatment indicator. That is, instead of the unemployment rate, I use the long-term unemployment rate as well as the share of social assistance recipients of 2002 to indicate treatment. Since the last and most controversial reform, Hartz IV, directly targeted the long-term unemployed by shortening the period the unemployed received earnings-based unemployment insurance and by combining the unemployment assistance with social assistance, it is interesting to examine whether and how the results change when either the long-term unemployment rate or the share of social

[14] Note, that as a consequence, the *Binary 50%* treatment variable uses all observations in the sample, while the *Binary 33%* (*Binary 25%*) uses only the upper and lower third (quarter) of the unemployment rate distribution and thus loses 33 percent (50 percent) of observations.

assistance recipients are used as alternative treatment indicators.[15] Again, I define treatment continuously as well as binarily.

Table 2.5: Alternative Treatment Definitions

**Panel A: Unemployment Rate**

| | (1) Baseline | (2) Winsorized | (3) Binary 50% | (4) Binary 33% | (5) Binary 25% |
|---|---|---|---|---|---|
| Treatment | 0.106*** | 0.114*** | 0.399*** | 0.694*** | 0.622** |
| | [0.001] | [0.001] | [0.007] | [0.001] | [0.015] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.98 | 26.98 | 26.78 | 26.59 |
| SD Dep. Variable | 2.51 | 2.51 | 2.51 | 2.53 | 2.54 |
| Mean Treatment Ind. | 8.60 | 8.49 | | | |
| △ Mean Treatment Ind. | | | 5.68 | 7.86 | 9.40 |
| $R^2$ | 0.323 | 0.323 | 0.319 | 0.356 | 0.383 |
| N | 4745 | 4745 | 4745 | 3185 | 2392 |

**Panel B: Long-term Unemployment Rate**

| | (1) Continuous | (2) Winsorized | (3) Binary 50% | (4) Binary 33% | (5) Binary 25% |
|---|---|---|---|---|---|
| Treatment | 0.208*** | 0.229*** | 0.346** | 0.714*** | 0.697*** |
| | [0.000] | [0.000] | [0.019] | [0.000] | [0.003] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.98 | 26.98 | 26.76 | 26.57 |
| SD Dep. Variable | 2.51 | 2.51 | 2.51 | 2.46 | 2.46 |
| Mean Treatment Ind. | 2.73 | | | | |
| △ Mean Treatment Ind. | | | 2.58 | 3.58 | 4.23 |
| $R^2$ | 0.324 | 0.323 | 0.319 | 0.351 | 0.375 |
| N | 4745 | 4745 | 4745 | 3159 | 2379 |

**Panel B: Share of Social Assistance Recipients**

| | (1) Continuous | (2) Winsorized | (3) Binary 50% | (4) Binary 33% | (5) Binary 25% |
|---|---|---|---|---|---|
| Treatment | 0.101*** | 0.129*** | 0.316** | 0.468*** | 0.421** |
| | [0.010] | [0.005] | [0.030] | [0.007] | [0.031] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.98 | 26.98 | 27.06 | 27.19 |
| SD Dep. Variable | 2.51 | 2.51 | 2.51 | 2.50 | 2.46 |
| Mean Treatment Ind. | 2.79 | | | | |
| △ Mean Treatment Ind. | | | 2.40 | 3.29 | 3.84 |
| $R^2$ | 0.316 | 0.317 | 0.316 | 0.334 | 0.327 |
| N | 4719 | 4719 | 4719 | 3146 | 2353 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

[15] For consistency reasons, I define the long-term unemployment rate as the share of the long-term unemployed among the working population. Alternatively, one could also look at the share of the long-term unemployed among all unemployed. DiD and event study results when using the share of the long-term unemployed among all unemployed as treatment indicator are presented in Table A2.5 and Figure A2.7 in the appendix.

Table 2.5 presents the results. Panel A of Table 2.5 shows the results for the winsorized and binary treatment variables when using the unemployment rate of 2002 as treatment indicator. To facilitate comparisons, column (1) shows the baseline results from column (3) in Table 2.4. Column (2) presents the estimate for the winsorized treatment variable, columns (3) to (5) present the results for the binary treatment variables. Panel B and Panel C of Table 2.5 show the results when treatment is based on the long-term unemployment rate and the share of social assistance recipients, respectively. In both cases, column (1) presents the results for the continuous treatment variable, column (2) presents the results for the winsorized treatment variable, and columns (3) to (5) present the results for the three binary treatment variables.

The results suggest that the effect of the Hartz reforms on income inequality is robust to the way treatment is defined. In all specifications, treatment effects are statistically significant and positive. I find that winsorizing the treatment variable hardly changes the results. In contrast, point estimates become larger when treatment is defined binarily rather than continuously (see Panel A). Treatment effects for the binary treatment variables range between 0.40 and 0.62. However, one has to keep in mind, that the continuous treatment variable measures the effect of an increase in the unemployment rate by one percentage point while the differences in the average unemployment rate of 2002 between treatment and control group when treatment is defined binarily are much larger.[16] Dividing the point estimates in columns (3), (4), and (5) in Panel A by the respective differences in mean treatment indicators to roughly approximate the effect of a one percentage point increase in treatment intensity yields effects between 0.06 and 0.09. This is in the same order of magnitude as the baseline effect.

Panel B reveals that the effect of the Hartz reforms on inequality is about twice as high when using the long-term unemployment rate as treatment indicator instead of the unemployment rate. An increase in the continuous (winsorized) long-term unemployment rate by one percentage point increases the Gini coefficient by 0.21 (0.23) percentage points. When instead treatment is defined binarily, the point estimates are 0.35, 0.71, and 0.70 depending on which binary treatment variable is used. Adjusting the coefficients to take into account the differences in the average long-term unemployment rates between treatment and control group to approximate a one percentage point increase yields effects between 0.13 and 0.20. Since long-term unemployment rates are about three times smaller than unemployment rates, the higher estimates are expected.

Turning to Panel C, the results indicate that a one percentage point increase in the continuous (winsorized) share of social assistance recipients in 2002 leads to an increase in the Gini coefficient by 0.10 (0.13) percentage points. When treatment is defined binarily, the point estimates range between 0.32 and 0.47. Adjusted for the difference in the average share of social assistance recipients between treatment and control group, the effects lie between 0.11 and 0.14.

[16] The differences between the mean treatment indicator for treated and untreated counties are 5.68, 7.86, and 9.40 for the *Binary 50%*, *Binary 33%*, and *Binary 25%* treatment indicator, respectively (cf. Table 2.5).

Taken together, the results demonstrate that the effect of the Hartz reforms on income inequality in Germany is robust to the definition of treatment. Moreover, the event study results show that the common trend assumption holds for all treatment indicators and all specifications (see Figures A2.4, A2.5, and A2.6 in the appendix).

### Sample Selection

In a final robustness check, I test how sample selection influences the results. One concern with using data from 1999 to 2014 in the DiD model might be that the pooled DiD treatment effect masks the fact that the financial crisis of 2008 and the subsequent economic crisis may confound the results. To the extent that the effect of the financial and economic crisis on a county's income distribution is related to its prior labor market performance, estimates will be biased. However, restricting the sample to include only the years 1999 to 2008 shows that the effects are stable over time (see columns (1) and (2) in Table 2.6).

Table 2.6: Sample Selection

| | (1) Baseline | (2) 1999–2008 | (3) Unbalanced | (4) West Germany |
|---|---|---|---|---|
| Treatment | 0.106*** | 0.098*** | 0.092*** | 0.120*** |
| | [0.000] | [0.003] | [0.000] | [0.001] |
| Demographic Controls | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.53 | 26.82 | 27.44 |
| SD Dep. Variable | 2.51 | 2.47 | 2.74 | 2.34 |
| Mean Treatment Ind. | 8.60 | 8.60 | 10.27 | 7.49 |
| $R^2$ | 0.323 | 0.344 | 0.310 | 0.251 |
| N | 4745 | 2555 | 6366 | 5136 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

Another source of concern may be that so far I have used a balanced sample, dropping all county observations that were subject to territorial reforms between 1999 and 2014. Since almost all territorial reforms were implemented in East German states, this implies that I lose disproportionally many East German observations (see also Section 2.3.3). I therefore re-estimate Equation 2.1 using the unbalanced sample as well as a sample including only West German counties. Columns (3) and (4) of Table 2.6 display the results. In comparison to the baseline estimate, the estimate for the unbalanced sample is just slightly smaller, whereas the estimate for the West German sample is just slightly larger.

## 2.5.3 Heterogeneity

The descriptive results in Section 2.4 established considerable level-differences in the treatment indicator between East and West German counties, highlighting the importance to analyze heterogeneous treatment effects. Moreover, the previous section has already shown that the treatment effect is somewhat smaller in the unbalanced sample which includes

more East German counties and somewhat larger in the West German sample, hinting at the presence of heterogeneous treatment effects. I test this formally by adjusting Equation 2.1 to include an interaction term between the treatment variable and two dummy variables for East and West German counties, respectively:

$$Y_{ct} = \alpha_c + \beta_E East{\times}Treatment_{ct} + \beta_W West{\times}Treatment_{ct} + \gamma^{'} X_{ct} + \delta_{Region,t} + \epsilon_{ct} \quad (2.4)$$

Column (2) in Table 2.7 displays the results. As already suspected, treatment effects differ between East and West German counties. In fact, the effect of the Hartz reforms on income inequality seems to be entirely driven by West Germany. Here, a one percentage point increase in treatment intensity leads to a rise in the Gini coefficient by 0.12 percentage points. The effect is statistically significant at the one percent level. In contrast, the Hartz reforms do not seem to have had an impact on income inequality in East German counties. Here, coefficients are smaller and statistically insignificant.

Table 2.7: Heterogeneous Treatment Effects

| | (1) Baseline | (2) East/ West | (3) Rural/ Urban |
|---|---|---|---|
| Treatment | 0.106*** | | |
| | [0.001] | | |
| West × Treatment | | 0.120*** | |
| | | [0.001] | |
| East × Treatment | | 0.051 | |
| | | [0.407] | |
| Rural × Treatment | | | 0.099*** |
| | | | [0.002] |
| Urban × Treatment | | | 0.113*** |
| | | | [0.001] |
| Demographic Controls | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | | |
| Mean Treatment Ind. | 8.60 | | |
| Mean Dep. Variable × East | | 24.56 | |
| Mean Dep. Variable × West | | 27.41 | |
| Mean Treatment Ind. × East | | 16.70 | |
| Mean Treatment Ind. × West | | 7.49 | |
| Mean Dep. Variable × Urban | | | 27.78 |
| Mean Dep. Variable × Rural | | | 26.28 |
| Mean Treatment Ind. × Urban | | | 8.04 |
| Mean Treatment Ind. × Rural | | | 9.22 |
| $R^2$ | 0.323 | 0.324 | 0.324 |
| N | 4745 | 4745 | 4745 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

Besides analyzing heterogeneous treatment effects for East and West German counties, I also test for heterogeneity between rural and urban counties. Column (3) of Table 2.7 presents the results. The difference in the treatment effect between rural and urban counties is small. An

increase in treatment intensity by one percentage point increases the Gini coefficients in rural counties by 0.10 percentages points. The treatment effect on urban counties is 0.11.[17]

To summarize the results so far: I have established that the Hartz reforms had a small but positive and statistically significant effect on income inequality. In the baseline result, a one percentage point increase in treatment intensity increases a county's Gini coefficient by 0.11 percentage points. This effect is robust to the way treatment is defined, the treatment indicator used, as well as sample selection. Looking at the income distribution more closely, I find that lower and middle income deciles lose while upper income deciles gain in income shares. These estimated treatment effects have causal interpretations. Applying an event study approach, I find that the parallel trend assumption holds in all specifications. Interestingly, the analysis also reveals that the effects are driven by West German counties only. I find no statistically significant effect of the Hartz reforms in income inequality for East German counties.

## 2.6 Transmission Channels

Where does the increase in disposable household income inequality come from? Due to the extensive nature of the Hartz reforms, targeting labor demand, market efficiency, as well as labor supply, the reforms may have affected the income distribution via various channels. In the following, I will discuss and/or test these mechanisms, namely changes to the generosity of the transfer system (i.e., redistribution), changes in the composition of the working population or within households, an increase in (full-time) wage inequality, as well as increasing dualization of the labor market.

### 2.6.1 Redistribution

The last reform package, Hartz IV, overhauled the German transfer and welfare system. These changes in redistribution had heterogeneous effects on the income of transfer recipients. Using household-level microdata from the Income and Consumption Survey (*Einkommens- und Verbrauchsstichprobe*, EVS) of 2003, Blos and Rudolph (2005) simulate the effect of the introduction of Hartz IV on household income. They show that for social assistance recipients the implementation of Hartz IV had hardly any impact. In contrast, 17 percent of former unemployment assistance recipients are now no longer entitled to benefits. This is because the new unemployment benefits are offset by family income. Of the 83 percent of households still entitled to benefits, 47 percent receive higher transfer payments. These are households which before entering unemployment had relatively low earnings. The other half, i.e., households with relatively high previous earnings, receive lower transfers.

To be able to judge whether and to which extent these mechanical or redistributional effects explain the increase in income inequality, I estimate the effect of the Hartz reforms on income

[17] Event study results are presented in Figure A2.8 in the appendix.

inequality excluding all households relying on government transfer payments. If the observed increase in inequality is partly caused by the mechanical effect on the income of households relying on government transfers, excluding these households and re-estimating Equation 2.1 should yield smaller treatment effects on income inequality. In order to do so, I use the Microcensus and re-calculate the county-level Gini coefficients, dropping all households which state government transfers as their main income source. Since excluding transfer recipients does not only account for the mechanical effect but also accounts for changes in the share of households relying on transfer payments, I include the latter as an additional control variable in the regression.

The DiD treatment effects when using either the unemployment rate, the long-term unemployment rate, or the share of social assistance recipients as treatment indicators are presented in columns (2), (4), and (6) of Table 2.8. To facilitate the comparison, columns (1), (3), and (5) present the effect on household income inequality including all households ('Baseline').

Table 2.8: Inequality Excl. Transfer Recipients

| | Unemployment | | Long-term Unemployment | | Social Assistance | |
|---|---|---|---|---|---|---|
| | (1) Baseline | (2) w/o Transfer | (3) Baseline | (4) w/o Transfer | (5) Baseline | (6) w/o Transfer |
| Treatment | 0.106*** | 0.065** | 0.208*** | 0.129** | 0.101*** | 0.086** |
| | [0.001] | [0.032] | [0.000] | [0.021] | [0.010] | [0.021] |
| Transfer Recipients | | 0.017 | | 0.017 | | 0.013 |
| | | [0.449] | | [0.442] | | [0.578] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 25.93 | 26.98 | 25.93 | 26.98 | 25.93 |
| SD Dep. Variable | 2.51 | 2.55 | 2.51 | 2.55 | 2.51 | 2.55 |
| Mean Treatment Ind. | 8.60 | 8.60 | 2.73 | 2.73 | 2.79 | 2.79 |
| $R^2$ | 0.323 | 0.279 | 0.324 | 0.279 | 0.316 | 0.277 |
| N | 4745 | 4745 | 4745 | 4745 | 4719 | 4719 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

For all treatment indicators, the estimated treatment effects become smaller when households relying on transfers are excluded. When the unemployment rate or long-term unemployment rate of 2002 are used as treatment indicators, estimates are about 40 percent smaller than in the baseline specifications, in the case of the share of social assistance recipients the estimate decreases by 15 percent. While this is only a rough approximation of the mechanical effect of the Hartz reforms on income inequality, the results do provide a first indication that part of the increase in income inequality is driven by a direct monetary impact on transfer reliant households.

### 2.6.2 Compositional Changes in the Working Population

Can changes in the composition of the working population explain the remainder of the increase in income inequality? Simulations of macroeconomic models calibrated to the

German economy suggest that the Hartz reforms, inter alia, lowered unemployment and increased part-time work. These changes in population shares may in turn have led to the increase in income inequality.

In order to test this mechanism, I evaluate whether changes in participation and employment rates, in the share of part-time workers, as well as in the share of households relying on government transfer payments generate the increase in income inequality. I use the Microcensus and compute county-level participation rates (by gender), employment rates (by gender), the share of part-time employees in the population, as well as the share of households stating government transfers as their main income source. In a first step, I estimate the effect of the Hartz reforms on these population shares. Next, I perform a simple mediation analysis, where I add the participation rates, employment rates, the share of part-time employees, and the share of households relying on transfers as additional controls to my baseline specification. If the Hartz reform did indeed cause compositional changes in the working population and if these changes did cause the increase in inequality, the estimates in the mediation analysis should be smaller than the baseline estimate.

Table 2.9 presents the DiD treatment effects on participation, employment, part-time work, and the share of households relying on transfer payments using the continuous unemployment rate of 2002 as treatment indicator. Table 2.10 displays the results of the mediation analysis.

Table 2.9: Participation, Employment, Part-time Work, and Transfers

| | Participation | | | Employment | | | Part-time | Transfers |
|---|---|---|---|---|---|---|---|---|
| | (1) All | (2) Male | (3) Female | (4) All | (5) Male | (6) Female | (7) All | (8) HH |
| Treatment | -0.005 | -0.070 | 0.060 | 0.075 | 0.017 | 0.133* | 0.110*** | 0.109*** |
| | [0.932] | [0.200] | [0.383] | [0.214] | [0.787] | [0.080] | [0.003] | [0.006] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 74.90 | 81.60 | 68.13 | 69.87 | 76.15 | 63.53 | 17.65 | 5.23 |
| SD Dep. Variable | 4.42 | 3.86 | 6.14 | 5.73 | 5.79 | 6.76 | 3.98 | 2.83 |
| Mean Treatment Ind. | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 |
| $R^2$ | 0.668 | 0.351 | 0.673 | 0.761 | 0.544 | 0.742 | 0.776 | 0.220 |
| N | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

As the results in Table 2.9 suggest, the Hartz reforms had no effect on participation rates but increased female employment, part-time work, and the share of households relying on transfer payments.[18] These results are largely in line with the literature. Especially the increase in part-time work and female employment is well documented (Burda and Seele, 2016; Carrillo-Tudela et al., 2018).

[18] Note that the parallel trend assumption does not hold for the employment and male employment rate. Even study results are available on request.

Table 2.10: Mediation Analysis

| | (1) Baseline | (2) Participation | (3) Employment | (4) Part-time | (5) Transfers | (6) All |
|---|---|---|---|---|---|---|
| Treatment | 0.106*** | 0.106*** | 0.110*** | 0.106*** | 0.090*** | 0.092*** |
| | [0.001] | [0.000] | [0.000] | [0.001] | [0.003] | [0.002] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Participation | No | Yes | No | No | No | Yes |
| Employment | No | No | Yes | No | No | Yes |
| Part-time | No | No | No | Yes | No | Yes |
| Transfers | No | No | No | No | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.98 | 26.98 | 26.98 | 26.98 | 26.98 |
| Mean Treatment Ind. | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 |
| $R^2$ | 0.323 | 0.328 | 0.332 | 0.323 | 0.338 | 0.343 |
| N | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

Nevertheless, the mediation analysis in Table 2.10 reveals that only the increase in the share of households relying on transfer payments has a small impact on income inequality. Adding the share of households relying on transfers as an additional control variable to the baseline specification lowers the point estimate from 0.11 to 0.09. On the other hand, neither the increase in the female employment rate nor the increase in the share of part-time employees can explain the effect of the Hartz reforms on income inequality. Including participation rates, employment rates, or the share of part-time workers in the regression does not change the estimate on the Gini coefficient. Overall, I conclude that compositional changes in the working population can only explain a small part of the the increase in income inequality and only stem from an increase in the share of households relying on transfers.

### 2.6.3 Changes within the Household

In this subsection, I analyze whether changes within the household as a result of the Hartz reforms have contributed to the increase in income inequality. In the last subsection, I documented that the Hartz reforms increased the share of part-time employees as well as the female employment rate. These two developments are closely related. As Carrillo-Tudela et al. (2018) point out, a large group of low-skilled married women took up mini- and midi-jobs after the introduction of Hartz II to contribute to household income.

To test whether such an increase in the number of earners per household can explain a part of the observed increase in household income inequality, I estimate the effect of the Hartz reforms on individual net income inequality between households' main income earners only. If the effect of the Hartz reform on household income inequality does partly stem from an increase in earners per household topping up household incomes, the treatment effect of the Hartz reforms on income inequality between main income earners should be smaller than the baseline effect. Using the Microcensus, I determine a household's main income earner

as the person with the highest individual net income within the household and compute county-level inequality measures using solely the individual net incomes of main income earners.[19]

Table 2.11: Inequality between Households' Main Income Earners

| | Unemployment | | Long-term Unemployment | | Social Assistance | |
|---|---|---|---|---|---|---|
| | (1) Baseline | (2) Main Earner | (3) Baseline | (4) Main Earner | (5) Baseline | (6) Main Earner |
| Treatment | 0.106*** | 0.111*** | 0.208*** | 0.233*** | 0.101*** | 0.143*** |
| | [0.001] | [0.002] | [0.000] | [0.000] | [0.010] | [0.006] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 29.92 | 26.98 | 29.92 | 26.98 | 29.92 |
| SD Dep. Variable | 2.51 | 2.91 | 2.51 | 2.91 | 2.51 | 2.91 |
| Mean Treatment Ind. | 8.60 | 8.60 | 2.73 | 2.73 | 2.79 | 2.79 |
| $R^2$ | 0.323 | 0.306 | 0.324 | 0.307 | 0.316 | 0.302 |
| N | 4745 | 4745 | 4745 | 4745 | 4719 | 4719 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

Columns (2), (4), and (6) of Table 2.11 present the DiD treatment effects using the continuous unemployment rate, long-term unemployment rate, and share of social assistance recipients as treatment indicator. To facilitate comparisons, columns (1), (3), and (5) display the results on household income inequality ('Baseline'). The estimated treatment effects are largely in line with the results on household income inequality, even somewhat larger. This indicates that an increase in the number of earners within the household is not what drives the effect of the Hartz reforms on income inequality.[20]

### 2.6.4 Wage Inequality

Wages constitute an important—for most households the most important—part of household income. One conjecture is therefore that the Hartz reforms caused an increase in wage inequality which in turn led to an increase in income inequality. In fact, a number of studies have established that wage inequality has risen substantially in Germany since the 1980s and 1990s (Card et al., 2013; Dustmann et al., 2009). Opinions are divided over whether this rise is attributable to increasing firm heterogeneity and assortativeness in the assignment of workers to firms (Card et al., 2013), to trade and technological changes, or to changes in labor market institutions including labor market reforms, unionization, and wage setting institutions (Dustmann et al., 2014, 2009). Studies explicitly analyzing the effect of the Hartz reforms on wages provide mixed evidence and in most cases do not look at the distributional effects in more detail (Bradley and Kügler, 2019; Launov and Wälde, 2013).

[19] Note that individual net incomes are interval-censored. I therefore impute continuous income figures using interval-regressions. Moreover, I adjust individual net incomes for changes in prices.

[20] In principle, changes in household size such as an increase of single households etc. could also lead to an increase in income inequality. I discuss this possibility in Section 2.6.5.

I analyze the effect of the Hartz reforms on the wage distribution by re-estimating Equation 2.1 and using wage inequality as the outcome variable. Since the Microcensus does not contain information on wages, I compute county-level wage inequality measures using information on individual daily wages from the Sample of Integrated Labor Market Biographies (SIAB) of the Institute for Employment Research (*Institut für Arbeitsmarktforschung*, IAB). The SIAB does not include any information on hours worked, I therefore restrict the sample to full-time employees.[21] Further details on the data, the sample selection, and the variables can be found in the appendix.

Table 2.12 presents the DiD treatment effects on the distribution of full-time wages. Column (1) displays the estimate on the Gini coefficient when using the continuous unemployment rate of 2002 as treatment indicator. Columns (2) and (3) display the results for the continuous long-term unemployment rate and the share of social assistance recipients, respectively. Event study results can be found in Table A2.9 in the appendix.

Table 2.12: Full-time Wage Inequality

| | (1) Unemployment | (2) Long-term Unemployment | (3) Social Assistance |
|---|---|---|---|
| Treatment | 0.009 | 0.034 | 0.055 |
| | [0.688] | [0.454] | [0.122] |
| Demographic Controls | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 29.93 | 29.93 | 29.93 |
| SD Dep. Variable | 2.34 | 2.34 | 2.34 |
| Mean Treatment Ind. | 8.60 | 2.73 | 2.79 |
| $R^2$ | 0.644 | 0.644 | 0.643 |
| N | 4745 | 4745 | 4719 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

The results show that the Hartz reforms did not have a statistically significant impact on full-time wage inequality. While point estimates are positive, they are a order of magnitude smaller than the estimates on income inequality. Moreover, coefficients are not statistically significant, irrespective of the choice of treatment indicator. Thus, the results suggest that the effect of the Hartz reforms on income inequality did not stem from an increase in wage inequality—at least not from an increase in full-time wage inequality. Descriptive evidence suggest that following the Hartz reforms part-time work became a new and important adjustment channel in the German labor market and that wage inequality among part-time employees increased (Burda and Seele, 2016). Unfortunately, due to data restrictions, I am not able to provide evidence on the effect of the Hartz reforms on the distribution of part-time wages, but discuss this potentially important transmission channel in the next section.

[21] Note, that I have also restricted the SIAB sample to those counties included in the balanced Microcensus sample.

### 2.6.5 Further Transmission Channels

Another possible transmission channel, which has only be hinted at in the preceding analysis, is labor market dualization. Labor market dualization refers to growing disparities between a relatively stable core labor force and a flexible margin of non-standard work (i.e., part-time, temporary, and marginal employment, fixed-term contracts etc.). Germany's path to a dual labor market began in the 1980s with the Employment Promotion Act (*Beschäftigungsförderungsgesetz*) and was reaffirmed by the Hartz reforms. The Hartz reforms deregulated the temporary work sector, introduced subsidies for marginal part-time employment exempted from or with reduced social security contributions, and further liberalized other forms of atypical employment such as lowering the age threshold for fixed-term contracts (Eichhorst and Marx, 2011).

Increasing dualization of the German labor market can lead to a rise in income inequality by increasing the share of workers in non-standard employment as well as by increasing the within-group wage inequality of non-standard workers. The growing importance of marginal and part-time work has already been documented (see, for example, Burda and Seele (2016) as well as Section 2.6.2 of this chapter). Nevertheless, the mediation analysis in Section 2.6.2 also showed that controlling for the share of part-time employees does not change the baseline estimate.

In addition to analyzing the effect of the Hartz reforms on the population share of (marginal) part-time employees, it would thus be informative to also study their effect on the distribution of (marginal) part-time wages. Unfortunately, data restrictions do not allow me to do so.[22] However, Carrillo-Tudela et al. (2018) provide first descriptive evidence that within group wage inequality increased for marginal and part-time workers after 2003/ 2005. They find that marginal workers incurred wage losses across the entire distribution but most notably above the median, for part-time employees wages below the median decreased. Far from being causal, this evidence nevertheless gives a first tentative indication that a part of the effect of the Hartz reforms on income inequality may be attributed to the increasing dualization of the German labor market.

Besides labor market dualization, another transmission channel might be changes in household structures. Peichl et al. (2012) find that changes in household structures can explain 78 percent of the rise in gross income inequality between 1991 and 2007 and 22 percent of the rise in net income inequality. Since the Hartz reforms have increased incentives for transfer recipients to live alone (benefits are now offset by household income), changes in household size may account for some of the increase in income inequality. Since the effect of the Hartz reforms on the average number of household members per county is statistically insignificant and including the average number of household members per county as an additional control

[22] In Germany, part-time work can mean anything from working one to 39 hours. Since the SIAB data includes no information on hours worked, looking at the distribution of part-time daily wages is not informative.

variable to the baseline specification does not alter my results, I do not find this explanation very likely.[23]

Taken together, the results in this section indicate that a part of the Hartz reforms' effect on income inequality is driven by the direct effect of Hartz IV on the income of households relying on government transfers as well as by an increase in the share of transfer recipients. In contrast, neither changes within the household nor changes in the full-time wage distribution can account for the increase in disposable household inequality. Since the Hartz reforms brought about a great deal of deregulation at the margin of the labor market but apart from benefit cuts left core (i.e., full-time) workers unaffected (Eichhorst and Marx, 2011), these results are expected. What drives the remainder of the increase in inequality? I find a rise in wage inequality of non-standard workers due to the increasing dualization of the German labor market the most likely candidate. Due to data restrictions, I am not able to test this directly, though, and leave it for further research.

## 2.7 Conclusion

The German Hartz reforms are often cited as the most far-reaching reform endeavor in the history of the German welfare state (Jacobi and Kluve, 2007). It is therefore not surprising that a huge controversy surrounds them. The public debate about the Hartz reforms boils down to efficiency vs. equity arguments, where proponents stress the reforms' apparent success in lowering unemployment and stimulating growth and opponents lament their distributional consequences. While a large literature exists investigating the Hartz reforms' impact on unemployment, so far, little is known about their impact on inequality. This chapter contributes to the literature by providing first causal evidence on the effect of the Hartz reforms on income inequality.

Exploiting the regional variation in the intensity German counties were affected by the reforms in a DiD framework and using county-level data on disposable household income inequality from the German Microcensus, I find that the Hartz reforms had a small positive effect on income inequality. This effect is robust to the way treatment is defined, the choice of treatment indicator, and sample selection. Looking at the income distribution more closely, I document that it is the second to sixth income deciles which lose in terms of income shares, whereas the upper three deciles gain.

Testing for possible transmission channels, the results suggest that the increase in income inequality is partly due to a mechanical monetary effect of the last Hartz reform on the income of households relying on government transfers. Another part of the increase in income inequality is caused by a rise in the share of households relying on transfer payments. In contrast, neither an increase in the number of earners per household nor changes in the

[23] Results are available on request.

distribution of full-time wages seem to play a role. Even though I am not able to test it directly, I argue that an increase in the wage inequality of non-standard workers due to increasing labor market dualization may be able to explain the remaining part of the observed increase in income inequality.

All in all, I find that the Hartz reforms seem to have had their intended effect. Designed to increase the flexibility of the German labor market and to decrease unemployment and stimulate growth, the Hartz reforms mainly deregulated non-standard work and cut benefits for the unemployed. Distributional consequences due to changes in the transfer system and an expansion of the non-standard work sector must have been taken under consideration. However, whether the social-democratic-green government coalition under Gerhard Schröder anticipated the immense political backlash to the reforms is another question. Given the fact, that the SPD has meanwhile distanced itself from the Hartz reforms and ran their 2017 election campaign under the heading 'social equity', it stands to reason that the SPD has underestimated employees' needs for secure and stable jobs. One lesson from the German Hartz reforms may thus be that reforms aimed to tackle structural problems in the economy come at a heavy political cost when components of flexibility and security are not appropriately balanced.

# Appendix

## Sample of Integrated Labor Market Biographies (SIAB)

The SIAB is a two percent random sample drawn from administrative social security records in Germany and made available by the Institute for Employment Research (IAB). It is representative of all individuals covered by the social security system (i.e. employees, benefit recipients, individuals officially registered as job-seeking or participating in programs of active labor market policies) and thus covers about 80 percent of the German workforce. The self-employed, civil servants, and individuals currently doing their military service are not included in the sample.

In the weakly anonymous version of the SIAB, which can be accessed via a research visit at the IAB or via remote data access only, the individuals' county of residence (since 1999) and place of work (since 1975) are available to the researcher. Therefore the data are well suited to compute wage inequality measures at the regional level. Note, that the SIAB data is structured in spells. In order to facilitate the analysis, I follow Eberle and Schmucker (2019) and create a cross-sectional data set, using June, $30^{th}$ as reference date.

Furthermore, daily wages in the SIAB are right-censored at the highest level of earnings subject to social security contributions. In order to impute the right-tail of the wage distribution, I follow Dustmann et al. (2009) and impute censored wages under the assumption that the error term in the wage regression is normally distributed with different variances for each age group, education group, and year (for more information on the imputation technique and assumptions made see Dustmann et al. (2009)).

Since the SIAB contains no information on hours worked, I restrict the sample to full-time employees. Moreover, I drop unrealistically low and high wages (i.e. daily wages below ten Euros or above the social security contribution limit). Adjusting for price changes, I again compute county-level inequality measures, using the information on the employee's county of residence.

## Additional Tables

Table A2.1: County Long-term Unemployment Rates in 2002—Summary Statistics

| | (1) All | (2) West Germany | (3) East Germany |
|---|---|---|---|
| N | 365 | 321 | 44 |
| Mean | 2.7 | 2.3 | 6 |
| Sd | 1.8 | 1.3 | 1.9 |
| Min | .3 | .3 | 2 |
| P25 | 1.5 | 1.4 | 4.3 |
| P50 | 2.2 | 2 | 5.6 |
| P75 | 3.5 | 2.9 | 7.6 |
| Max | 9.9 | 7.5 | 9.9 |
| Observations | 6935 | 6935 | 6935 |

*Notes:* The long-term unemployment rate is measured in percent.

Table A2.2: County Shares of Social Assistance Recipients in 2002—Summary Statistics

| | (1) All | (2) West Germany | (3) East Germany |
|---|---|---|---|
| N | 363 | 319 | 44 |
| Mean | 2.8 | 2.8 | 2.8 |
| Sd | 1.6 | 1.6 | 1.4 |
| Min | .4 | .4 | .9 |
| P25 | 1.6 | 1.5 | 2 |
| P50 | 2.5 | 2.6 | 2.4 |
| P75 | 3.5 | 3.6 | 3.2 |
| Max | 10.1 | 10.1 | 7.8 |
| Observations | 6935 | 6935 | 6935 |

*Notes:* The share of social assistance recipients is measured in percent.

## Table A2.3: Hartz Reforms and Inequality—Event Study

| | (1) Gini | (2) Gini | (3) Gini |
|---|---|---|---|
| Treatment × 1999 | -0.030 | -0.041 | -0.035 |
| | [0.219] | [0.371] | [0.462] |
| Treatment × 2000 | -0.013 | -0.051 | -0.051 |
| | [0.601] | [0.240] | [0.249] |
| Treatment × 2001 | 0.034 | -0.006 | -0.007 |
| | [0.130] | [0.864] | [0.837] |
| Treatment × 2002 | -0.011 | -0.040 | -0.039 |
| | [0.560] | [0.194] | [0.200] |
| Treatment × 2004 | 0.054*** | 0.051* | 0.055* |
| | [0.001] | [0.077] | [0.058] |
| Treatment × 2005 | 0.117*** | 0.072** | 0.070** |
| | [0.000] | [0.036] | [0.045] |
| Treatment × 2006 | 0.097*** | 0.021 | 0.020 |
| | [0.000] | [0.624] | [0.639] |
| Treatment × 2007 | 0.108*** | 0.063 | 0.059 |
| | [0.000] | [0.136] | [0.147] |
| Treatment × 2008 | 0.155*** | 0.120*** | 0.113** |
| | [0.000] | [0.009] | [0.011] |
| Treatment × 2009 | 0.147*** | 0.082** | 0.073* |
| | [0.000] | [0.036] | [0.054] |
| Treatment × 2010 | 0.178*** | 0.132*** | 0.119*** |
| | [0.000] | [0.001] | [0.003] |
| Treatment × 2011 | 0.165*** | 0.112*** | 0.090** |
| | [0.000] | [0.006] | [0.025] |
| Treatment × 2012 | 0.167*** | 0.099** | 0.071* |
| | [0.000] | [0.015] | [0.077] |
| Treatment × 2013 | 0.156*** | 0.116*** | 0.087** |
| | [0.000] | [0.008] | [0.043] |
| Treatment × 2014 | 0.111*** | 0.068 | 0.038 |
| | [0.000] | [0.136] | [0.380] |
| Demographic Controls | No | No | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | No | No |
| Region-Year FE | No | Yes | Yes |
| Mean Dep. Variable | 27.06 | 27.06 | 27.06 |
| SD Dep. Variable | 2.50 | 2.50 | 2.50 |
| Mean Treatment Ind. | 8.60 | 8.60 | 8.60 |
| $R^2$ | 0.275 | 0.278 | 0.290 |
| N | 5840 | 5840 | 5840 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

## Table A2.4: Income Shares—DiD

| | (1)<br>1. Decile | (2)<br>2. Decile | (3)<br>3. Decile | (4)<br>4. Decile | (5)<br>5. Decile | (6)<br>6. Decile | (7)<br>7. Decile | (8)<br>8. Decile | (9)<br>9. Decile | (10)<br>10. Decile |
|---|---|---|---|---|---|---|---|---|---|---|
| Treatment | -0.006 | -0.015*** | -0.020*** | -0.017*** | -0.016*** | -0.009*** | -0.002 | 0.008** | 0.027*** | 0.050*** |
| | [0.465] | [0.001] | [0.000] | [0.000] | [0.000] | [0.005] | [0.515] | [0.019] | [0.000] | [0.007] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 3.66 | 5.40 | 6.53 | 7.53 | 8.50 | 9.52 | 10.69 | 12.21 | 14.53 | 21.43 |
| SD Dep. Variable | 0.44 | 0.43 | 0.42 | 0.36 | 0.30 | 0.26 | 0.25 | 0.33 | 0.56 | 1.45 |
| Mean Treatment Ind. | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 | 8.60 |
| $R^2$ | 0.079 | 0.312 | 0.356 | 0.333 | 0.245 | 0.132 | 0.075 | 0.106 | 0.183 | 0.244 |
| N | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 | 4745 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

## Table A2.5: Alternative Treatment Indicator: Share of Long-term Unemployed among all Unemployed

| | (1) Continuous | (2) Binary 50% | (3) Binary 33% | (4) Binary 25% |
|---|---|---|---|---|
| Treatment | 0.034*** | 0.244* | 0.540*** | 0.666*** |
| | [0.002] | [0.091] | [0.003] | [0.001] |
| Demographic Controls | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes |
| Region-Year FE | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 26.98 | 26.98 | 26.88 | 26.85 |
| SD Dep. Variable | 2.51 | 2.51 | 2.37 | 2.28 |
| Mean Treatment Ind. | 29.50 | | | |
| △ Mean Treatment Ind. | | 11.32 | 15.53 | 18.05 |
| $R^2$ | 0.322 | 0.317 | 0.327 | 0.333 |
| N | 4745 | 4745 | 3146 | 2392 |

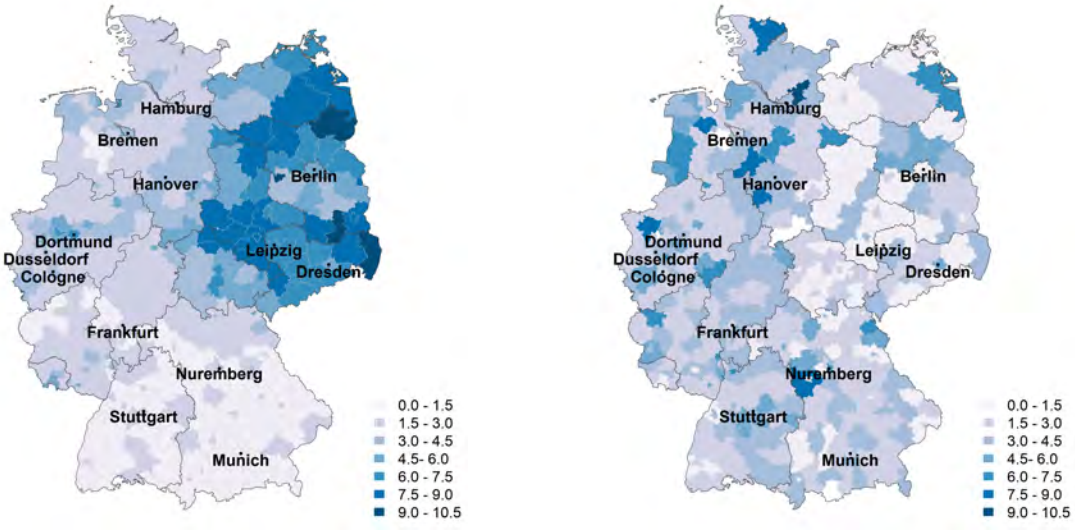
*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the level of labor market regions.

## Additional Figures

### Figure A2.1: Alternative Treatment Indicators across German Counties in 2002

(a) Long-term Unemployment Rate across Counties in 2002

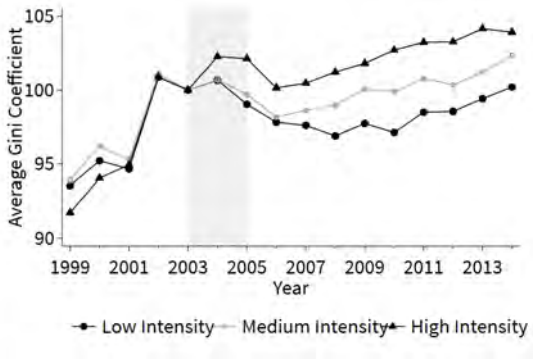
(b) Share of Social Assistance Recipients across Counties in 2002



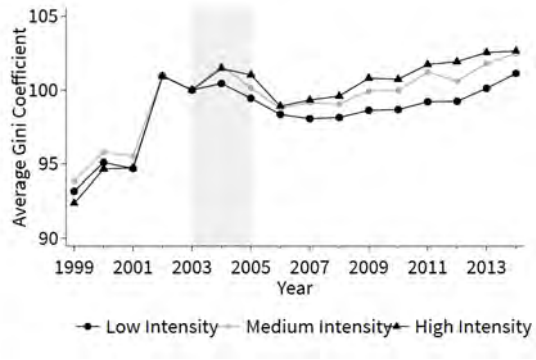

*Notes:* The figure shows long-term unemployment rates and share of social assistance recipients across German counties in 2002. Long-term unemployment rates and shares of social assistance recipients are measured in percent.

## Figure A2.2: Alternative Treatment Indicators: Visualization of the Parallel Trend Assumption

(a) Long-term Unemployment Rate



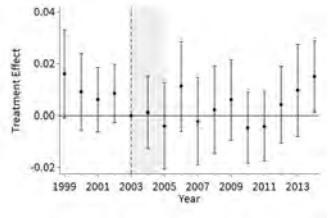

(b) Share of Social Assistance Recipients



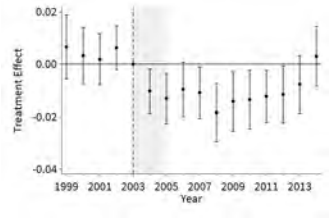

*Notes:* The figure shows the trend of the average Gini coefficients by treatment intensity group from 1999 to 2014. Gini coefficients are normalized to 2003=100.

## Figure A2.3: Income Shares—Event Study

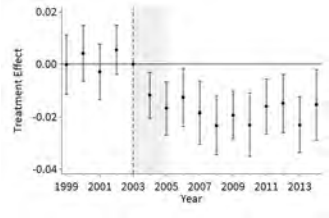
(a) 1. Decile



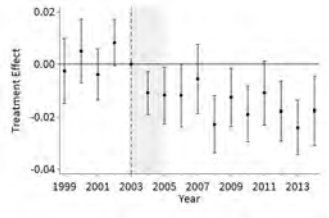

(b) 2. Decile



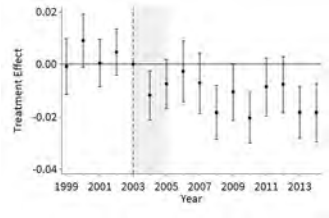

(c) 3.Decile



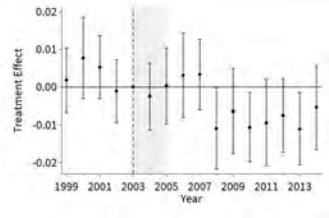

(d) 4. Decile



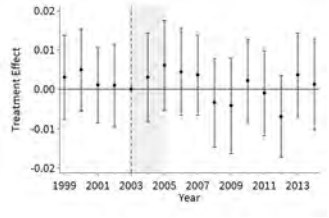

(e) 5. Decile



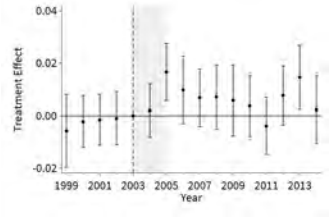

(f) 6. Decile



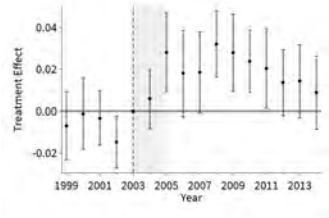

(g) 7. Decile



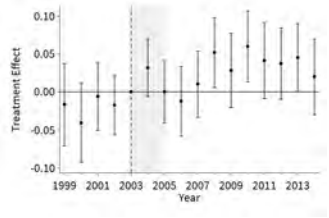

(h) 8. Decile




(i) 9. Decile



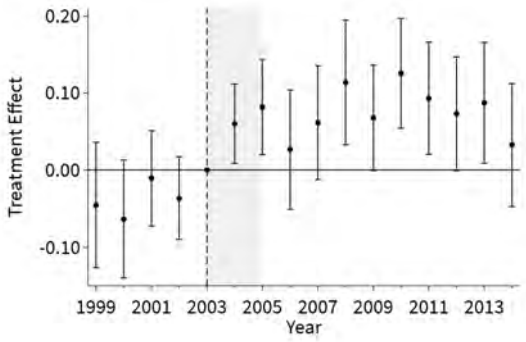

(j) 10. Decile



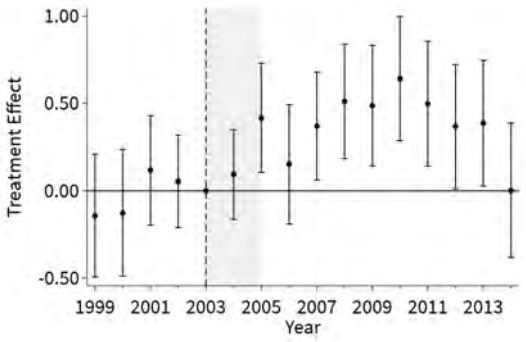

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

## Figure A2.4: Alternative Treatment Definitions—Event Study

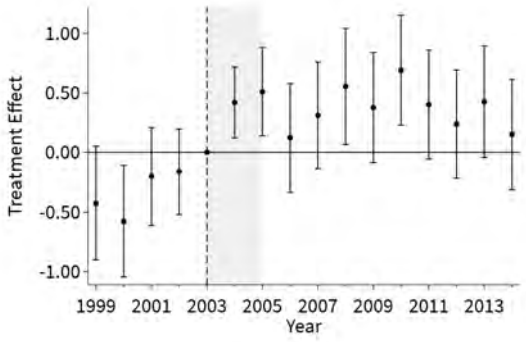
(a) Continuous Treatment: Winsorized



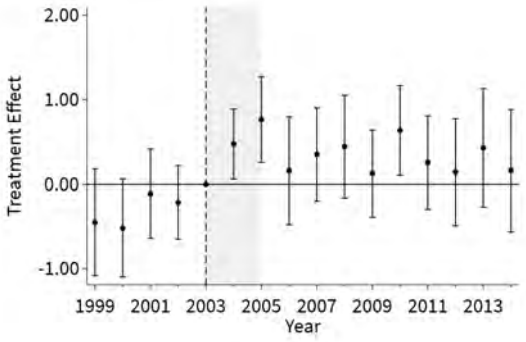

(b) Binary Treatment: 50%




(c) Binary Treatment: 33%



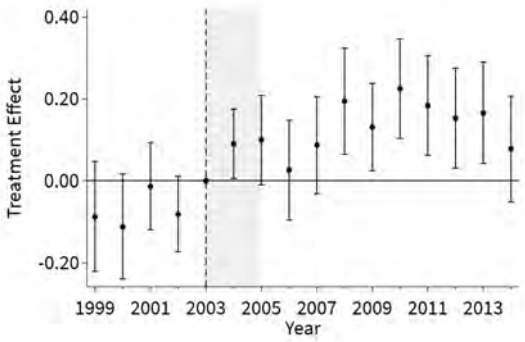

(d) Binary Treatment: 25%



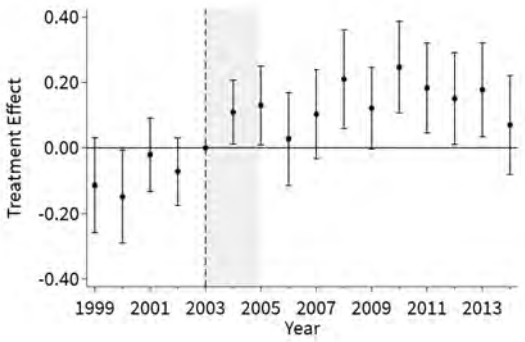

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

## Figure A2.5: Alternative Treatment Indicators: Long-term Unemployment Rate—Event Study

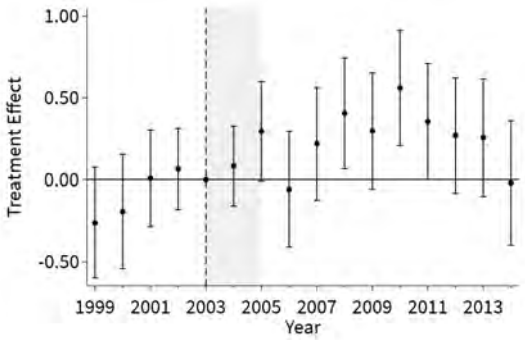
(a) Continuous Treatment



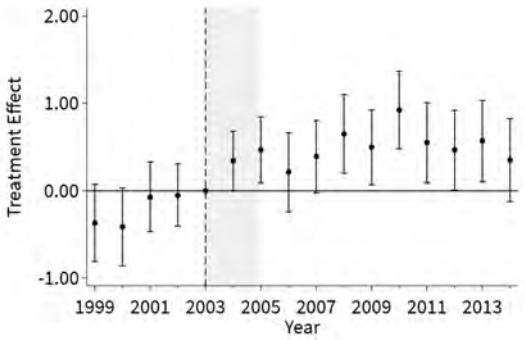

(b) Winsorized



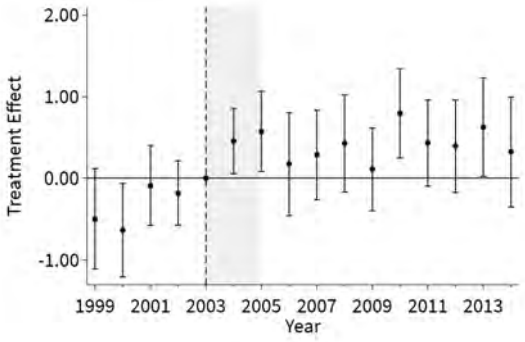

(c) Binary Treatment: 50%




(d) Binary Treatment: 33%



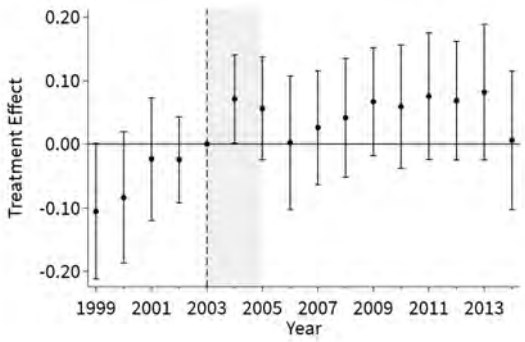

(e) Binary Treatment: 25%



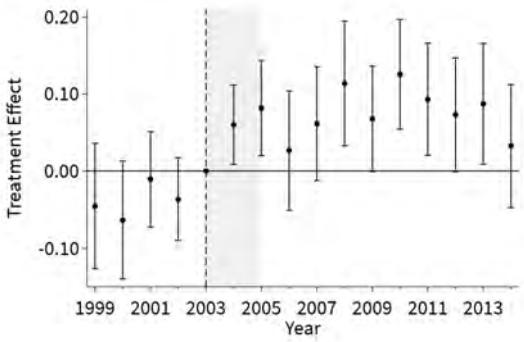

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

Figure A2.6: Alternative Treatment Indicators: Social Assistance Recipients—Event Study

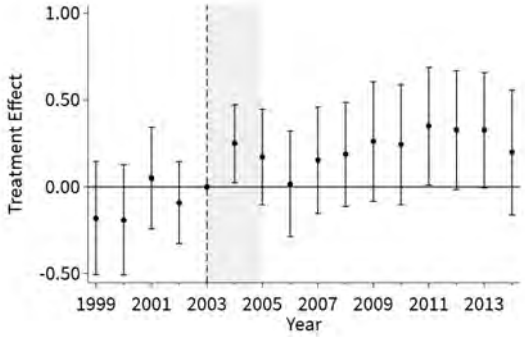
(a) Continuous Treatment



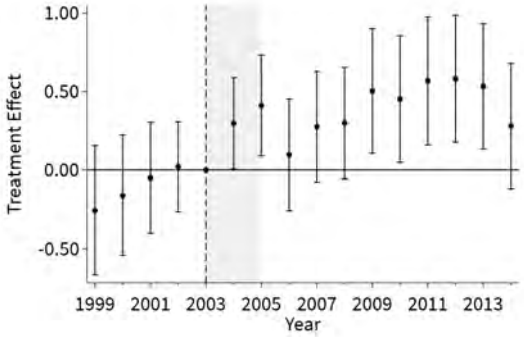

(b) Winsorized



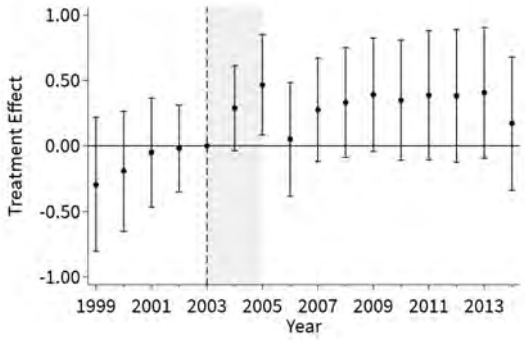

(c) Binary Treatment: 50%




(d) Binary Treatment: 33%



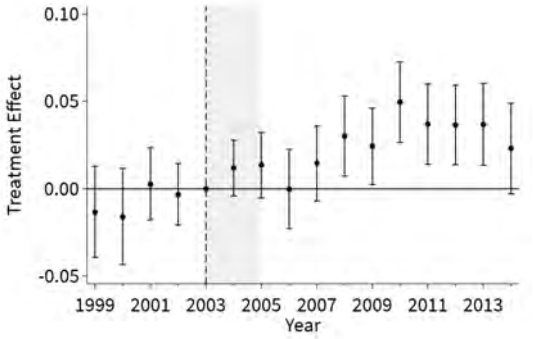

(e) Binary Treatment: 25%



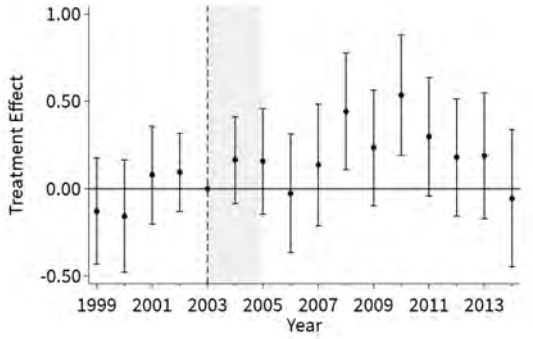

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

## Figure A2.7: Alternative Treatment Indicators: Share of Long-term Unemployed among all Unemployed—Event Study

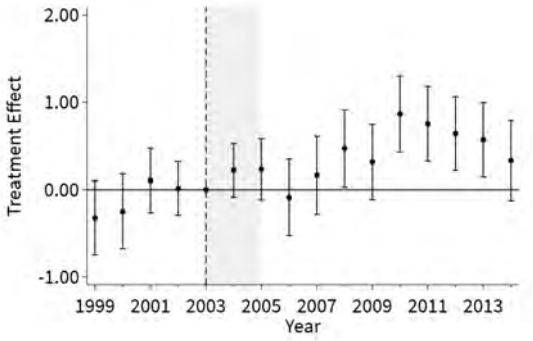
(a) Continuous Treatment



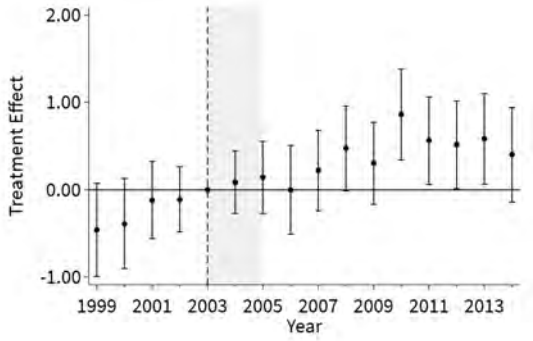

(b) Binary Treatment: 50%




(c) Binary Treatment: 33%



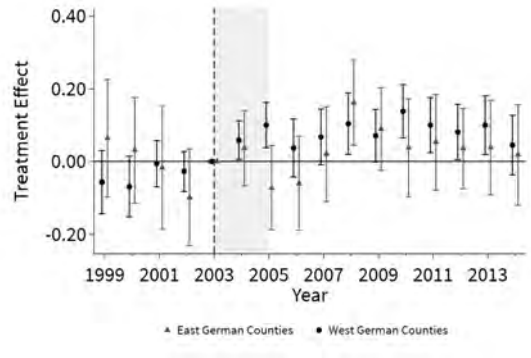

(d) Binary Treatment: 25%



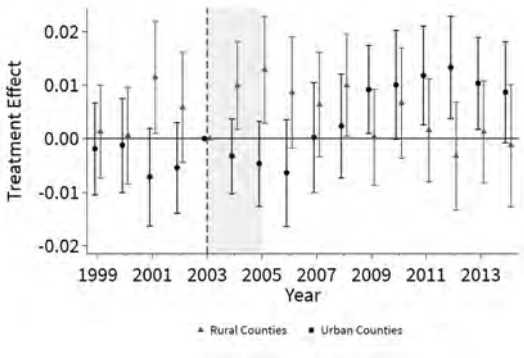

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

## Figure A2.8: Heterogeneous Effects—Event Study

(a) East vs. West German Counties



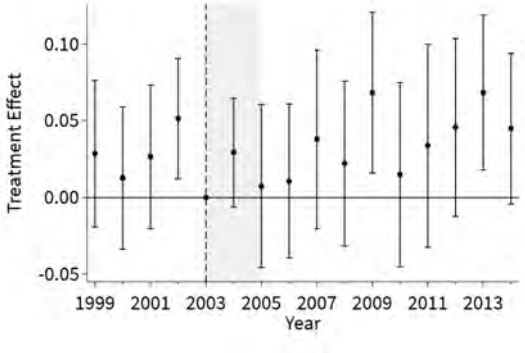

(b) Rural vs. Urban Counties



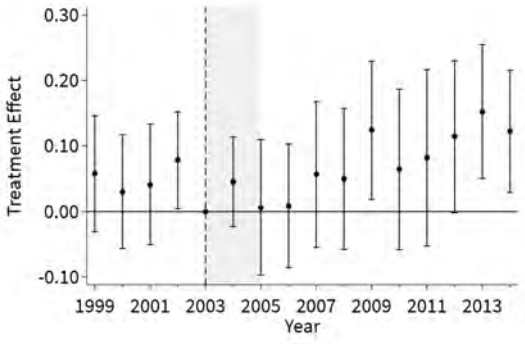

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

## Figure A2.9: Full-time Wage Inequality—Event Study

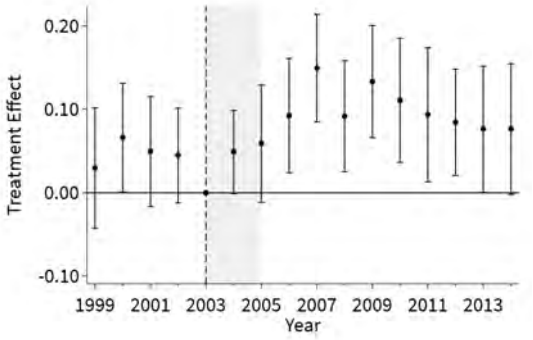
(a) Continuous Treatment: Unemployment




(b) Continuous Treatment: Long-term Unemployment




(c) Continuous Treatment: Social Assistance




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

# 3 University Openings and their Long-term Impact on Regional Wages: Evidence from West Germany*


## Abstract

This chapter investigates the long-term effect of university openings on regional wages in West Germany. We combine geo-coded data on the universe of German universities with information on individual wages from social security records to estimate the impact of university openings on different wage percentiles of the county wage distribution. We find that establishing a new university has a positive effect on wages in nearby counties. This effect differs in terms of size and timing along the wage distribution as well as between different employee subgroups. Moreover, the effect is driven by establishing universities of applied sciences and by universities in urban regions. In an extension of our analysis, we study the impact of university openings on the composition of the county's work force. Our results suggest that opening a university leads to a rise in the share of employees having attained a university degree as well as to a shift from the primary and secondary to the tertiary industry sector.


* This chapter is joint work with Clemens Fuest. It is based on our paper 'University Openings and their Long-term Impact on Regional Wages: Evidence from West Germany'.

## 3.1 Introduction

In most countries in the world, large differences in income and wages exist between regions. Since these regional discrepancies do not seem to disappear over time, but instead have been growing in many developed countries over the last decades,[1] regional inequalities have become an important policy concern. This raises the question what policy makers can do to help the development of under-performing regions. Here, the last two decades saw a shift in regional development policies from place-based policies offering incentives to individual firms to locate in less favored regions to policies supporting the endogenous growth of regions (Amin, 1999; Goddard and Vallance, 2011).

This shift in regional policy thinking has led to a re-examination of the role of universities and other higher education institutions (Goddard and Vallance, 2011; OECD, 2007). In the past, higher education institutions served national goals or the pursuit of knowledge without considering their impact on the surrounding communities. In a competitive and globalized world where developed countries turn their production towards knowledge-intensive products and services, universities are now increasingly attributed a central role in the building of knowledge economies, both at the national as well as at the regional level (OECD, 2007). Yet, little is known empirically about their causal impact on regional economic development.

Theoretically, universities can contribute to regional economic development in various ways. First, they have a direct effect on the regional economy. Universities offer employment, demand local government services and local firms' products, and supply goods and services. Second, universities produce graduates. If these graduates stay in the region, the average skill level of the local workforce and, as a result, productivity and wages may rise. Third, universities may give rise to knowledge spillovers to local firms. This may happen through informal exchange between university researchers and firm employees or through common projects. Fourth, many universities encourage the creation of new firms by their own researchers or in cooperation with the private sector.

For regional development, a key issue is whether these productivity enhancing effects of knowledge creation have a local impact or whether they are more dispersed. For instance, it may well be that a university produces many highly productive graduates, but if the graduates move to jobs in other regions, the impact will not be felt locally. The same applies to local knowledge spillovers. While these effects are all theoretically plausible, little is known about their empirical relevance.

This is where we contribute to the literature. In this chapter, we investigate the causal long-term impact of university openings on regional wages and other labor market variables in West Germany. West Germany offers a particularly well-suited setting to study the effect of universities on regional wages for two reasons. First, post-war Germany saw a large expansion

[1] See Glaeser and Mare (2001) for the United States, Combes et al. (2008) for France, or Dauth et al. (2018) for Germany, among others.

of higher education institutions giving us ample variation to exploit in our empirical analysis. Second, detailed employee-level administrative data from social security records which include information on daily wages as well as firm location at the county-level are available since 1975, enabling us to estimate the long-term effects of university openings on regional wages at a very disaggregated level. We focus on wages because the aim of regional policies is ultimately to increase the prosperity of the local population.

To analyze the effect of university openings on regional wages, we combine a two percent sample of social security records with a geo-coded list of all public and state-recognized German higher education institutions and estimate a generalized event study with multiple events. In order to estimate causal effects, we compare counties in whose close vicinity a new university has been established to counties in whose vicinity no new university has been opened. In our baseline specification, we classify a county as treated if its geometric center is within 75 km of a newly established university and as untreated otherwise. We test the robustness of our results by using several alternative cutoffs as well as by employing continuous instead of binary treatment indicators. Besides estimating the effect of university openings on different percentiles of the county wage distribution, we also analyze the effect on median wages by employees' education level, occupation group, and industry sector. Moreover, we evaluate heterogeneous treatment effects between different types of higher education institutions as well as between rural and urban counties.

Our findings suggest that in the long-term establishing a new university has a small but significant positive effect on wages in the counties around the new university. This result holds for various ways of defining treatment. In our baseline specification, county median wages start to increase 17 years after a university opening. 30 and more years after the opening, annual median wages in the surrounding counties increase, on average, by 255 Euros. In relation to the sample mean, this translates into an increase in county median wages of 0.8 percent.

The effect of university openings on wages differs in terms of magnitude and timing along the wage distribution as well as between different employee subgroups. In the lower part of the distribution establishing a new university takes longer to show effect and wage effects are smaller. In the upper part of the distribution wage increases occur earlier and are markedly larger. Similarly, we find that while opening a university is positively associated with wages for all employee subgroups, effects on median wages are largest for employees with post-secondary education as well as for employees providing commercial and business-related services.

Analyzing heterogeneous treatment effects reveals that the effect of university openings on county median wages is driven by establishing so-called universities of applied sciences. In contrast to 'regular' universities which are strongly research oriented, universities of applied sciences put strong emphasis on practical work and application. Treatment effects are also

stronger when opening a public rather than private or ecclesiastical university and mainly come from establishing universities in urban rather than rural regions.

In an extension of our analysis, we study the impact of university openings on the composition of the county's work force. In particular, we evaluate the effects of establishing a new university on the share of full-time employees with different levels of education, different job requirements, and in different industries. We find that establishing a new university leads to a rise in the share of employees with post-secondary education and a decrease in the share of employees with secondary education. This effect is strong—the share of employees with post secondary education increases by 5.5 percent relative to the sample mean. In contrast, university foundations do not seem to influence the composition of the work force in terms of job requirements. Instead, it leads to a shift of employment from the primary and secondary industry sector to the tertiary sector.

Our study is related to a growing literature examining the contribution of universities to regional economic development. Following Jaffe (1989), much of the empirical research has focused on estimating local spillover effects of universities on innovation and research (Abramovsky et al., 2007; Anselin et al., 1997; Varga, 2000), business start-ups and entrepreneurialism (Bania et al., 1993; Woodward et al., 2006), or employment changes (Beeson and Montgomery, 1993). Most closely related to our study are the studies by Moretti (2004), Hausman (2012), and Kantor and Whalley (2014) which estimate spillover effects from universities or university activities on income and earnings.

Moretti (2004) estimates the spillover effects from post-secondary education by comparing wages of individuals who work in cities with different shares of college graduates but are otherwise similar. He finds positive wage effects for all education levels but the effects are largest for less educated employees. Hausman (2012) examines the extent to which U.S. universities stimulate nearby economic activity by linking university innovation to economic outcomes in U.S. counties. His results show that long-run employment and payroll per worker rise after an exogenous shock to the spread of innovation from universities, particularly in sectors closely related to local university strength. Kantor and Whalley (2014) use endowment values and stock market shocks as instruments for university spending to analyze local agglomeration spillover effects from university activity. They find statistically significant local spillover effects on the income of workers in other industries. The effects are larger when local universities are more research intensive or local firms are technologically close to universities.

Instead of estimating local spillover effects, work in economic geography explores the role of universities in local growth through case study accounts. A large-scale OECD (2007) report on universities in 14 countries concludes that the involvement of universities in the development of regions varies significantly depending on national and regional features, such as industrial characteristics or the institutional make-up of the national higher education system. Goddard and Vallance (2011) provide an extensive review on the contribution of universities to innovation and technology-based development in their regional economies.

The rest of this chapter proceeds as follows. Section 3.2 provides background on higher education institutions in Germany. Section 3.3 describes the data. In Section 3.4, we present descriptive evidence on regional wages in Germany. We explain our empirical strategy in Section 3.5. Section 3.6 presents our empirical results on university openings and regional wages, in Section 3.7 we extend our analysis and present empirical results on the composition of the work force. Section 3.8 concludes.

## 3.2 The Expansion of Higher Education Institutions in Germany

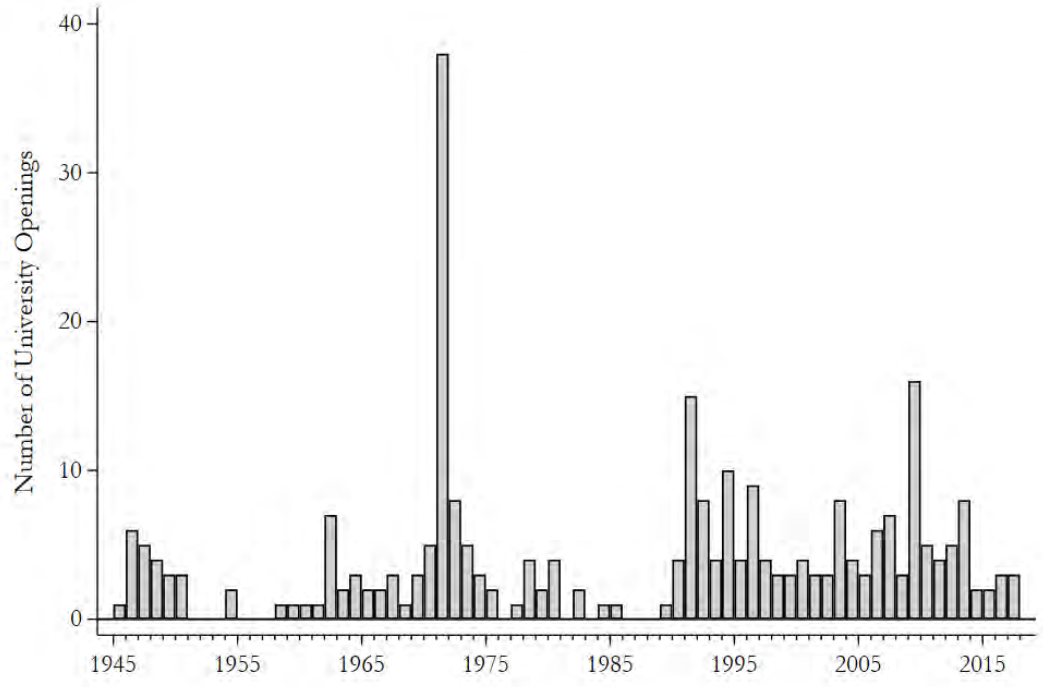
Germany has a long history of higher education dating back to the $14^{th}$ century. Its first university, Heidelberg University, was founded in 1386 followed by Cologne University in 1388 and Leipzig University in 1409. Nowadays, the higher education sector includes not only universities but also universities of applied sciences (*Fachhochschulen*), colleges of art and music (*Kunst- und Musikhochschulen*), as well as other higher education institutions such as universities of cooperative education (*Berufsakademien*) or film academies (*Film- und Schauspielhochschulen*).

While universities are strongly research oriented and typically offer a wide range of subjects, the distinguishing feature of universities of applied sciences is their strong emphasis on practical work and application. They focus on teaching professional skills and often specialize in a particular field such as engineering, technology, or business. Colleges of art and music have the same legal status as universities. They provide education in the visual, creative, and performing arts as well as in musical subjects (Hochschulkompass, 2020b). Moreover, universities of cooperative education offer studies with a strong practical orientation and link theoretical training to the practical training in a company. Film academies serve to train film and television professionals as well as professional actors.

The higher education landscape in Germany has not always been this diverse. It has undergone many transformations over time, particularly an expansion of universities in the post-war period. Figure 3.1 depicts the number of university openings between 1945 and 2018. As can be seen, the largest expansions of universities occurred in the 1960s and 1970s as well as in the 1990s and 2000s.

In 1957 the 'Sputnik Shock' radically challenged the educational system of all Western states. In Germany, the philosopher Georg Picht spoke of an 'educational catastrophe', predicting Germany's disadvantages in international competition and a threat to democracy through an 'education crisis'. Picht recommended an expansion of the already existing universities as well as the establishment of several new universities (Picht, 1964). Growing calls to reform the higher education system in Germany led to numerous reform efforts and a wave of university openings in the mid-1960s (for instance, Bochum (1962), Düsseldorf (1965), or Konstanz (1966)).

Figure 3.1: The Expansion of Higher Education Institutions in Germany



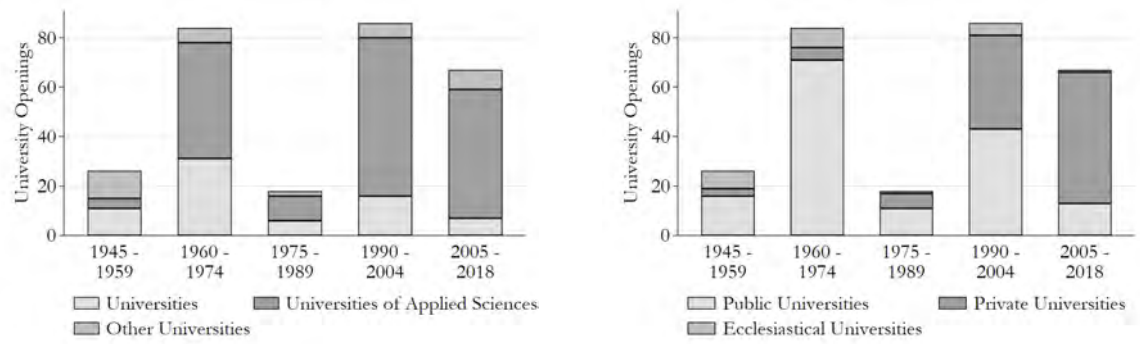

*Notes:* The figure shows the number of university openings per year from 1945 to 2017. It is based on data from the German Rector's Conference (2020). The years 1945 to 1990 include West German institutions only.

Furthermore, in 1968 the ministers (*Ministerpräsidenten*) of the German states agreed to introduce universities of applied sciences as a new type of higher education institution. Universities of applied sciences were based on the already existing technical and engineering schools (*Fach- und Ingenieursschulen*), whose students demanded an upgrade of their education. The first universities of applied sciences were established as early as 1969 but the vast majority was founded in 1971 (see also Figure 3.1).

Another large expansion of higher education institutions can be observed since the 1990s. In the 1990s the expansion was mainly a result of the integration of East German institutions into the West German system but also of further openings of universities of applied sciences in the East as well as in the West. The 2000s and 2010s saw an expansion of private state-recognized institutions, most of which are also universities of applied sciences (see Figure 3.2). In the rest of this chapter, we will use the term university to refer to all higher education institutions without distinguishing between universities, universities of applied sciences, and other types of higher education institutions, unless otherwise specified.

Figure 3.2: University Openings by University Type and Sponsorship

(a) University Openings by University Type (b) University Openings by Sponsorship




*Notes:* The figure shows the number of university openings by university type and sponsorship. It is based on data from the German Rector's Conference (2020). The years 1945 to 1990 include West German institutions only.

## 3.3 Data Description

For our analysis, we combine three data sets, the Sample of Integrated Labour Market Biographies (*Stichprobe der intergrierten Arbeitsmarktbiographien*, SIAB), the Establishment History Panel (*Betriebs-Historik-Panel*, BHP), and a list of German universities made available by the German Rector's Conference (*Hochschulrektorenkonferenz*, HRK) via their information portal Higher Education Compass (*Hochschulkompass*).

### 3.3.1 The Sample of Integrated Labor Market Biographies and the Establishment History Panel

The SIAB is a two percent random sample drawn from social security records and made available by the Institute for Employment Research (*Institut für Arbeitsmarktforschung*, IAB). The SIAB is representative of all individuals covered by the social security system and thus covers about 80 percent of the German workforce, excluding only the self-employed, civil servants, as well as individuals currently doing their military service. Among other variables, it includes information on the individual's year of birth, nationality, gender, employment status, school leaving qualification, employer, occupation, and the skill-level required to perform the task/job.

The data is structured in spells. In order to facilitate the analysis, we follow Eberle and Schmucker (2019) and create a cross-sectional data set, using June, $30^{th}$ as reference date. Furthermore, daily wages in the SIAB are right-censored at the highest level of earnings subject to social security contributions, which pertains to roughly ten percent of wages per year. In order to impute the right-tail of the wage distribution, we follow Dustmann et al. (2009) and impute censored wages under the assumption that the error term in the wage regression is

normally distributed with different variances for each age group, education group, and year.[2] Even so, to insure that top-coding and imputation do not confound our results, we mainly use the uncensored part of the wage distribution in our analysis and focus on median rather than mean wages. Another difficulty in the data is a structural break in the wage measure between 1983 and 1984. Since 1984, daily wages include bonus payments as well as other one-time payments which were not included before (Steiner and Wagner, 1998). We correct for the break using the same procedure as in Dustmann et al. (2009).

For West Germany, the SIAB covers the years 1975 to 2017. East German employees are included since 1992. Because we are interested in the long-term effects of university openings, we restrict the sample to West Germany. Moreover, the SIAB contains no information on hours worked. As in Germany working part-time can mean anything from working one to 39 hours, data on daily part-time wages are not informative. We therefore further restrict the sample to include only full-time employees. Finally, we drop unrealistically low and high full-time wages (i.e., daily wages below ten Euros or erroneous wage data above the social security contribution limit) and adjust daily wages for price changes using the German consumer price index.

The individual employee data of the SIAB can be matched to the firm-level BHP data via a unique firm identifier (Ganzer et al., 2020). The BHP is a cross sectional data set covering all establishments in Germany that are also included in the IAB Employment History on June, $30^{th}$, i.e., all establishments with at least one employee liable to social security on the reference date. The BHP contains information about the branch of industry, the numbers of employees liable to social security, as well as the number of marginal and part-time employees. Most important for our analysis, the BHP includes the location of an establishment at the county-level.

Matching employees to establishments allows us to to aggregate the individual-level SIAB data to the county-level. For the 324 West German counties we compute the $25^{th}$, $50^{th}$ (i.e., median), and $75^{th}$ percentile of counties' real daily (full-time) wage distribution as well as county median wages by education level (no secondary, secondary, and post-secondary education), occupation group (production, commercial and business-related services, and other services), and industry sectors (primary and secondary sector, tertiary sector excluding public administration and education, and public administration and education sector).

In addition to aggregating wages at the county-level, we compute the shares of full-time employees with no secondary, secondary, and post-secondary education in a county as well as the shares of full-time employees performing unskilled, skilled, complex, and highly complex tasks. Making use of the information on the firms' industry classification and number of full-time employees in the BHP, we further compute the number of full-time employees in a county as well as county shares of full-time employees working in the primary and secondary sector, the tertiary sector, and in the public administration and education sector. Finally, we

[2] For more information on the imputation technique and assumptions made see Dustmann et al. (2009).

determine the longitude and latitude of each county's geometric center (i.e., centroid) and calculate the linear distance of each county centroid to the nearest university established in the given year.

### 3.3.2 The Higher Education Compass

The Higher Education Compass is an information portal of the HRK, an association of public and state-recognized universities in Germany, that publishes information from German universities about their study and doctorate opportunities and international partnerships. All information found in the Higher Education Compass is authorized by the universities and is updated by employees at the universities themselves (Hochschulkompass, 2020a). The data can be downloaded from the HRK's website[3] and cover all public and state-recognized universities in Germany. The latest data are from 2018.

The data include information on university name, address, year of opening, as well as information on university type (i.e., university, university of applied sciences, or others) and sponsorship (i.e., public, private, or ecclesiastical). One drawback of the data is that it does not cover private institutions that are not state-recognized or still in the process of being recognized. Moreover, as the data comprises only a list of all current higher education institutions, we do not observe institutions that have been established and closed again between 1945 and 2018. For our analysis, we restrict the data to the 281 universities founded after 1945 and geo-code the addresses.

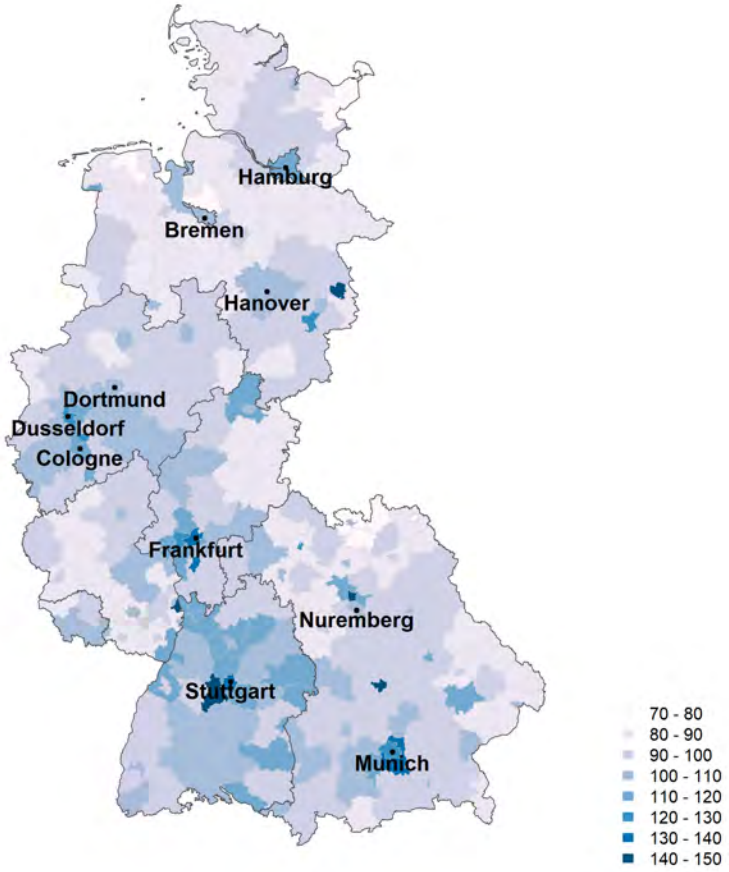
## 3.4 The Regional Distribution of Wages in West Germany

In this section, we present descriptive evidence on the regional distribution of wages in West Germany. Figure 3.3 depicts the median daily wage across counties in 2015. The darker the shade, the higher the county median wage. The figure illustrates that West Germany exhibits vast differences in wages across counties. Differences are particularly large between urban and rural regions and between North and South.

Most pronounced are the wage discrepancies between urban and rural regions. The highest median wages are clustered around Germany's larger cities Hamburg, Cologne, Düsseldorf, Frankfurt, Stuttgart, and Munich. The association between wage discrepancies and city size is well established in the literature. Urban wage premiums have been documented for the U.S. (Glaeser and Mare, 2001), France (Combes et al., 2008), Spain (Roca and Puga, 2017), the U.K. (Rice et al., 2006), and Germany (Dauth et al., 2018), among others, and seem to partly reflect the sorting of workers with different skills (Combes et al., 2008; D'Costa and Overman, 2014). For Germany, Dauth et al. (2018) find that wages in large cities are higher not only because

[3] https://www.hochschulkompass.de/hochschulen/downloads.html, accessed on August, $3^{rd}$ 2020

Figure 3.3: Median Daily Wages across West German Counties in 2015




*Notes:* This figure shows the median daily wage across West German counties in 2015. Median daily wages are measured in Euros.

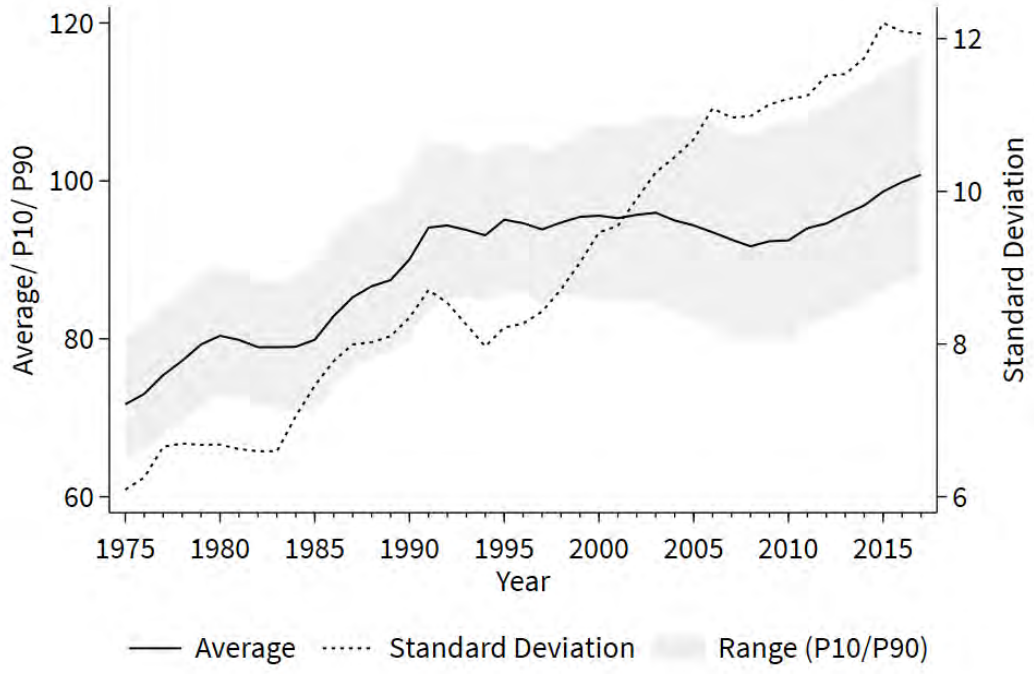
they attract high-quality workers but also because the probability of matching high-quality workers with high-quality firms is larger.

Besides wage differences between urban and rural regions, Figure 3.3 also reveals a North/South divide within West Germany, where median wages in the North are lower than in the South. Across all counties, median daily wages in 2015 range between 75 Euros in the county of Wittmund in Lower Saxony to 148 Euros in Erlangen in Bavaria. The standard deviation of median wages is 12.2 (see also Table A3.1 in the appendix for summary statistics).

These differences in wages have become larger over time. Figure 3.4 illustrates the evolution of regional wage discrepancies between West German counties from 1975 to 2017. The solid line represents the average county median wage, the dotted line presents the standard deviation. The range between the $10^{th}$ and $90^{th}$ percentile of county median wages is shaded in gray. Taking the standard deviation of county median wages as a measure for regional wage inequality, the figure clearly shows that wage inequality between West German counties has

increased since the 1970s. In fact, the standard deviation has doubled in four decades, from 6.1 in 1975 to 12.2 in 2015.

Figure 3.4: Regional Wage Discrepancies 1975–2017




*Notes:* This figure shows the development of average median daily wages of West German counties from 1975 to 2017 in Euros. Additionally, the standard deviation and the range (P10/P90) are displayed.

## 3.5 Empirical Strategy

Identifying the causal effect of university openings on regional wages is challenging since the choice where to establish a new university is endogenous. State governments may decide to open a university in peripheral regions to advance regional development. Private universities may prefer to be located in prospering regions where they have the opportunity to connect their students to the companies nearby.

To estimate the causal effect of university openings on regional wages, we compare regions where a new university has been established (treated regions) to regions where no new university has been opened (untreated regions) within an event study framework. The estimated treatment effects have a causal interpretation if the identifying assumptions, namely the stable unit treatment value assumption (SUTVA) and the parallel trend assumption, hold.

The SUTVA requires that the potential outcome observation in one unit should not be affected by the particular assignment of treatment to other units. The assumption implies that if a

university is established in one region, it should have no effect on the wage distribution of other untreated regions, i.e., there should be no spillover effects from treated regions to untreated regions. If we were to classify a county as treated in case a university was established within the county and as untreated otherwise, SUTVA is unlikely to hold in the German context. The reason is that counties in Germany also include so-called city districts (*Kreisfreie Städte*), which are relatively large in terms of population size but small in terms of area size. Opening a new university in a city district is therefore very likely to have spillover effects on its neighboring counties.

To address this concern, we classify a county as treated if its county centroid is within 75 km of a newly established university. Counties further away from the university form the control group. We believe choosing 75 km is a good compromise between too close a distance and potentially biasing our results due to spillover effects and too long a distance which would lead to wrongly classify untreated counties as treated. To test the robustness of our results with respect to the treatment indicator, we use several alternative cutoffs and further employ continuous rather than binary treatment indicators in Section 3.6.3.

The parallel trend assumption requires that the untreated units provide an appropriate counterfactual of the trend the treated units would have followed in the absence of treatment. The parallel trend assumption may be violated if universities were only established in regions with specific characteristics which are also correlated with diverging regional growth paths. For instance, if treated regions mostly consisted of larger cities with higher wage growth and untreated regions were mainly made up of smaller cities and rural regions, wage trends of treated and untreated regions are unlikely to be parallel, violating the identifying assumption.

Figure 3.5 shows that this is not the case. The figure depicts the location of all universities in our sample established between 1945 and 2018. While there is is a slight accumulation of university openings in cities such as Hamburg, Berlin, Dortmund, Frankfurt, or Munich overall university openings are equally spread out across the country.[4]

To set up our event study formally, we follow Schmidheiny and Siegloch (2019). In our setting, an event is defined as a university opening and the treatment indicator is determined via the distance of a county to this newly established university. In case more than one university has been opened in any given year, we use the distance to the closest university. Since in some regions more than one university was established between 1945 and 2018 (e.g., in Hamburg, Hanover, Frankfurt etc.) we use a generalized event study approach with multiple events.

We limit the event window to $\overline{j} = 30$ years after the university opening and assume that effects are constant afterwards by binning the treatment indicator at the endpoint. In order to assess

[4] Note that in contrast to wage observations, we do not exclude East German universities from our analysis. The reason is that all East German universities in our data were established after 1990. If they are located near the former inner German border and are within 75 km of a West German county, they are likely to have an effect on the county's wage distribution.

Figure 3.5: The Geography of University Openings 1945–2018


*Notes:* This figure shows the location of universities established between 1945 and 2018. University locations are marked with •.

the plausibility of the parallel trend assumption, we test for differences in trends between treated and untreated groups prior to treatment. Here, we limit the window to $\underline{j} = -10$ years before the university opening and again bin the treatment indicator at the endpoint. Taking into account the data requirements of the event study design as well as data availability, we thus observe our dependent variables from 1975 to 2008 and university openings from 1945 to 2018.[5]

Hence, we estimate the following regression equation:

$$Y_{ct} = \alpha_c + \sum_{j=-10}^{30} \beta_j Treatment_{ct}^{j} + \delta_{s,t} + \epsilon_{ct} \tag{3.1}$$

[5] For a given panel of the dependent variable from $[\underline{t}, \overline{t}]$ and an effect window $[\underline{j}, \overline{j}]$, we need to observe treatment status from $\underline{t} - \overline{j}$ to $\overline{t} + |\underline{j}| - 1$ (Schmidheiny and Siegloch, 2019).

The index $c$ refers to the county and the index $t$ to the year. $Treatment_{ct}^{j}$ is the treatment indicator binned at the endpoints, indicating when the university opening has happened relative to $t$. In our baseline specification, we classify a county as treated if its centroid is within 75 km to the newly established university, but use 50 km, 100 km, 125 km, and 150 km as alternative cutoffs in a robustness check. Thus, our treatment indicator is a dummy variable equal to one if the county's geometric center is within the specified distance to the university and zero otherwise. Additionally, we also use three continuous treatment indicators, namely the negative distance, $-km$, the negative logarithm of distance, $-log(km)$, and the inverse of the distance, $1/km$, to the nearest newly established university in an event study approach with multiple events of varying treatment intensity.

We are interested in studying the dynamics of the treatment effects, $\beta_j$, over a window ranging from $\underline{j} = -10$ years before the university opening to $\overline{j} = 30$ years after the opening. In order to standardize $\beta_{-1}$ to zero we drop $Treatment_{ct}^{-1}$ from the regression.[6] $\alpha_c$ denotes county fixed effects. Since education in Germany is primarily the responsibility of the individual German states, we include state–year fixed effects, $\delta_{s,t}$, in the regression to control for unobserved time varying variables at the state-level.

$Y_{ct}$ denotes our outcome variable of interest. For the main part of our analysis, we employ different measure of county wages. In particular, we look at the county median wage as well as the $25^{th}$ and $75^{th}$ wage percentile. We also analyze the effects of university openings on county median wages by education level, occupation group, and industry sector. In an extension of the analysis, we move beyond the effects on wages and analyze how university openings affect the composition of the county's workforce. That is, we estimate the treatment effects on the number of full-time employees, the county's share of employees with no secondary, secondary, and post-secondary education, the share of employees performing unskilled, skilled, complex, and highly complex tasks, as well as the share of employees working in the primary and secondary, the tertiary, and the public administration and education sector. In all specifications standard errors are clustered at the level of labor market regions.[7]

## 3.6 University Openings and Regional Wages

In this section, we present evidence on the impact of university openings on regional wages. We start by estimating the effect of university openings on different percentiles of the county

[6] Note that due to data availability, pre-treatment effects mainly come from universities which were established after 1975, post-treatment effects after 20 years are mainly estimated using university openings before 1990. In our analysis, we thus implicitly assume that pre- and post-treatment effects are constant over time and do not vary, for instance, between universities which were opened in the 1960s and 1970s and universities which were established in the 1990s and 2000s.

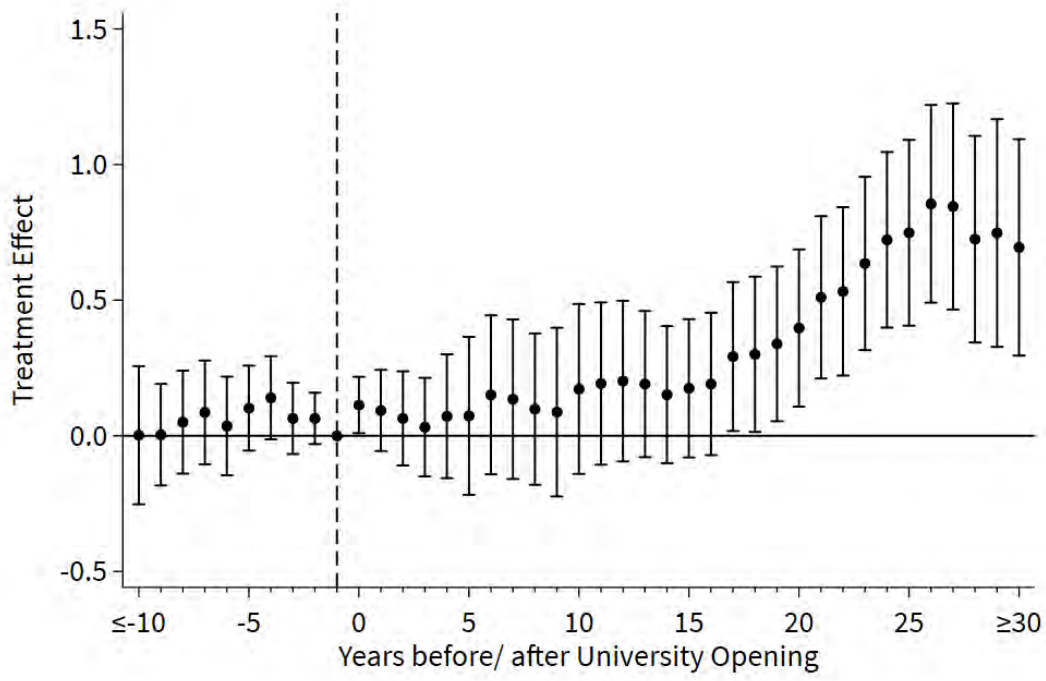
[7] The labor market region is a categorization of German regions which takes into account commuter connections between counties and is comparable to U.S. commuter zones. The 324 West German counties form 204 labor market regions.

wage distribution, namely the $50^{th}$ percentile (i.e., the median wage), the $25^{th}$, and the $75^{th}$ percentile. To gain further insights, we undertake a wide set of additional analyses. First, we evaluate the impact of university openings on median wages by education levels, occupation groups, and industry sectors. Furthermore, we test the robustness of our results with respect to the definition of treatment and estimate heterogeneous treatment effects by university type and sponsorship as well as between urban and rural areas. All regression results are illustrated graphically. Additional regression tables are available on request.

### 3.6.1 Wage Percentiles

Estimating the effect of university openings on different county wage percentiles shows that in the long-run founding a new university has a positive and statistically significant effect on wages in the surrounding counties. Figure 3.6 presents the event study results for the county median wage. Figures 3.7a and Figures 3.7b present the results for the $25^{th}$ and $75^{th}$ wage percentile, respectively. All figures display the time-varying treatment effects from ten years before to 30 years after the university opening as well as 90 percent confidence intervals. The dashed line represents one year before the opening, i.e., our base effect.

Figure 3.6: University Openings and Median Wages



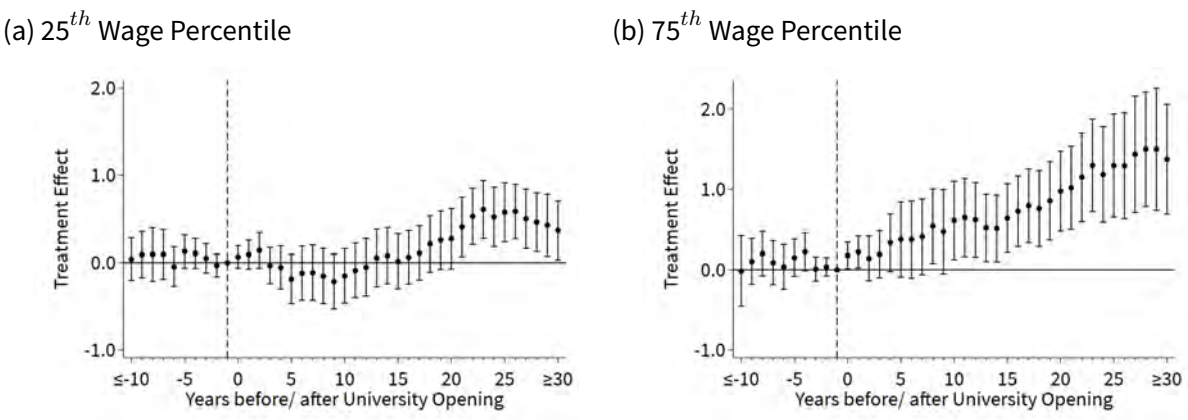

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

Figure 3.6 reveals that establishing a new university starts to statistically significantly increase county median wages 17 years after the opening. After 17 years, the treatment effect is 0.29,

indicating that the increase in median daily wages of counties within 75 km of the newly established university is 29 cents higher than the increase in median daily wages of counties further away. This corresponds to an increase in annual median wages by 105.85 Euros. The size of the effect becomes larger over time. 30 and more years after the opening, the daily (annual) median wage in surrounding counties increases by 70 cents (255.5 Euros). In relation to the sample mean, this translates into an increase in median wages by about 0.8 percent.[8]

The effect of university openings on wages differs along the wage distribution, both in terms of magnitude and timing. In the lower part of the wage distribution, establishing a new university takes longer to show effect and the size of the effect is smaller. Opening a new university starts to increase the $25^{th}$ wage percentile after 21 years. 30 and more years after a new university was established, the $25^{th}$ percentile of daily (annual) wages increases by 37 cents (135.05 Euros). This corresponds to an increase of 0.58 percent in relation to the sample mean. In contrast, wage effects in the upper part of the distribution occur earlier and are markedly larger. The treatment effect on the $75^{th}$ percentile is statistically significant ten years after university opening. 30 and more years after the opening, the treatment effect is 1.38, i.e., opening a university leads to an increase in the $75^{th}$ daily (annual) wage percentile by 1.38 Euros (503.70 Euros) or of about 1.25 percent in relation to the sample mean (see Figure 3.7).

Figure 3.7: Alternative Wage Percentiles




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

Note that all pre-treatment effects are statistically insignificant, independent of the wage percentile. The fact that we observe no differences in trends prior to university openings adds plausibility to the parallel trend assumption. Counties further away seem to provide an appropriate counterfactual for the trend counties within 75 km of the newly established university would have followed had the university not been founded. We therefore conclude

[8] Results in table format are available upon request.

that our identification strategy is valid and that the estimated treatment effects have a causal interpretation.

### 3.6.2 Wages by Education Level, Occupation Group, and Industry Sector

In the previous section, we documented positive wage effects of university openings for different parts of the county wage distribution. This is a first indication that the treatment effects are not solely driven by a direct effect on the wages of employees who either attended or are employed by the newly established university but also come from an indirect or spillover effect of university openings on the wages of other employees. We test this further by estimating the effect of university openings on median wages of different employee subgroups. To be more specific, we analyze the effect on median wages by education level, occupation group, and industry sector.

#### Wages by Education Level

Figure 3.8 illustrates the treatment effect of university openings on median wages of employees without secondary education (Figure 3.8a), with secondary education (Figure 3.8b), and with post-secondary education (Figure 3.8c). The results suggest that opening a new university does not only increase wages of university graduates but also spills over to employees without a university degree.

Treatment effects are strongest for employees with post-secondary education. Their median wage starts to increase ten years after a new university has been established. After 30 and more years, their median daily (annual) wage increases by 2.77 Euros (1010 Euros). This corresponds to a 1.87 percent increase in relation to the sample mean. The treatment effects on median wages of employees with secondary or no secondary education are smaller. 30 and more years after university opening the median wage of employees with secondary education in the surrounding counties increases by 0.36 Euros, or by 0.40 percent in relation to the sample mean. The point estimates for employees with no secondary education are larger, ranging between 0.97 and 1.17 22 to 27 years after the university opening. However, 30 and more years after the opening, the effect is no longer statistically significant.

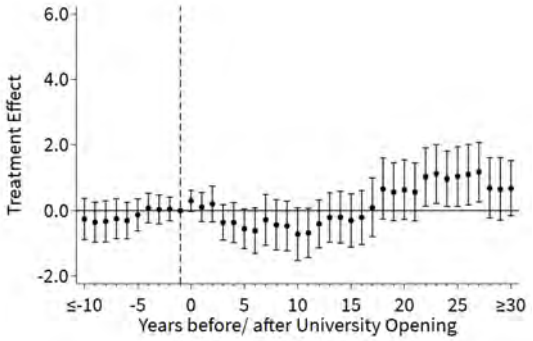
#### Wages by Occupation Group

Figure 3.9 presents the treatment effects on median wages by occupation group. We divide employees into production workers (Figure 3.9a), employees providing commercial and business-related services (Figure 3.9b), and employees providing other types of services (Figure 3.9c).

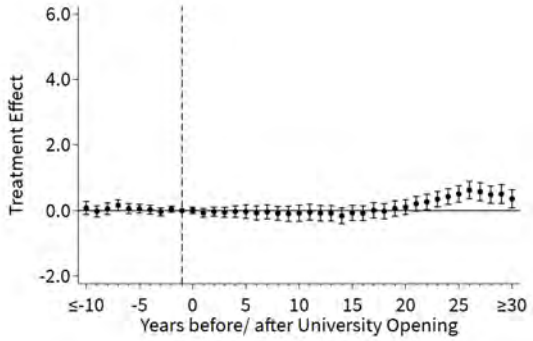
Median wages increase for all occupation groups but the wage effects of opening a university are larger and show effects earlier for employees working as commercial or business-related

Figure 3.8: Median Wages by Education Level

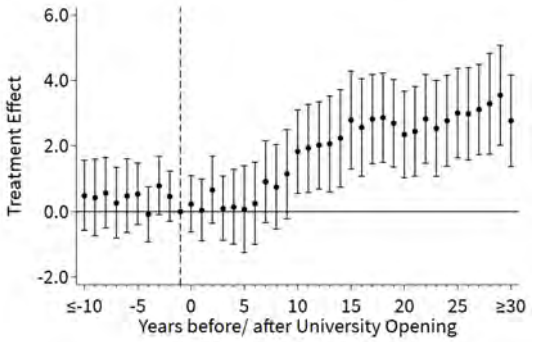
(a) No Secondary Education




(b) Secondary Education




(c) Post-Secondary Education




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

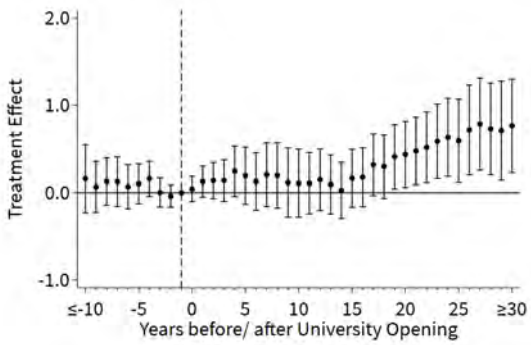
service providers. The median wage of commercial and business-related service providers starts to increase ten years after the university opening. In contrast, the median wage of production workers begins to rise after 19 years and the median wage of employees providing services that are not commercial or business-related (i.e., other services) starts to rise only after 23 years. 30 and more years after a new university has been established, the median daily wage of employees providing commercial and business-related services in counties within 75 km of the university increases by 1.13 Euros. For production workers the effect is 0.76 Euros, for employees providing other services the effect is 0.57 Euros. In relation to the respective sample means, the effects translate into a rise in the median wage of commercial and business-related service providers of 1.32 percent, a rise in median wages of production workers of 0.83 percent, and a rise in the median wage of other service providers of 0.60 percent.

**Wages by Industry Sector**

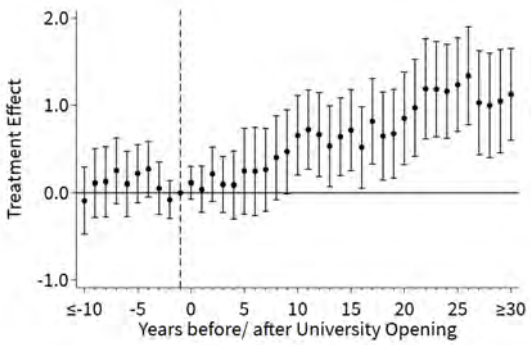
In order to estimate the effect of university openings on median wages of employees working in different industry sectors, we divide employees into employees working in the primary (i.e.,

Figure 3.9: Median Wages by Occupation Group

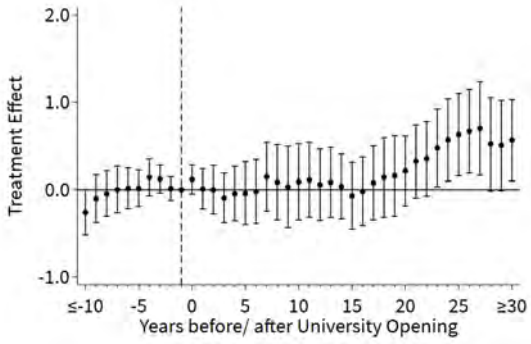
(a) Production




(b) Commercial and Business-Related Services




(c) Other Services



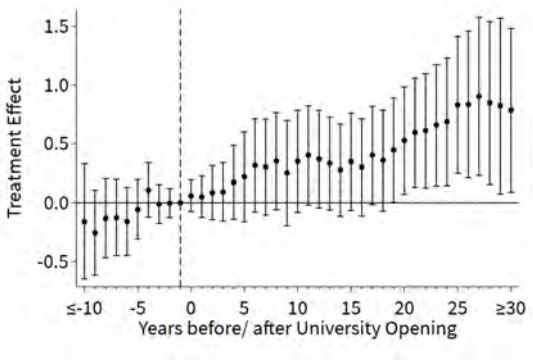

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

agriculture) and secondary (i.e., production) sector and employees working in the tertiary (i.e., services) sector. To ensure that effects on the tertiary sector are not driven by hiring academic and administrative staff for the newly established university, we further subdivide employees in the tertiary sector and analyze the effect on median wages in the public administration and education sector separately. Results are presented in Figure 3.10.

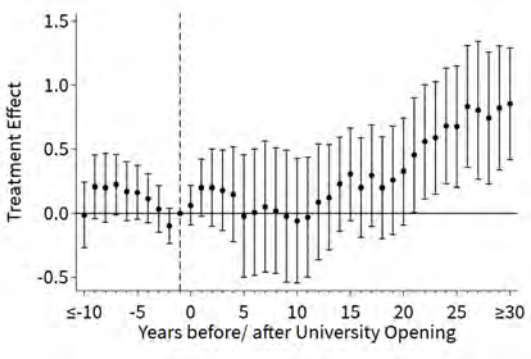
Taking a look at Figure 3.10 reveals that the wage effects of opening a new university are positive in all industry sectors. 30 and more years after a university opening the median wage in the primary and secondary sector increases by 0.79 Euros, (i.e., 0.84 percent in relation to the sample mean). The effect on median wages in the tertiary sector is slightly larger. Here, median wages increase by 0.85 Euros which corresponds to an increase of 1.1 percent in relation to the sample mean. Figure 3.10c further reveals that the wage effects estimated in previous sections are not driven by newly established universities hiring staff. Treatment effects on median wages in the public administration and education sector are for the most part statistically insignificant.

Figure 3.10: Median Wages by Industry Sector

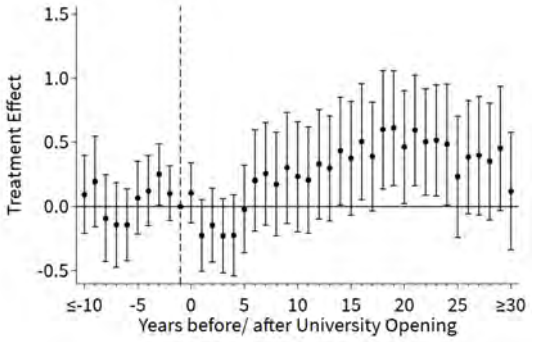
(a) Primary and Secondary Sector




(b) Tertiary Sector w/o Public Administration and Education




(c) Public Administration and Education



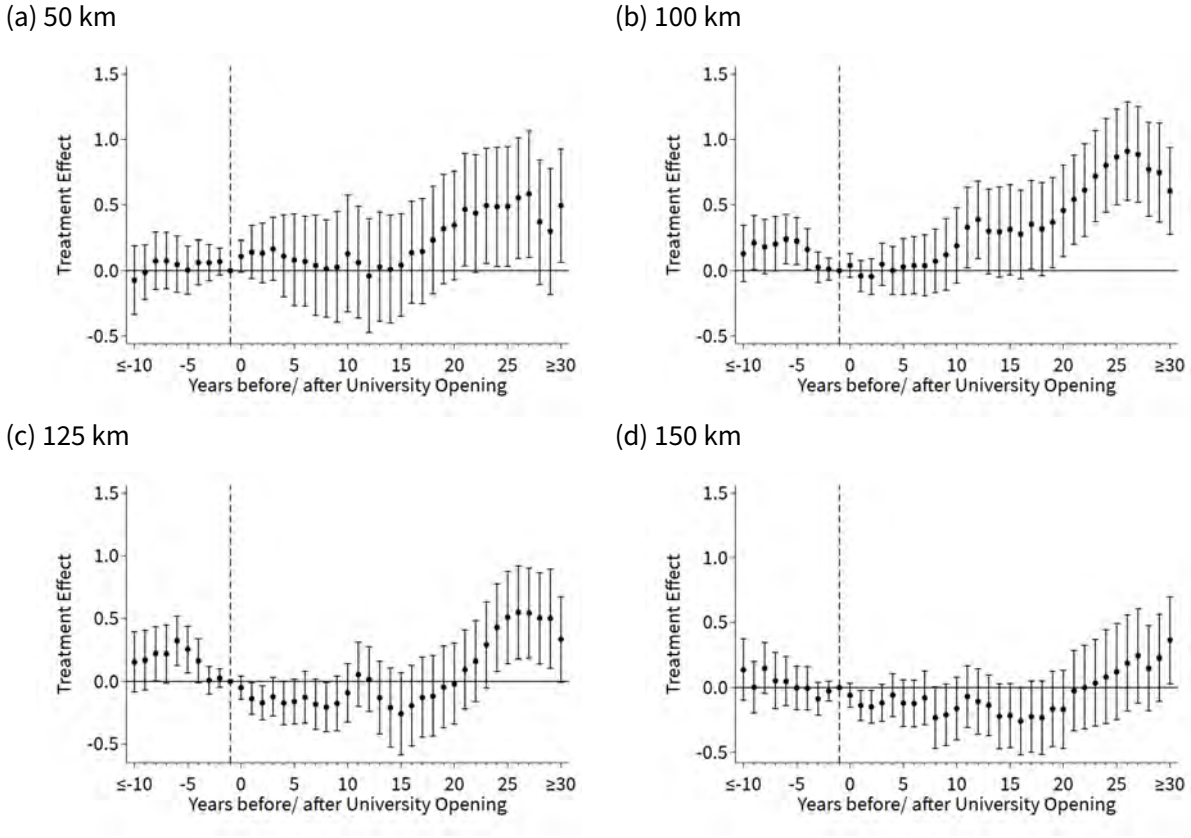

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

### 3.6.3 Robustness Test

In this section, we test the robustness of our results with respect to the definition of treatment. We do this in two ways. First, we alter the cutoff to determine treatment by classifying counties as treated if they are either within 50 km, 100 km, 125 km, or 150 km of a newly established university and as untreated otherwise. Second, rather than defining treatment as a binary variable, we define it continuously and estimate an event study with multiple events of varying treatment intensity. To this end, we use the negative distance to the nearest newly established university, $-km$, the negative logarithm of distance, $-log(km)$, as well as the inverse of distance, $1/km$, to define treatment. Figure 3.11 presents the treatment effects of university openings on county median wages when using alternative binary treatment indicators. Figure 3.12 presents the results for the continuous treatment indicators.

Figures 3.11 and 3.12 show that, independent of the way we define treatment, university openings are positively associated with county median wages. When we use 50 km as cutoff, coefficients are always positive but for most years statistically insignificant. We attribute this

Figure 3.11: Alternative Binary Treatment Indicators: Median Wages




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

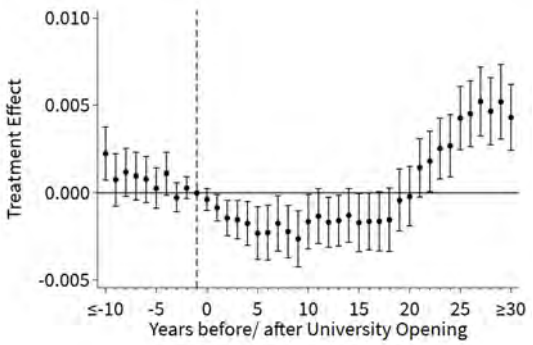
to the fact than only 8.26 percent of county observations are treated when we use 50 km as cutoff and we lack the power to estimate statistically significant results.[9] Moreover, the point estimates are also smaller than the estimates in our baseline results. This may indicate that the closer we move the cutoff to the university, the more likely we are to wrongly classify counties as treated even though they are in close enough proximity to the university to benefit from the opening. When 100 km is chosen as cutoff, results are very similar to the baseline results, both in terms of magnitude and significance.

Figures 3.11c and Figures 3.11d reveal that treatment effects become smaller and statistically insignificant the further we move the cutoff away from the university. When we use 125 km as alternative cutoff, establishing a new university starts to increase county wages only after 24 years. Choosing 150 km as cutoff yields statistically insignificant results in almost all years. Coefficients are also smaller. 30 and more years after the opening, point estimates for the 125 km and 150 km cutoff are 0.33 and 0.36, respectively. We take this as evidence that the

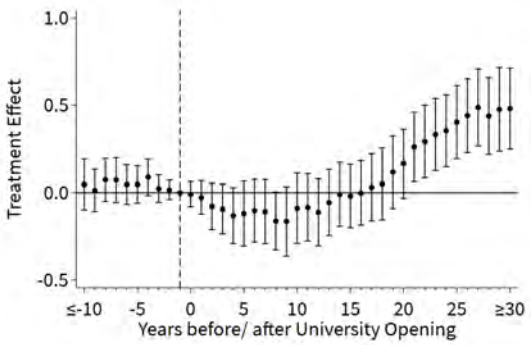
[9] Note that when choosing 75 km as the cutoff 15.31 percent of counties in the sample are treated, for 100 km, 125 km, and 150 km it is 22.79, 33.65, and 38.32 percent, respectively.

Figure 3.12: Alternative Continuous Treatment Indicators: Median Wages

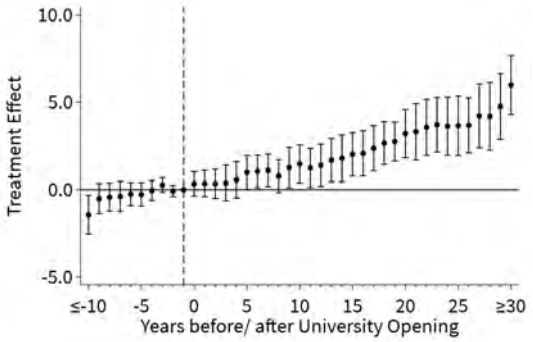
(a) - km




(b) - log(km)




(c) 1/km




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

further we move the cutoff from the university, the more likely we are to wrongly classify untreated counties as treated, biasing our estimates towards zero.

As in the case of the binary treatment indicators, Figure 3.12 shows that in the long-term all continuous treatment indicators yield positive and statistically significant effects of university openings on median wages. Moreover, the figure shows that these wage increases occur earlier, the more weight the continuous treatment indicator puts on shorter distances relative to longer distances. For instance, using the inverse of distance as treatment indicator, which puts most weight on short distances and hardly any weight on longer distances, yields statistically significant and positive treatment effects even after six years (cf. Figure 3.12c).

### 3.6.4 Heterogeneity

Do university openings have heterogeneous effects on county median wages depending on which type of university was opened, who sponsored it, or where it is located? In this section, we analyze heterogeneous treatment effects between universities, universities of applied sciences, or other types of universities, between public, private, and ecclesiastical

universities, as well as between rural and urban counties. In all specifications, we use our baseline specification and define treatment as a binary variable using 75 km as cutoff to classify counties into treated and untreated counties.

**Heterogeneity by University Type and Sponsorship**

In order to estimate heterogeneous treatment effects by university type or sponsorship, we adjust Equation 3.1 and include interaction terms between the treatment variable and three dummy variables indicating either universities, universities of applied sciences, and other university types or public, private, and ecclesiastical universities. That is, we estimate the following equations:

$$\begin{aligned} Y_{ct} &= \alpha_c \\ &+ \sum_{j=-10}^{30} \beta_{j,Uni} Uni \times Treatment_{ct}^{j} + \beta_{j,Appl.} Applied \times Treatment_{ct}^{j} + \beta_{j,Oth.} Other \times Treatment_{ct}^{j} \\ &+ \delta_{s,t} + \epsilon_{ct} \end{aligned} \tag{3.2}$$

and

$$\begin{aligned} Y_{ct} &= \alpha_c \\ &+ \sum_{j=-10}^{30} \beta_{j,Publ.} Publ. \times Treatment_{ct}^{j} + \beta_{j,Priv.} Priv. \times Treatment_{ct}^{j} + \beta_{j,Eccl.} Eccl. \times Treatment_{ct}^{j} \\ &+ \delta_{s,t} + \epsilon_{ct} \end{aligned} \tag{3.3}$$

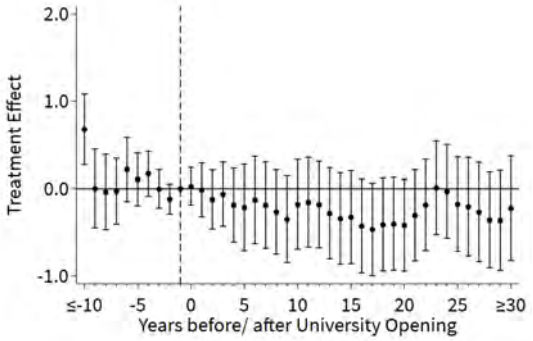
Figures 3.13 and 3.14 illustrate the treatment effects graphically. Figure 3.13 clearly reveals that the effect of university openings on county median wages is driven by establishing universities of applied sciences. Whereas treatment effects are statistically significant for universities of applied sciences, coefficients are insignificant for universities as well as other types of universities. At the same time, Figure 3.14 shows that the effect mainly comes from opening public universities (of all types) rather than private or ecclesiastical institutions.

**Heterogeneity between Rural and Urban Regions**

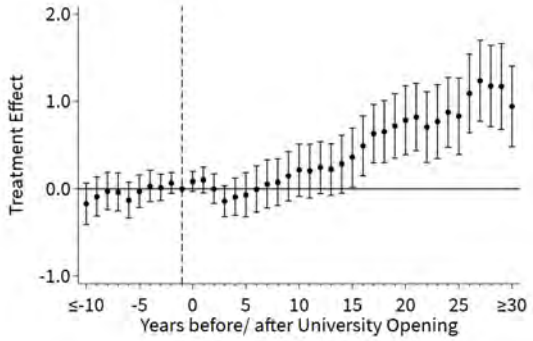
To estimate heterogeneous treatment effects of university openings on county median wages between rural and urban counties, we again adjust Equation 3.1 and include interaction terms

Figure 3.13: Heterogeneous Effects by University Type

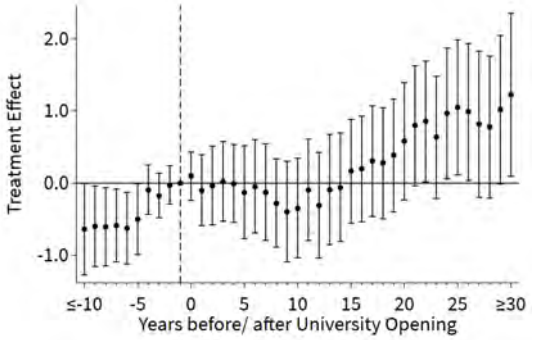
(a) Universities




(b) Universities of Applied Sciences




(c) Other Universities




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

between the treatment variable and two dummy variables indicating whether a county is classified as rural or urban:[10]

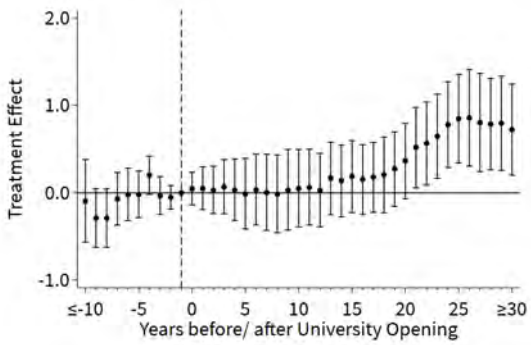
$$Y_{ct} = \alpha_c + \sum_{j=-10}^{30} \beta_{j,Rur.} Rural \times Treatment_{ct}^{j} + \beta_{j,Urb.} Urban \times Treatment_{ct}^{j} + \delta_{s,t} + \epsilon_{ct} \quad (3.4)$$

Results are presented in Figure 3.15. The figure shows that the effect of university openings on county median wages documented in Section 3.6.1 comes from the positive wage effect of establishing a university in urban regions. While treatment effects on median wages of rural counties are statistically insignificant, treatment effects on median wages of urban counties are largely in line with our baseline results, both in terms of magnitude and timing.

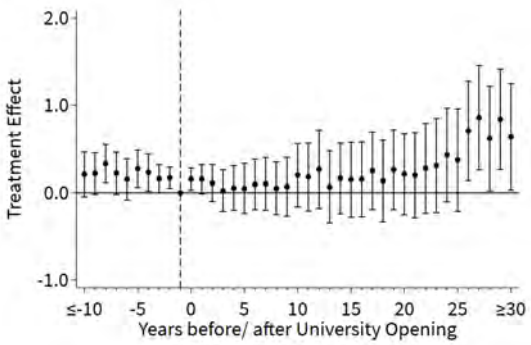
[10] The classification of urban counties and rural counties is taken from the Federal Institute for Research on Building, Urban Affairs and Spatial Developments (*Bundesinstitut für Bau-, Stadt-, und Raumforschung*, BBSR). Basis for the classification is the population density.

## Figure 3.14: Heterogeneous Effects by University Sponsor

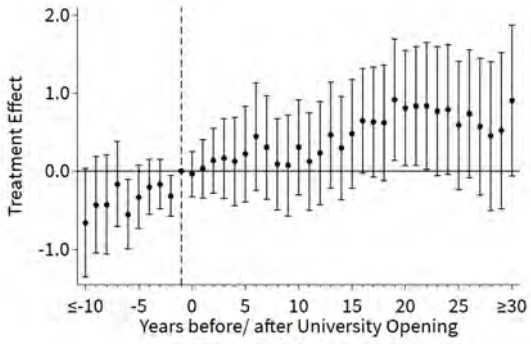
(a) Public Universities




(b) Private Universities



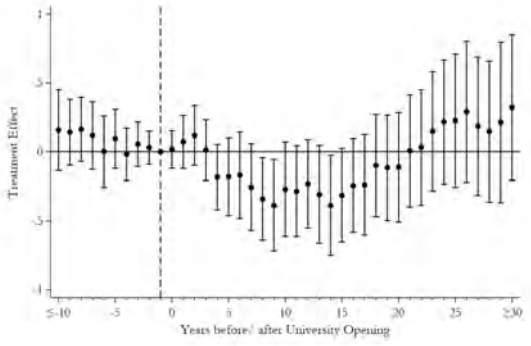

(c) Ecclesiastical Universities



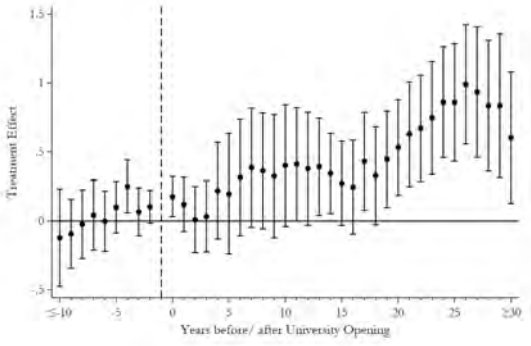

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

## Figure 3.15: Heterogeneity between Rural and Urban Counties

(a) Rural Counties



(b) Urban Counties



*Notes:* The figure displays 90 percent confidence intervals; standard errors are clustered at the level of labor market regions. *Sources:* SIAB, HRK, own calculations.

### 3.6.5 Discussion

To summarize our results so far: Estimating the effect of university openings on regional wages, we have documented that in the long-term establishing a new university has a positive and statistically significant effect on wages in the counties in the neighborhood of the new university. This effect has a causal interpretation and is robust to the way we define treatment. The magnitude and timing of the wage effect differs between different parts of the wage distribution as well as between different employee subgroups.

In particular, wage effects are markedly larger and show effect earlier in the upper part of the county wage distribution, for employees with post-secondary education, as well as for employees providing commercial and business-related services. Moreover, the treatment effects are driven by establishing universities of applied sciences, opening a public rather than private or ecclesiastical university, and establishing universities in urban rather than rural regions.

The fact that we observe effects only for universities of applied sciences may be due to two reasons. First, universities of applied science may have a comparative advantage in regional integration and engagement. While universities are known for their national and international network and internationally-oriented research, universities of applied sciences are more likely to engage in regional industry-connections. As a consequence, universities of applied sciences may adapt their curriculum to the demands of local companies and thus be thematically more aligned with the regional employment structure (Jaeger and Kopper, 2014). Indeed, linking data of German higher education institutions to data of employed academics, Jaeger and Kopper (2014) confirm this hypothesis.

Second, graduates of universities of applied sciences may have a lower regional mobility than graduates of regular universities. Studies analyzing graduates' regional mobility in Germany usually focus on the state-level. Busch and Weigert (2010), for instance, find that graduates of universities of applied sciences have a statistically higher attachment to the state of their studies than university graduates.

Moreover, Krabel and Flöther (2014), studying the determinants of the regional mobility of graduates, show that graduates are more likely to leave rural areas. Kratz and Lenz (2015) and Falk and Kratz (2009) find similar results for Bavarian regions. Overall, our findings regarding the heterogeneity between university types and between urban and rural regions are thus well in line with the literature.

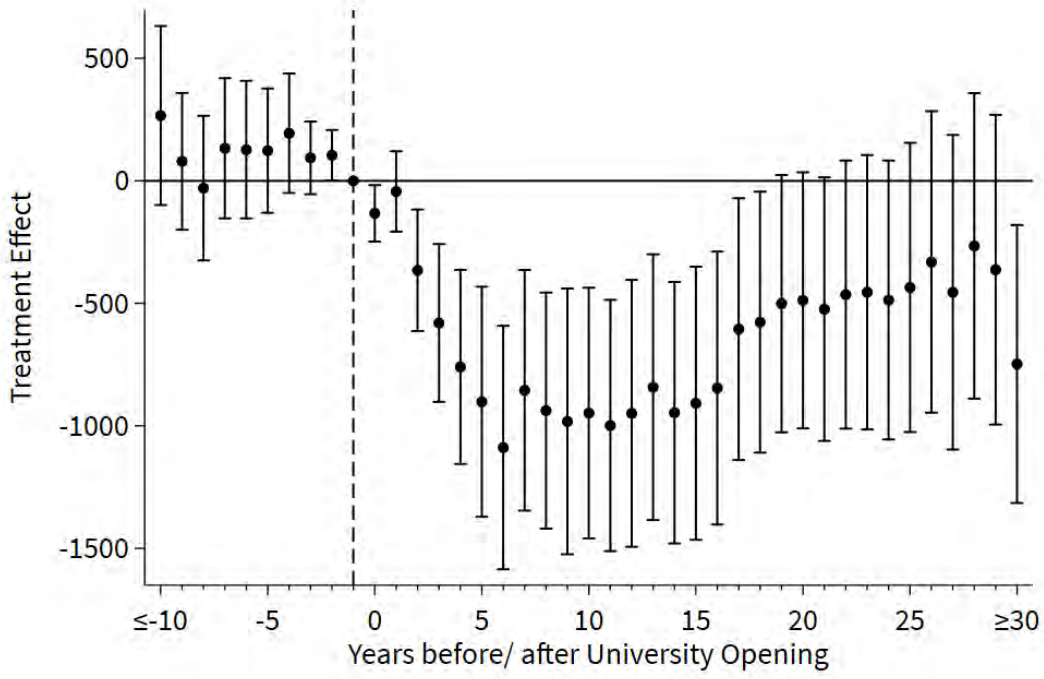
## 3.7 University Openings and the Composition of the Work Force

In the previous section, we have focused on the effect of university openings on regional wages. In this section, we extend our analysis and investigate the impact of university openings on the

number of full-time employees and the composition of the regional work force. In particular, we evaluate the effect of university openings on the share of full-time employees with different levels of education, different job/task requirements and in different industries. Again, we use our baseline specification and define treatment binarily using 75 km as the cutoff. All regression results are illustrated graphically, additional regression tables are available on request.

Before looking at compositional effects, we begin by estimating the overall impact of university openings on full-time employment. Figure 3.16 illustrates the treatment effects on the number of full-time employees in a county. Interestingly, university openings have a negative effects on the number of full-time employees in counties around the new university. This may be due to the fact that establishing a new university may encourage some of the high school graduates, which would otherwise have looked for employment, to pursue a post-secondary degree and enroll in university. Another explanation for the decrease in the number of full-time employees may be that opening a university leads to a shift from labor intensive production/industries to more capital intensive production/industries.

Figure 3.16: Number of Full-time Employees



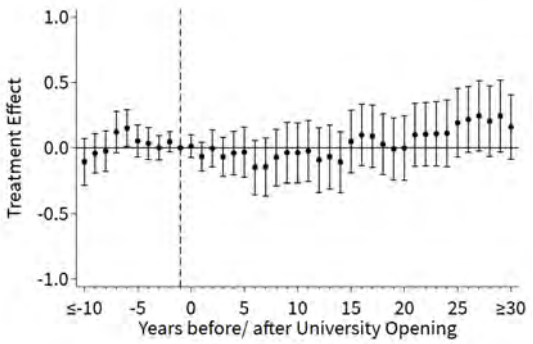

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

### 3.7.1 Education

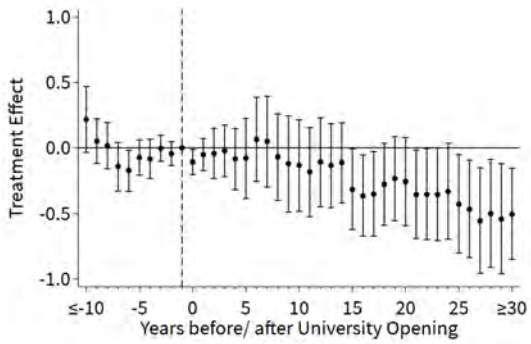
How does establishing a new university change the composition of full-time employees? Figure 3.17 presents the treatment effects of university openings on the share of employees having attained no secondary, secondary, and post-secondary education from ten and more years before to 30 and more years after the opening. The figure reveals that establishing a new university leads to a rise in the share of employees with post-secondary education and a decrease in the share of employees with secondary education.

Figure 3.17: University Openings and Education

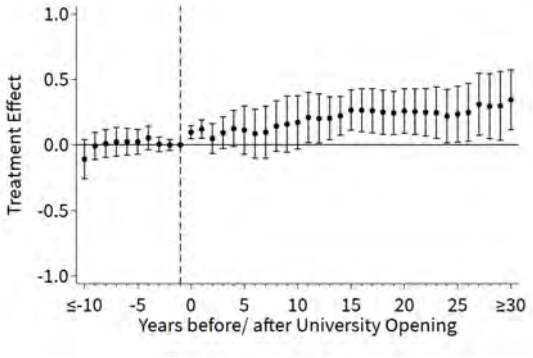
(a) No Secondary Education




(b) Secondary Education




(c) Post-Secondary Education



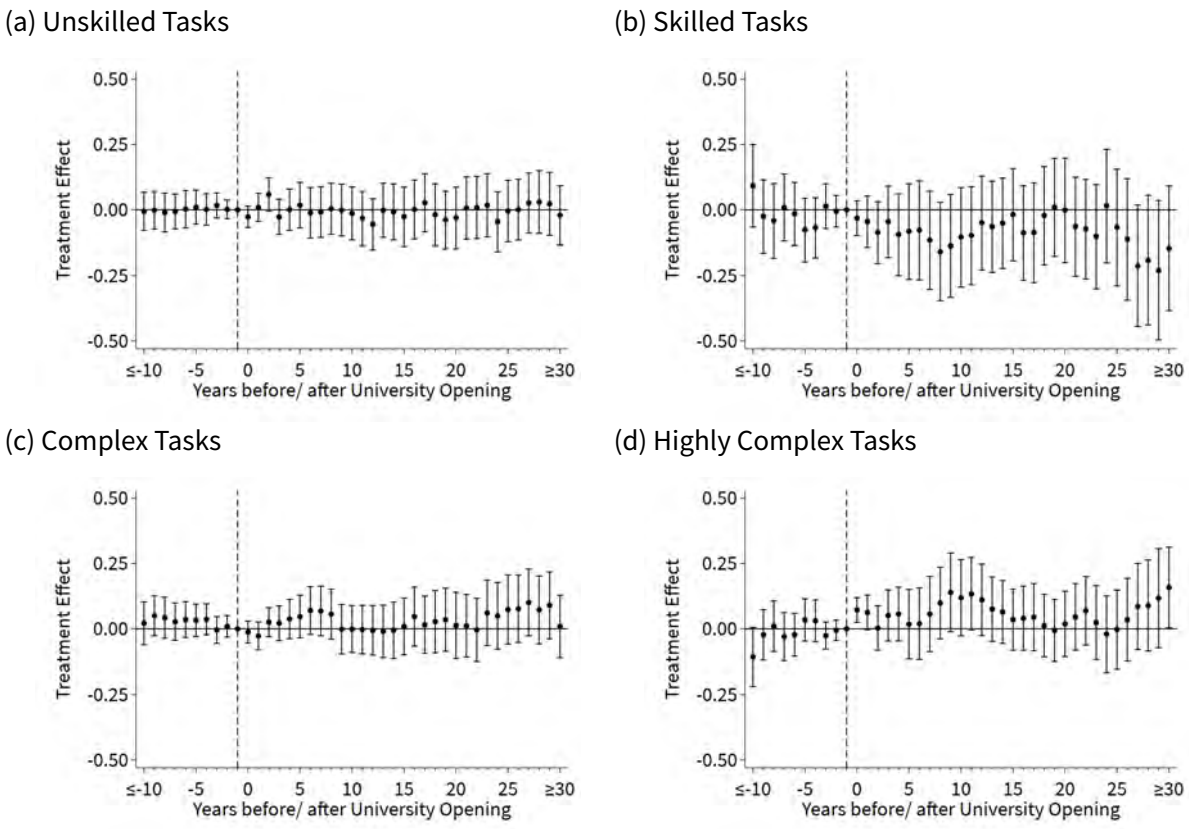

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

The share of employees having attained post-secondary education starts to increase 17 years after university opening. After 30 and more years, the share increases by 0.35 percentage points which corresponds to a strong increase of 5.5 percent in relation to the sample mean. This increase in the share of employees with post-secondary education is accompanied by a decrease in the share of employees with secondary education. Here, the treatment effect after 30 and more years is -0.50. Point estimates on the share of employees without secondary education are negative until 14 years after university opening and turn positive afterwards. However, they are statistically indistinguishable from zero in every year.

### 3.7.2 Task Requirement

Does opening a new university have an impact on the share of employees performing tasks with different skill requirements? In order to answer this question, we use the share of full-time employees performing unskilled, skilled, complex, and highly-complex tasks as outcome variables in Equation 3.1. Figure 3.18 illustrates the results. As one can see, opening a new university does not have an influence on the composition of the work force in terms of task requirements.

Figure 3.18: University Openings and Task Requirements



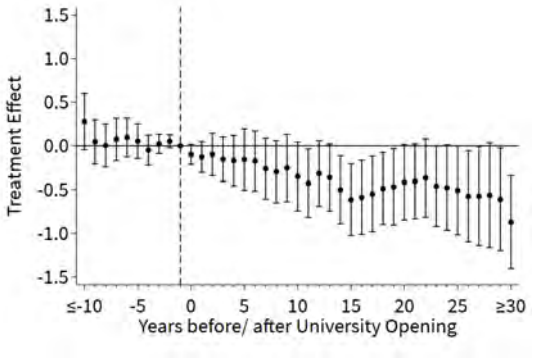

*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

### 3.7.3 Industry Shares

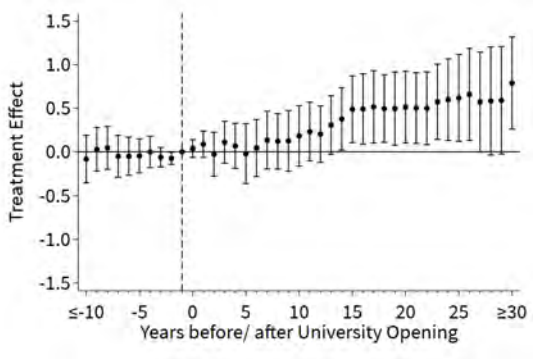
At last, we investigate whether opening a new university causes a shift of employees between industries by estimating the treatment effects on the share of employees working in the primary and secondary sector, the tertiary sector (excluding public administration and education), and in the public administration and education sector. Results are presented in Figure 3.19.

Figure 3.19: University Openings and Employee Shares by Industry

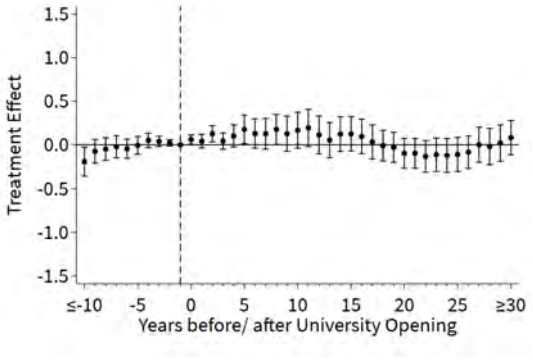
(a) Primary and Secondary Sector




(b) Tertiary Sector w/o Public Administration and Education




(c) Public Administration and Education




*Notes:* The figure plots the event study results. Circles represent point estimates, black lines represent 90 percent confidence intervals. Standard errors are clustered at the level of labor market regions.

Over time, establishing a new university leads to a shift of full-time workers from the primary and secondary sector to the tertiary sector. 30 and more years after the opening, the share of employees working in the primary and secondary sector decreases by 0.87 percentage points, which translates into a decrease of 1.53 percent in relation to the sample mean. Correspondingly, the share of employees working in the tertiary sector increases by 0.79 percentage points (i.e., 2.29 percent in relation to the sample mean). Treatment effects of the share of employees working in the public administration and education sector are statistically insignificant.

## 3.8 Conclusion

In many developed countries, regional discrepancies of wages and income have become an important policy concern. In the past, policy makers used to address such regional inequalities by providing incentives to individual firms to locate in less favored regions. The last two decades, however, saw a shift in regional policy thinking to supporting the endogenous growth of regions by mobilizing its institutional capacity. This shift has led to growing attention on

the contribution of universities and other types of higher education institutions to regional growth. Can establishing new universities contribute to the economic development of its region?

This chapter investigates this question by exploiting the variation in university availability across time and regions to estimate the causal impact of university openings on regional wages and the composition of the workforce in West Germany within an event study framework. Using detailed administrative data from social security records, we show that establishing a new university has a positive long-term effect on wages in its surrounding counties. This is not just a mechanical result of creating new jobs directly in the university. The wage gains take place in other sectors of the local economy.

The magnitude and timing of the wage effect differs between different parts of the wage distribution as well as between different employee subgroups. Wage effects are larger and show effect earlier in the upper part of the wage distribution, for employees with post-secondary education, as well as for employees providing commercial and business-related services. Analyzing heterogeneous treatment effects between universities of different types and sponsorship as well as between rural and urban regions, we find that the treatment effects are driven by establishing universities of applied sciences, opening a public rather than private or ecclesiastical university, and establishing universities in urban rather than rural regions. A possible explanation is that graduates of universities of applied sciences have a stronger tendency to stay in the region where their universities are located, in particular if these are urban regions, where more job opportunities are available.

In an extension of our analysis, we study the impact of university openings on the composition of the county's work force. Our results suggest that establishing a new university leads to a rise in the share of employees with post-secondary education and a decrease in the share of employees with secondary education. It does not seem to influence on the composition of the work force in terms of job requirements but leads to a shift of full-time workers from the primary and secondary industry sector to the tertiary sector.

# Appendix

## Additional Tables

### Table A3.1: Summary Statistics

| | 1975 - 2017 | | 1975 | | 1985 | | 1995 | | 2005 | | 2015 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | mean | sd | mean | sd | mean | sd | mean | sd |
| ***Wage Percentiles:*** | | | | | | | | | | | | |
| P50 | 89.5 | 12.1 | 71.7 | 6.1 | 79.9 | 7.5 | 95.1 | 8.2 | 94.4 | 10.7 | 98.7 | 12.2 |
| P25 | 65.0 | 9.7 | 53.4 | 7.1 | 58.1 | 8.0 | 72.7 | 7.4 | 65.7 | 8.7 | 70.1 | 7.6 |
| P75 | 113.9 | 19.3 | 87.4 | 8.1 | 98.7 | 10.5 | 118.8 | 13.0 | 122.5 | 17.0 | 131.9 | 19.6 |
| ***Employee Shares:*** | | | | | | | | | | | | |
| No Secondary Education | 19.5 | 9.1 | 39.3 | 6.7 | 25.7 | 4.7 | 16.8 | 3.5 | 13.7 | 2.8 | 9.8 | 2.1 |
| Secondary Education | 72.7 | 6.6 | 58.3 | 5.9 | 70.0 | 4.0 | 76.2 | 3.6 | 76.2 | 4.0 | 75.5 | 5.4 |
| Post-Secondary Education | 7.8 | 5.6 | 2.4 | 1.7 | 4.3 | 2.4 | 6.9 | 3.5 | 10.1 | 4.8 | 14.7 | 6.3 |
| Unskilled Tasks | 6.0 | 3.7 | 6.0 | 2.0 | 4.9 | 1.9 | 4.6 | 1.6 | 4.0 | 1.4 | 13.4 | 3.2 |
| Skilled Tasks | 78.1 | 8.3 | 83.4 | 3.2 | 83.3 | 3.7 | 80.7 | 4.2 | 78.5 | 4.7 | 62.4 | 5.2 |
| Complex Tasks | 8.3 | 3.2 | 7.0 | 2.1 | 6.8 | 2.1 | 7.5 | 2.2 | 7.7 | 2.1 | 13.1 | 3.0 |
| Highly-Complex Tasks | 7.7 | 4.1 | 3.7 | 1.8 | 5.1 | 2.2 | 7.3 | 2.9 | 9.9 | 3.6 | 11.2 | 4.1 |
| Primary and Secondary Sector | 55.0 | 15.4 | 64.8 | 14.4 | 60.8 | 14.4 | 56.0 | 13.7 | 49.3 | 14.2 | 46.7 | 14.5 |
| Tertiary Sector w/o | 36.9 | 13.3 | 26.5 | 11.2 | 30.1 | 11.1 | 35.6 | 11.0 | 43.0 | 12.1 | 45.8 | 12.4 |
| Public Admin. and Education | 8.1 | 4.6 | 8.6 | 5.8 | 9.0 | 5.8 | 8.4 | 4.7 | 7.6 | 3.6 | 7.5 | 3.4 |
| Observations | 13932 | | 324 | | 324 | | 324 | | 324 | | 324 | |

*Notes:* Wage percentiles are measured in Euros, employee shares are measured in percent.

# 4 Economic Deprivation and Radical Voting: Evidence from Germany*


## Abstract

This chapter studies the impact of economic deprivation on radical voting. We use a unique data set covering different indicators of economic deprivation as well as federal election outcomes at the county-level in Germany from 1998 to 2017 to examine whether economic deprivation affects the vote share of radical right-wing and left-wing parties. To identify causal effects, we employ instrumental variable estimation. Our results suggest that an increase in economic deprivation has a sizeable effect on the support for radical parties. The higher a county's rate of relative poverty, the average shortfall from the national median income, or the poverty line, the higher the vote shares of radical right-wing parties. We also provide evidence that regional variation in economic deprivation gave rise to the electoral success of the populist right-wing party AfD in the federal election of 2017. Our findings thus indicate that a rise in economic deprivation may undermine moderate political forces and be a threat to political stability.


* This chapter is joint work with Florian Dorn, Clemens Fuest, and Florian Neumeier. It is based on our paper 'Economic Deprivation and Radical Voting: Evidence from Germany', *ifo Working Paper*, 2020.

## 4.1 Introduction

Over the past decades, economic inequality as well as the share of people suffering from (relative) economic deprivation has increased in many industrialized countries. This trend has not only spurred research into the underlying causes and economic consequences, but also triggered heated public debates about its political and social implications. One of the major concerns is that the rise in economic deprivation jeopardizes social cohesion and nourishes radical and populist political movements. The economic pressure experienced by certain groups in society is widely believed to fuel resentment against mainstream political parties as well as the political order itself.

Many pundits thus link the emergence of populist movements and the surge in public support for radical parties in Europe and other parts of the world to the increase in economic deprivation: *Syriza* in Greece, *Podemos* in Spain, 5-Star-Movement (*MoVimento-5-Stelle*) and *Lega* in Italy, *Front National* in France, *Fidesz* in Hungary, the Sweden Democrats (*Sverigedemokraterna*) in Sweden, or the Alternative for Germany (*Alternative für Deutschland*, AfD) in Germany are only a few examples of parties at the far left and far right of the political spectrum that capitalize on growing economic insecurity and deprivation. While the available empirical evidence suggests that, in general, economic deprivation and support for radical views and parties are indeed correlated, so far, evidence on the causal relationship is scarce.

We contribute to the literature by examining the causal effect of economic deprivation on support for radical parties in German federal elections. Germany is particularly well-suited to study the effect of regional economic deprivation on the support for radical parties as its multi-party system covers parties from the entire political spectrum, including far left-wing and far right-wing parties. Arguably, this constitutes an important advantage over studies that focus on countries where only few parties compete in elections, like the U.S. or U.K., as it facilitates the measurement of political polarization. Moreover, by using data on election outcomes, we observe the electorate's revealed support for radical parties. This is an advantage over studies that rely on survey data, which only include stated preferences, not real voting behavior.

To estimate the effect of economic deprivation on the support for radical parties, we exploit regional variation in election outcomes as well as the prevalence and intensity of economic deprivation. More precisely, we estimate regressions linking the share of radical left-wing and right-wing votes to indicators of economic deprivation at the county-level, corresponding to NUTS-3. We use the average shortfall from the national median income (median gap) or the poverty line (poverty gap), as well as the poverty rate to measure economic deprivation. That is, our indicators measure the economic deprivation of county's citizens relative to the national average (not inequality or relative deprivation within regions). To identify causal effects, we follow Boustan et al. (2013) and construct instruments for region-specific measures of economic deprivation that are exogenous to asymmetric economic developments, endogenous political reactions to the rise in the support for radical parties, as well as endogenous sorting of individuals into regions.

In the main part of our analysis, we use data for the period from 1998 to 2017 and cover six federal elections. In an extension of our analysis, we restrict our focus to the federal election held in 2017 and the vote share of the AfD. This is interesting for at least two reasons. First, the AfD is the first nationalist party represented in the German federal parliament of significant size since World War II. Second, survey evidence indicates that AfD supporters—unlike supporters of other radical right-wing parties in Germany—do not differ in their socioeconomic characteristics from supporters of parties at the center of the political spectrum, like the Christian Democratic Party (*Christlich Demokratische Union*, CDU) or the Social Democratic Party (*Sozialdemokratische Partei Deutschland*, SPD), in terms of income, education, or employment status (e.g. Bergmann et al., 2017; Hansen and Olsen, 2019).

Our findings suggest that economic deprivation has a statistically and economically significant effect on the vote share of radical parties. The higher the intensity of economic deprivation in a county, the more successful are radical parties at the polls. For instance, if the poverty gap (median gap) increases by one percentage point, the share of radical right-wing party votes rises, on average, by 1.2 (0.7) percentage points. This effect is even more pronounced when focusing on the AfD votes at the 2017 federal election. Here, a one percentage point increase in the poverty gap (median gap) leads to a rise in the AfD vote share by 4.9 (1.9) percentage points. The size of effect is larger in East Germany than in West Germany.

Our results thus indicate that economic deprivation is an important determinant of the electoral success of radical right-wing parties in Germany. In contrast, our results for radical left-wing parties are more ambiguous in that they are sensitive to the definition of radical parties, and whether East or West German counties are examined. How can these results be reconciled with the observation from survey evidence that AfD voters are not poorer, on average, than other voters (Bergmann et al., 2017; Hansen and Olsen, 2019)? One explanation is that middle or even upper class voters in counties with a high degree of deprivation vote for AfD because they perceive higher economic threat and fear for their status, not because they are poor.

The rest of this chapter is organized as follows. In Section 4.2, we review the related literature, motivate the connection between economic deprivation and the support for radical parties, and explain our contribution. Section 4.3 describes our data. Section 4.4 provides some descriptive evidence on regional variation in economic deprivation and election outcomes in Germany. In Section 4.5, we explain our estimation strategy. Our results are presented in Section 4.6. In Section 4.7, we examine the effect of economic deprivation on election outcomes of the radical party AfD in the federal election of 2017. Section 4.8 concludes.

## 4.2 Related Literature, Hypotheses, and Contribution

### 4.2.1 The Economics of Radical Voting

Economic conditions matter at the polls. In fact, among the various determinants of voting behavior scholars have been analyzing, economic circumstances are typically considered to be among the most important ones (e.g. Fair, 1978; Lewis-Beck, 1990; Lewis-Beck and Stegmaier, 2000, 2013). Consequently, in an attempt to explain the increase in political polarization as well as the rising support for radical parties—especially nationalist ones—various Western countries have been experiencing over the past few years, many scholars focus on economic factors. Recent empirical studies have linked the rise in political radicalism and nationalist (including anti-immigration) sentiments to major macroeconomic trends and events: economic globalization and its adverse consequences (Autor et al., 2020; Colantone and Stanig, 2018; Dippel et al., 2018; Malgouyres, 2017), growing economic insecurity (Algan et al., 2017; Dal Bó et al., 2018; Guiso et al., 2017), the economic strains resulting from the financial and economic crisis (Funke et al., 2016; Mian et al., 2014), as well as rising economic inequality (Duca and Saving, 2016; Garand, 2010; Jesuit et al., 2009; McCarty et al., 2016; Voorheis et al., 2015; Winkler, 2019).[1]

Most approaches linking radical voting to inequality and economic deprivation emphasize the importance of *relative deprivation*. The concept of relative deprivation suggests that individual support for radical (political) views results from an unfavorable comparison with other members of society (Runciman, 1966; Runciman and Bagley, 1969). Plainly speaking, people tend to be more concerned about their relative standing in a society's income distribution than their absolute level of income. An unfavorable social comparison or the fear of social decline are believed to trigger feelings of anxiety and frustration—people are convinced that they are not getting what they are entitled to.

Those feelings, in turn, may foster resentments against the political mainstream as well as the political system itself (Algan et al., 2017; Dal Bó et al., 2018; Mutz, 2018). An inclination toward such sentiments seems to make the economically deprived particularly responsive to the messages of radical political parties and movements. Radical and populist politicians try to appeal to voters experiencing relative economic deprivation by posing as their advocates and discrediting mainstream political parties and political institutions (Mudde, 2007).

The traditional view is that economic deprivation translates into greater support for left-wing parties as they advocate redistributive policies and cater to the needs of those at the bottom of the income distribution (Meltzer and Richard, 1981; Romer, 1975). However, recent studies point out that economic deprivation can increase the popularity of right-wing parties as well. Aggeborn and Persson (2017) develop a theoretical model to explain why low-income

[1] A related literature strand links economic strain to anti-immigrations sentiments as well as right-wing extremist crime. See, for example, Becker et al. (2017), Guiso et al. (2017), Davis and Deole (2015), Billiet et al. (2014), Falk et al. (2011), Facchini and Mayda (2009), and Mayda (2006).

voters are prone to support right-wing (populist) parties. They argue that low-income voters are particularly vulnerable to economic insecurity and depend more heavily on basic public services. In contrast to left-wing parties, right-wing parties oppose spending on global goods such as generous refugee support systems, foreign aid, and environmental protection in favor of basic public services that mainly benefit the domestic population.

Other scholars emphasize that in a highly globalized world, the welfare state is constrained in its ability to redistribute resources and to raise taxes due to the danger of capital flight (Antràs et al., 2017; Sinn, 2003). When redistribution becomes prohibitively costly, protectionist views and hostile attitudes toward globalization may become particularly popular among voters suffering from economic deprivation. As Colantone and Stanig (2018, p.3) put it: "As the losers (of globalization; authors' note) realize that effective redistribution policies are not feasible, the demand for protection emerges as an alternative. This breeds the success of economic nationalism." Consequently, in a country that is highly integrated into the world economy, radical right-wing parties may have a particularly great appeal to voters suffering from economic deprivation.

### 4.2.2 Empirical Evidence on the Association between Deprivation and Polarization

Existing empirical evidence appears to support the conjecture that indicators related to economic deprivation such as unemployment, a low income level, and economic inequality are positively related to political polarization and the support for radical parties.[2] Duca and Saving (2016), Garand (2010), and McCarty et al. (2016) for the U.S., Guiso et al. (2017) and Jesuit et al. (2009) for samples of European countries, Lubbers and Scheepers (2001) for Germany, as well as Dal Bó et al. (2018) and Rydgren and Ruth (2011) for Sweden are just a few of the studies that document such an empirical relationship.

However, the bulk of the empirical literature analyses statistical correlations. Causal evidence on the effect of economic deprivation on political polarization or radical voting is scarce. To the best of our knowledge, the only studies that employ a credible identification strategy to estimate the causal impact of indicators of economic deprivation on the support for radical parties and political polarization are Voorheis et al. (2015), Algan et al. (2017), and Winkler (2019).

Voorheis et al. (2015) and Winkler (2019) adopt the instrumental variable (IV) approach proposed by Boustan et al. (2013) that is also used in the present chapter and explained in detail below. Voorheis et al. (2015) use data on the degree of political polarization in U.S. state

[2] Some scholars argue that unemployed people, lower skilled workers and the 'old middle class' are particularly affected by economic insecurity and perceptions of relative economic deprivation (Dal Bó et al., 2018; Inglehart and Norris, 2017; Rydgren, 2007).

legislatures and state-level data on income inequality covering the years from 2005 to 2011. The authors report a positive effect of income inequality on political polarization.

Winkler (2019) uses survey data from different European countries aggregated at different NUTS levels covering the period from 2002 and 2014. The evidence he provides suggests that an increase in inequality within a region increases the share of people supporting extreme left-wing parties. In contrast, an increase in inequality increases the support for extreme right-wing parties only among older voters.

Algan et al. (2017) use data from European countries at the NUTS-2 level for the period from 2000 to 2016 and examine the effect of crises-driven increases in regional unemployment on vote shares for anti-establishment parties. The authors use regional variation in the pre-crisis share of real estate and housing construction as instrument for regional unemployment. Their estimates suggest that a crisis-induced rise in unemployment increases vote shares of anti-establishment parties, especially populist ones.

This chapter contributes to the literature in several ways. First, by focusing on German counties (corresponding to the NUTS-3 level), this chapter uses data collected at a much more granular regional level than the literature cited above. In Germany, there are currently more than 400 counties with, on average, roughly 170,000 inhabitants. Exploiting variation at such a highly disaggregated regional level increases both our sample size as well as the variation in our measures of economic deprivation and, thus, the power of the statistical tests we perform.

Second, most of the studies listed above use survey data to study the association between economic deprivation and political polarization. In contrast, we assess the support for radical parties using data on election outcomes and, thus, capture the electorate's revealed (and not stated) political preferences.

Third, many studies utilize data from the U.S. Due to its two-party system, it is rather tedious to measure the degree of political polarization in the U.S. The multi-party system in Germany covers parties from the entire political spectrum, including parties at the far right and the far left. This facilitates the measurement of political polarization.[3]

Fourth, our sample period covers two decades and, thus, a considerably larger time span than the studies discussed above. This is particularly important because the degree of economic deprivation typically changes only slowly over time. Finally, in our empirical analysis, we employ different measures of regional economic deprivation, that is, the poverty rate, the poverty gap, as well as the median gap, which has not been done before.

[3] Studies with a focus on the U.S. typically rely on DW-nominate scores to measure the degree political polarization within U.S. politics. DW-nominate scores represent measures of the distance between legislators. These scores indicate how similar or different, respectively, the voting records of legislators are. DW-nominate scores are not without criticism. Only recently, the political science journal *Studies in American Political Development* has devoted a special issue on the advantages and disadvantaged of the DW-nominate scores. See *Studies in American Political Development*, Vol. 30, Issue 2, 2016.

## 4.3 Data Description

To study the influence of economic deprivation on electoral outcomes, we construct a unique panel data set covering more than 400 counties in Germany. Our data set combines county-specific measures of economic deprivation and outcomes of federal elections that took place between 1998 and 2017. During this period, federal elections were held six times; in 1998, 2002, 2005, 2009, 2013, and 2017. Due to territorial reforms, the number of counties varies across our sample period. Therefore, our panel data set is slightly unbalanced.

To construct our variables of main interest, we mainly rely on two sources. Regional measures of economic deprivation are constructed based on microdata from the German Microcensus (*Mikrozensus*). Federal election outcomes at the county-level are provided by the Federal Returning Officer (*Bundeswahlleiter*).

### 4.3.1 The German Microcensus

The Microcensus is a household survey carried out annually since 1957 by the statistical offices of the German states (*Statistische Landesämter*) and administered by the Federal Statistical Office (*Statistisches Bundesamt*). It comprises a representative one percent-sample of the German population, resulting in a sample size of more than 800,000 persons in almost 400,000 households per year. The sample is representative at the regional level. The Microcensus contains information on various demographic characteristics, including the county of residence, employment status, household size, the age of all household members, and household income. For our analysis, we use the waves from 1991 to 2017.

Besides the large number of variables, one major advantage of the Microcensus is its large sample size, which allows us to construct indicators of economic deprivation at the regional level. Moreover, the Microcensus is administered by a federal agency and there is a legal obligation to answer the questions. Hence, item-non-response is not an issue. Also, answers must be truthful and complete. This makes the Microcensus well-suited to study economic deprivation at the county-level in Germany.

To construct our measures of economic deprivation, we use information on monthly net household income. To account for differences in household size, we compute equivalized household incomes using the OECD equivalence scale. In addition, we adjust the income figures for changes in prices using the consumer price index for Germany. Note that the income variable in the Microcensus data set is interval-censored, i.e., respondents are asked to indicate in which income class they are. However, the width of the income classes are rather narrow and the number of income classes is large, varying between 18 and 24, depending on the survey year.

To obtain continuous household income figures, we apply an imputation approach. We estimate a continuous income figure for each household based on information on a household's

income class as well as various socio-demographic characteristics using interval regressions. This imputation technique ensures that the empirical distribution of the continuous income variable fits the shape of the distribution of the income classes and that the income figure computed for each household lies within the borders of the income household's income class (see Royston, 2008).

### 4.3.2 Indicators of Economic Deprivation

A large literature suggests that concerns about personal economic well-being determine preferences for redistribution and protectionism and thereby voting behavior (cf. Section 4.2). When focusing on federal elections, we thus expect that an individual's position in the national income distribution is decisive for her vote. This implies that a regionally aggregated measure of economic deprivation should indicate how residents residing in a county compare to the national average.

In our empirical analysis, we employ three different indicators of economic deprivation that account for the relative economic well-being of a county's citizens compared to the national average. Our first indicator is the poverty rate, i.e., the share of households in a county with an income below the national poverty line $z^{nat}_{pov,t}$. As it is common, we set the poverty line equal to 60 percent of the national median income $z^{nat}_{50,t}$, so that $z^{nat}_{pov,t} = 0.6 \times z^{nat}_{50,t}$.

Our second indicator of economic deprivation is the poverty gap, which is defined as the average shortfall from the national poverty line:

$$Poverty\ Gap_{ct} = 100 \frac{1}{n_{ct}} \sum_{j=1}^{q} \frac{z^{nat}_{pov,t} - y_{cjt}}{z^{nat}_{pov,t}} \tag{4.1}$$

Here, $n_{ct}$ is the number of households in county $c$ and year $t$ that are included in the Microcensus data, $q$ is the number of households with an income below the poverty line, and $y_{cjt}$ is the income of household $j$.

Our third measure of relative economic deprivation is constructed in a similar fashion, but measures the average shortfall from the national median income (instead of the poverty line). We refer to this measure as the median gap. It is constructed as follows:

$$Median\ Gap_{ct} = 100 \frac{1}{n_{ct}} \sum_{j=1}^{r} \frac{z^{nat}_{50,t} - y_{cjt}}{z^{nat}_{50,t}} \tag{4.2}$$

$r$ refers to the number of households in a county with an income below the national median income, while the other variables in Equation 4.2 are defined as above.

### 4.3.3 The German Electoral System and the Definition of Radical Parties

The electoral system in Germany is based on proportional representation and multiple parties run for elections, covering the entire political spectrum from the far left to the far right. At federal elections voters have two votes: the first vote (*Erststimme*) is for a local candidate whom voters would like to see in parliament, the second vote (*Zweitstimme*) is for one of the political parties running for election.[4] In our analysis, we focus on the second votes since they determine the number of seats parties receive in parliament, provided a party passes the five percent election threshold.[5]

We are mainly interested in the vote shares of radical left-wing and radical right-wing parties in the federal elections held between 1998 and 2017. We consider parties to be radical in case the party or a subgroup of party members have been under surveillance of the German Federal Office for the Protection of the Constitution (*Bundesverfassungsschutz*) or its state-level equivalents (*Landesverfassungsschutz*).[6] Parties or party members are put under surveillance if they impose an imminent threat to the free democratic basic order. Table 4.1 provides a list of parties that we label radical right-wing and radical left-wing, respectively. The marks indicate in which federal elections the parties ran.

Our list of radical left-wing parties includes five parties. The Left Party (*Die Linke*) was founded in 2007 when the Party of Democratic Socialism (*Partei des Demokratischen Sozialismus*, PDS)[7] and the Electoral Alternative for Labour and Social Justice (*Wahlalternative Arbeit und soziale Gerechtigkeit*, WASG) merged. It is the most popular left-wing party in Germany and regularly represented in the German federal parliament (*Deutscher Bundestag*).[8]

Besides the Left Party (*Die Linke*), there are several small radical left-wing parties, but none of those has ever passed the five percent election threshold during our sample period. Small radical parties on the far left are communist parties such as the German Communist Party (*Deutsche Kommunistische Partei*, DKP), the Communist Party of Germany (*Kommunistische Partei Deutschlands*, KPD), the Marxist-Leninist Party of Germany (*Marxistisch-Leninistische*

[4] The candidate who receives the majority of first votes in an election district is directly elected to the parliament. The distribution of seats in the parliament is, however, solely determined by the share of second votes a party receives.

[5] Note that the five percent threshold is not binding if a party wins at least three election districts directly by the first vote. In all federal elections in Germany since 1990, this occurred only once in 1994, when four candidates of the leftist Party of Democratic Socialism (PDS) received the majorities of first votes in their election districts. As result, the party got in total 30 seats in parliament, corresponding to its 4.4 percent vote share of second votes.

[6] We also define parties as radical if they cooperate in elections with other parties that are monitored by the German Federal Office for the Protection of the Constitution or its state-level equivalents.

[7] The PDS was founded in 1990 and is the successor of the Socialist Unity Party of Germany (*Sozialistische Einheitspartei Deutschlands*, SED), the communist party governing the German Democratic Republic (*Deutsche Demokratische Republik*, DDR) between 1949 and 1989.

[8] In the first unified German federal elections in 1990, the Left Party received only 2.4 percent of the second votes. However, the party was represented in the parliament with 17 seats because of a one-time exception that was made for parties that won at least five percent of all votes in the former German Democratic Republic.

Table 4.1: Radical Parties at Federal Elections in Germany 1998–2017

| | Federal Elections in Germany | | | | | |
|---|---|---|---|---|---|---|
| | 1998 | 2002 | 2005 | 2009 | 2013 | 2017 |
| ***Radical Right-wing Parties:*** | | | | | | |
| ADM | | | | x | | |
| AfD | | | | | x | x |
| BfB* | x | | | | | |
| Büso | x | x | x | x | x | x |
| Die RECHTE* | | | | | x | x |
| DM | | | | | | x |
| DVU* | x | | | x | | |
| NPD* | x | x | x | x | x | x |
| Pro Deutschland* | | | | | x | |
| REP (Republikaner)* | x | x | x | x | x | |
| Volksabstimmung* | x | | x | x | x | x |
| 50plus | | | x | | | |
| ***Radical Left-wing Parties:*** | | | | | | |
| Die LINKE (PDS) | x | x | x | x | x | x |
| DKP* | | | | x | | x |
| KPD* | | x | | | | |
| MLPD* | x | | x | x | x | x |
| SGP* | x | | x | x | x | x |

*Notes:* *indicates parties also included in the narrow definition.
*Abbr.:* ADM (Allianz der Mitte), AfD (Alternative für Deutschland), BfB (Bund freier Bürger), Büso (Bürgerrechtsbewegung Solidarität), DM (Deutsche Mitte), DVU (Deutsche Volksunion), NPD (Nationaldemokratische Partei Deutschlands), PDS (Partei des Demokratischen Sozialismus), DKP (Deutsche Kommunistische Partei), KPD (Kommunistische Partei Deutschlands), MLPD (Marxistisch-Leninistische Partei Deutschlands), SGP (Sozialistische Gleichheitspartei).

*Partei Deutschlands*, MLPD), and the Trotzkyist oriented Party for Socialist Equality (*Sozialistische Gleichheitspartei*, SGP).

On the far right, twelve parties ran in German federal elections since 1998. The populist party Alternative for Germany (*Alternative für Deutschland*, AfD) is the most successful radical right-wing party in Germany since 1945. The AfD started to run for elections in 2013 and entered the European parliament one year later, i.e., in 2014. However, despite its Euro-skepticism, the AfD was not a radical right-wing party in its early years, but rather a conservative, market-liberal party (see Arzheimer, 2015; Schmitt-Beck, 2017). Since 2015, however, the AfD became more and more radical after several leading moderate politicians left the party. The nationalist and radical fraction took over power and clearly favored anti-immigration policies, emphasized German nationalism, and provoked distrust in the political order. This new radical right-wing party was successful in several state elections held in 2015 and 2016. In 2017, the AfD entered the German federal parliament for the first time. The AfD received a vote share of 12.6 percent and became the third largest party in parliament.

Besides the AfD, there are eleven other radical right-wing parties, the most prominent ones being the National Democratic Party of Germany (*Nationaldemokratische Partei Deutschlands*, NPD), the German People's Union (*Deutsche Volksunion*, DVU; merged with NPD in 2011), and the Republicans (*Republikaner*, REP). While none of these parties was ever represented in the

federal parliament, they do have regional strongholds and entered some state parliaments in the past. Moreover, the NPD has won a seat in the European parliament in 2014, after the three percent threshold was removed by the Federal Constitutional Court of Germany (*Bundesverfassungsgericht*). Besides AfD, NPD, DVU, and REP, there is a number of other radical right-wing parties that ran for federal elections during our sample period, such as the nationalist Union of Free Citizens (*Bund freier Bürger*, BfB), the Right Party (*Die Rechte*), Pro Germany (*Pro Deutschland*), the party Popular Referendum (*Volksabstimmung*), and the Civil Rights Movements Solidarity (*Bürgerrechtsbewegung Solidarität*, BüSo).[9]

To test the sensitivity of our results with regard to the definition of radical parties, we also employ a narrow definition. In the narrow definition, we only label a party radical in case the party as a whole is under surveillance of the Office for the Protection of the Constitution. This reduces the number of radical right-wing parties from twelve to seven and the number of radical left-wing parties from five to four. Note that the two largest radical parties, i.e., the Left Party (*Die Linke*) and the AfD, are excluded from the narrow definition.

As a further robustness test, we also estimate the impact of relative economic deprivation on the vote shares of established parties. Our definition of established parties includes the Social Democratic Party (*Sozialdemokratische Partei Deutschland*, SPD), the Green Party (*Bündnis90/Die Grünen*), the Christian Democratic Party (*Christlich Demokratische Union*, CDU), the Christian Social Union (*Christlich-Soziale Union*, CSU), and the Free Democratic Party (*Freie Demokratische Partei*, FDP). During our sample period, each of these parties was a coalition member of the federal government for at least one legislative period.

### 4.3.4 Control Variables

In our empirical analysis, we include several control variables describing the demographic and economic situation in a county. We control for the population share of different age groups, population density, the unemployment rate, the share of recipients of social transfers, the shares of graduates from different schooling tracks (no degree (reference category), lower secondary degree (*Hauptschule*), intermediate secondary degree (*Realschule*), higher secondary degree (*Gymnasium*)), and the share of foreigners. Population density figures are provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Developments (*Bundesinstitut für Bau , Stadt-, und Raumforschung*, BBSR). The share of foreigners is taken from the German Regional Database (*Regionaldatenbank Deutschland*) as well as the statistical offices of the German states (*Statistische Landesämter*). Information on school graduates comes from the Federal Statistical Office (*Statistisches Bundesamt*). The remaining control variables are calculated based on individual responses from the German Microcensus (see Section 4.3.1).

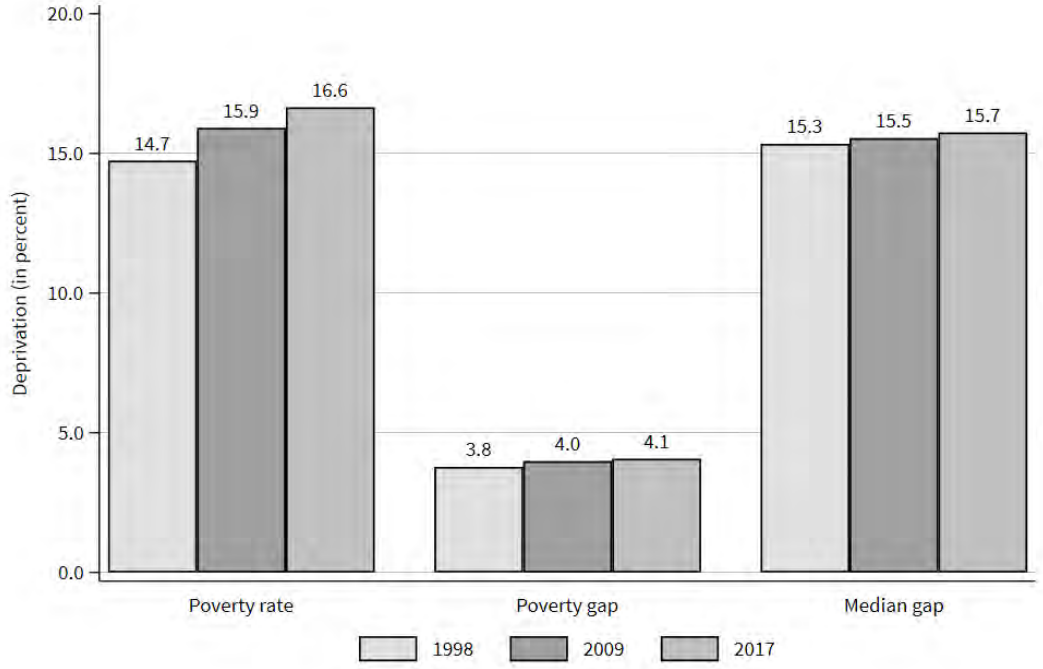
[9] Note that many scholars studying right-wing extremism in Germany only include the AfD, NPD, DVU, and REP to their lists of radical right-wing parties, as they are the largest ones.

## 4.4 Descriptive Statistics

### 4.4.1 Regional Variation in Economic Deprivation

Figure 4.1 illustrates how the average realizations of the economic deprivation indicators developed over the past 20 years. Between 1998 and 2017, the average degree of relative economic deprivation at the county-level in Germany increased slightly. The share of households with an income below the poverty line grew from 14.7 percent in 1998 to 16.7 percent in 2017. Similarly, the average shortfall from the poverty line (median income), that is, the poverty gap (median gap), rose from 3.8 (15.3) percent to 4.1 (15.8) percent.

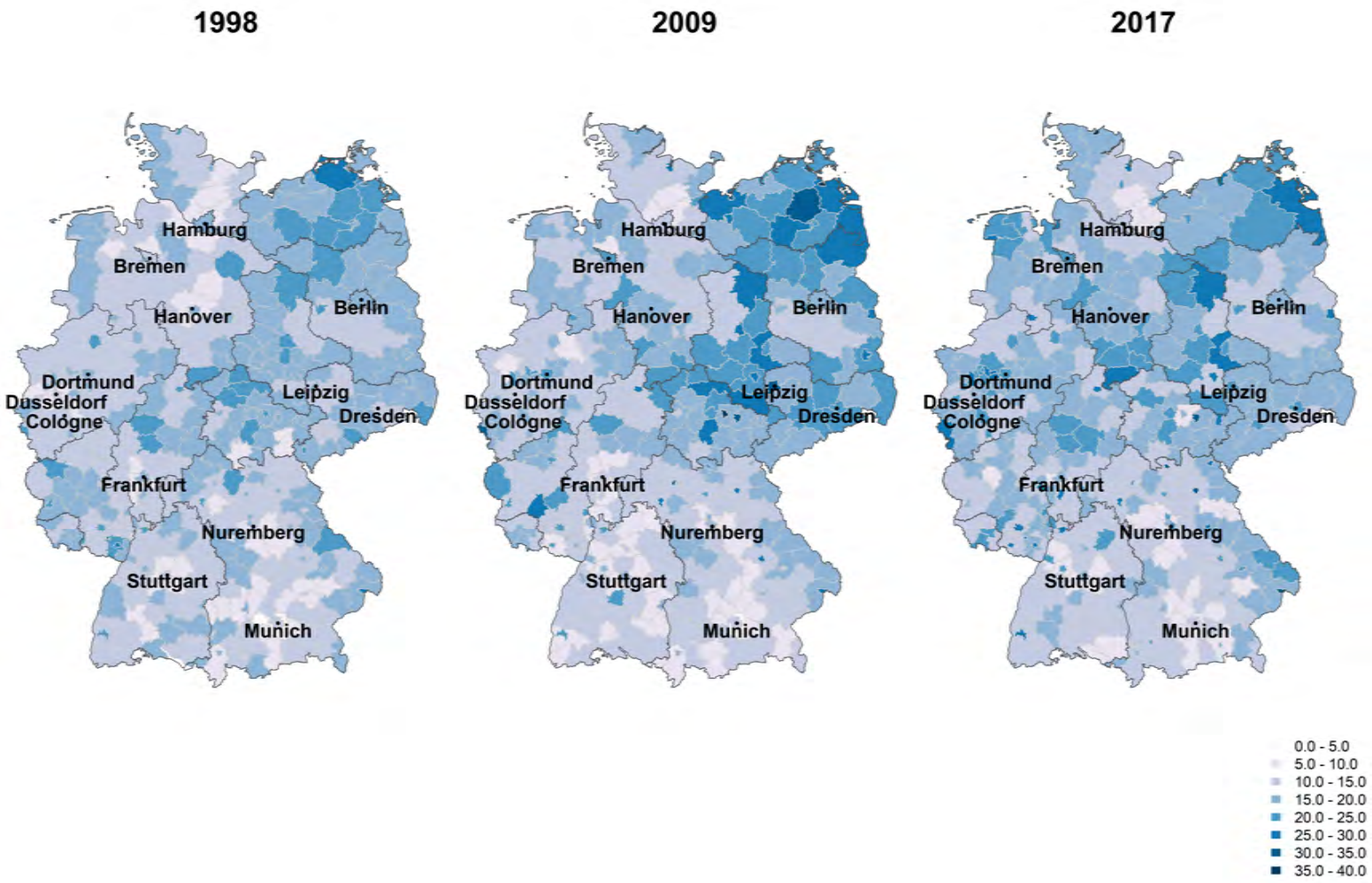
Figure 4.1: Economic Deprivation over Time




*Notes:* This figure shows the average poverty rate, poverty gap, and median gap of German counties in 1998, 2009, and 2017. The poverty rate, the poverty gap, and the median gap are measured in percent.

Figure 4.2 shows the realizations of the poverty rate in 1998, 2009, and 2017 at the county-level. The figure reveals that the extent of economic deprivation varies considerably across regions. Particularly pronounced are the differences between West and East German counties as well as between North and South. Interestingly, it appears that the differences between West and East Germany became smaller over time, while the North/South divide grew.

Figure 4.2: Poverty Rates across German Counties

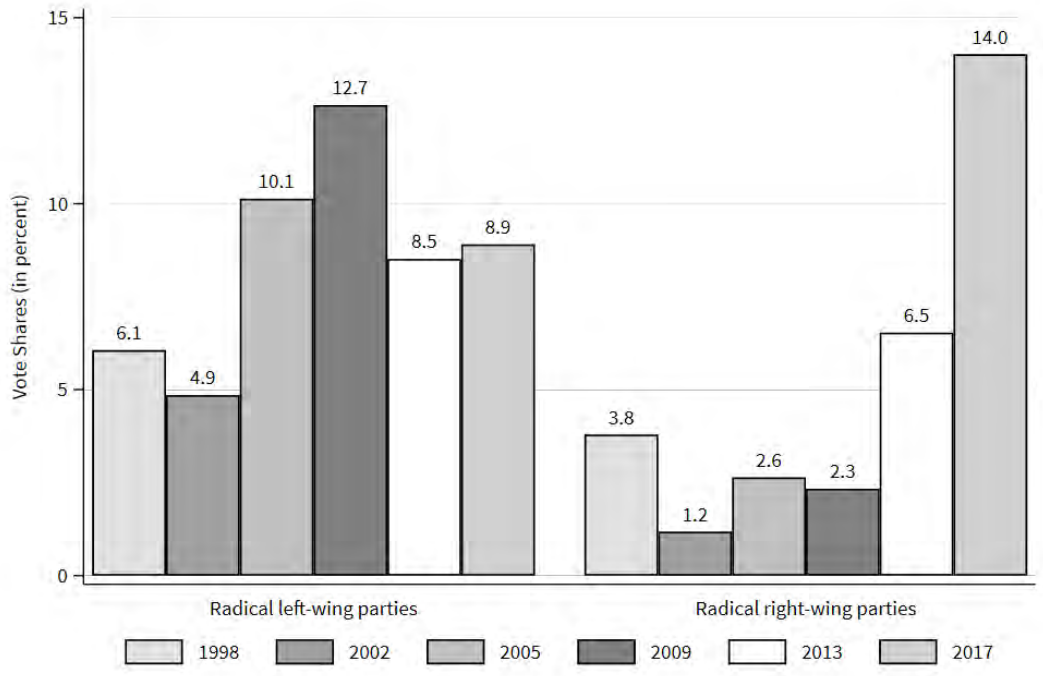



*Notes:* This figure shows the poverty rate across counties for 1998, 2009, and 2017. The poverty rate is measured in percent.

### 4.4.2 Support for Radical Parties

Figure 4.3 shows the average vote shares of radical right-wing and left-wing parties at the federal elections held between 1998 and 2017. Until 2017, radical left-wing parties have consistently been more successful at the polls than radical right-wing parties. This is mainly due to the popularity of the socialist Left Party and its predecessor, the PDS, in East Germany, where these parties have managed to always receive roughly one fifth of the votes. Many pundits link the noticeable jump in the average vote share of radical left-wing parties at the 2005 federal election to the so-called Hartz reforms, which led to a liberalization of the German labor market and were implemented by the left-wing coalition government consisting of the SPD and the Green Party. This resulted in many voters turning away from the SPD and Green Party and turning to the Left Party.

Figure 4.3: Average Vote Shares in German Counties




*Notes:* This figure shows average county vote shares of radical left-wing and right-wing parties at federal election between 1998 and 2017. Vote shares are measured in percent.

In 2013, however, there has been a notable rise in the share of votes for radical right-wing parties, which is entirely driven by the success of the newly founded right-wing populist party AfD. The AfD was founded in April 2013 to oppose German federal policies concerning the eurozone crisis and just missed the five percent election threshold in 2013. In 2017, the AfD received 12.6 percent of the votes and became the third-largest party in the federal parliament, having completed the turn from a Eurosceptical conservative party to a radical right-wing party favoring anti-immigration policies.

Thus, whereas in 1998 the combined county vote shares of radical right- und left-wing parties was on average 9.9 percent, it more than doubled to 22.9 percent in 2017. However, these averages conceal substantial differences in voting outcomes between East and West Germany. East German counties exhibit considerably larger vote shares for radical parties. This is not only due to the success of the Left Party (*Die Linke*), but also the AfD enjoys greater popularity in the East than in the West. In 2017, the average vote share of radical left-wing (right-wing) parties was 17.2 (23.4) percent in East German counties and 7.0 (11.8) percent in West German counties (see Figure A4.1 in the appendix).

## 4.5 Empirical Strategy

To study the association between economic deprivation and support for radical parties, we estimate the following empirical panel data model:

$$Y_{ct} = \alpha_c + \beta Deprivation_{ct} + \gamma^{'} X_{ct} + \delta_t + \epsilon_{ct} \tag{4.3}$$

Index $c$ refers to the county and index $t$ to the year of the federal election. Our sample covers six federal elections: 1998, 2002, 2005, 2009, 2013, and 2017. We use two dependent variables in our empirical model (see Section 4.3.3): the vote share of radical right-wing parties and the vote share of radical left-wing parties. $Deprivation_{ct}$ is a measure of regional economic deprivation. We consecutively employ three deprivation measures: (i) the poverty rate, (ii) the poverty gap, and (iii) the median gap (see Section 4.3.2). The vector $X_{ct}$ includes the control variables described in Section 4.3.4. Finally, $\alpha_c$ is a county fixed effect that is included to account for time-invariant regional-specific factors related to economic conditions and $\delta_t$ is a year fixed effect included to capture the effect of nation-wide events.

Identifying the causal effect of economic deprivation on voting behavior is challenging since there are several confounding factors that are correlated with both election outcomes and regional economic conditions. First, households may sort into regions depending on their socio-demographic characteristics as well as political preferences. For example, households may prefer to live among people who are similar to them with regard to lifestyle and political views. Spatial segregation of households based on their economic situation may also occur due to regional differences in labor market conditions, housing prices, and costs of living. All those factors could also be related to election outcomes, implying that omitting them from the regression would lead to biased estimates when using ordinary least squares (OLS) estimation to identify the parameters of Equation 4.3.

Unfortunately, the data we would need to control for those factors are typically not available at the county-level, and neither are suitable proxy variables. Furthermore, there are a number of regional characteristics that are potentially correlated with both regional economic deprivation and voting behavior such as, for example, factors related to labor supply in a

county, household structure, geographic features, etc. While some important variables can be controlled for, we cannot exclude the possibility that there are other relevant variables we cannot observe.

To address concerns regarding biased OLS estimates due to the endogeneity of our covariates, we construct instrument variables for our deprivation measures that are similar to the instrument proposed by Boustan et al. (2013). The construction proceeds in four steps. In step one, we compute the average household income for each income percentile of the national income distribution and for all survey years (i.e., 1991–2017). In the second step, we compute percentile-specific annual national income growth rates for each survey year. In step three, we focus on household incomes in a base year, determine to which percentile of the national income distribution each household in that base year belongs, and multiply each household's income with the percentile-specific annual national income growth rates. That way, we obtain a time-series of hypothetical incomes for each household that we observe in the base year. In the final step, we use these hypothetical incomes to compute counterfactual economic deprivation measures which we then use as instruments for the actual realizations of the regional deprivation measures.

The counterfactual deprivation measures indicate how regional economic deprivation would have developed in the absence of inward and outward migration and if each household's income would have changed over time in accordance with the percentile-specific national average. Consequently, our instruments only capture changes in the regional income distribution that are driven by national trends and cannot, by design, be influenced by county-specific trends such as mobility into and out of regions or asymmetric economic and political developments (Boustan et al., 2013). The cross-sectional variation in our instruments stems entirely from the variation in the base year's income distribution, whereas the time-variation comes from the percentile-specific income growth rate at the national level.

The results of our first-stage IV regressions demonstrate that the instruments are highly relevant. The coefficients of all instrumental variables are highly significant with coefficient estimates that are close to unity.[10] The relevance of our instruments is further indicated by the Cragg-Donald F statistics for exclusion restriction tests, which are far larger than the critical values proposed by Stock and Yogo (2005) (cf. Section 4.6.2).

An additional challenge specific to the use of county-level data in Germany is that the number of counties in East Germany has changed considerably after German unification due to various administrative-territorial reforms. For example, from 1990 to 1996, the number of counties in East Germany (excluding East-Berlin) decreased from 215 to 111. For this reason, we are forced to use 1997 as our base year for the construction of our instruments for East German counties. For West Germany, our base year for the constructions of the instrumental variables is 1991.

[10] Results available on request.

## 4.6 Empirical Results

### 4.6.1 Main Results

We start with the results of OLS estimation, which are presented in Table 4.2. The left panel shows the results for radical left-wing parties, the right panel for radical right-wing parties.

Table 4.2: Support for Radical Parties—OLS

| | Radical Left-wing Parties | | | Radical Right-wing Parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.062*** | | | 0.013 | | |
| | [0.007] | | | [0.455] | | |
| Poverty Gap | | 0.152** | | | -0.008 | |
| | | [0.016] | | | [0.849] | |
| Median Gap | | | 0.123*** | | | -0.022 |
| | | | [0.001] | | | [0.428] |
| Unemployment | 0.317*** | 0.325*** | 0.303*** | -0.429*** | -0.423*** | -0.417*** |
| | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] |
| Transfer Recipients | -0.002 | 0.007 | -0.005 | 0.003 | 0.009 | 0.014 |
| | [0.975] | [0.876] | [0.909] | [0.935] | [0.781] | [0.678] |
| Population Density | 7.242*** | 7.118*** | 7.180*** | -7.336*** | -7.435*** | -7.482*** |
| | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] |
| Age 15 - 24 | 0.237*** | 0.232*** | 0.236*** | -0.420*** | -0.417*** | -0.416*** |
| | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] |
| Age 25 - 34 | 0.202*** | 0.197*** | 0.206*** | -0.271*** | -0.268*** | -0.268*** |
| | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] |
| Age 35 - 44 | 0.178*** | 0.169*** | 0.183*** | -0.257*** | -0.259*** | -0.262*** |
| | [0.006] | [0.008] | [0.005] | [0.000] | [0.000] | [0.000] |
| Age 45 - 54 | 0.167*** | 0.160*** | 0.179*** | -0.228*** | -0.230*** | -0.234*** |
| | [0.001] | [0.001] | [0.001] | [0.000] | [0.000] | [0.000] |
| Age 55 - 64 | 0.077* | 0.069* | 0.085** | -0.161*** | -0.164*** | -0.168*** |
| | [0.068] | [0.096] | [0.047] | [0.000] | [0.000] | [0.000] |
| Age 65+ | 0.110*** | 0.107*** | 0.111*** | -0.207*** | -0.207*** | -0.207*** |
| | [0.007] | [0.009] | [0.007] | [0.000] | [0.000] | [0.000] |
| Schooling Lowest Track | 0.049 | 0.053 | 0.051 | 0.104*** | 0.105*** | 0.105*** |
| | [0.206] | [0.172] | [0.188] | [0.000] | [0.000] | [0.000] |
| Schooling Interm. Track | -0.005 | -0.004 | -0.002 | 0.066** | 0.065** | 0.065** |
| | [0.892] | [0.912] | [0.946] | [0.016] | [0.017] | [0.018] |
| Schooling Highest Track | 0.151*** | 0.151*** | 0.152*** | 0.033 | 0.033 | 0.033 |
| | [0.000] | [0.000] | [0.000] | [0.266] | [0.261] | [0.261] |
| Foreigners | 0.185*** | 0.184** | 0.185*** | 0.028 | 0.030 | 0.031 |
| | [0.010] | [0.010] | [0.009] | [0.739] | [0.720] | [0.711] |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| $R^2$ | 0.958 | 0.958 | 0.958 | 0.911 | 0.911 | 0.911 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level. Broad definition of radical parties.

The estimates reveal a statistically significant relationship between the level of economic deprivation in a county and the vote share of radical left-wing parties. The estimated effects are of modest size, though. The coefficient estimates suggest that a one percentage point increase in the poverty rate is associated with an increase in the share of votes for radical left-wing parties of 0.06 percentage points. In relation to the sample mean, this is equivalent to

an increase in the vote share of 0.7 percent. For the poverty gap (median gap), the estimated effect of a one percentage point increase is 0.15 (0.12) percentage points, implying a 1.8 (1.5) percent increase in votes compared to the sample mean. In contrast, for radical right-wing parties, we do not detect any significant association between the share of votes these parties receive and our deprivation measures.

A glance at the coefficient estimates of the control variables reveals some interesting findings. An increase in the unemployment rate as well as population density is associated with an increase in the vote share of radical left-wing parties, but a decrease in the vote share of radical right-wing parties. The latter result suggests that right-wing parties are more popular in rural areas, which is well in line with anecdotal evidence. Older people appear to be less likely to vote for radical left-wing and radical right-wing parties, as suggested by the decrease in the magnitudes of the corresponding coefficient estimates. People with a low level of education show stronger support for radical right-wing parties, whereas highly educated people appear to be more likely to support radical left-wing parties. Interestingly, the share of foreigners is significantly positively related to the vote share of radical left-wing parties, but not significantly related to the share of votes for radical right-wing parties.

The OLS estimates should be interpreted with caution, though, as we cannot rule out that they are affected by confounding factors. Table 4.3 reports the two-stage least squares (2SLS) results of the IV estimation where we instrument the actual realizations of our deprivation measures by measures that are computed based on counterfactual incomes. Again, the left panel shows the results for the share of votes for left-wing parties, the right panel for right-wing parties.

Table 4.3: Support for Radical Parties—2SLS

| | Radical Left-wing Parties | | | Radical Right-wing Parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | -0.261** | | | 0.496*** | | |
| | [0.039] | | | [0.000] | | |
| Poverty Gap | | 0.213 | | | 1.243*** | |
| | | [0.329] | | | [0.000] | |
| Median Gap | | | 0.050 | | | 0.683*** |
| | | | [0.775] | | | [0.003] |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 56.37 | 98.48 | 44.98 | 56.37 | 98.48 | 44.48 |
| Kleibergen-Paap | 42.25 | 54.33 | 5.64 | 42.25 | 54.33 | 5.64 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level. Broad definition of radical parties.

Comparing the 2SLS estimates to the OLS estimates suggests that the OLS estimates are indeed severely biased. With regard to the vote share of radical left-wing parties, the results we obtain based on IV estimation are very different to the OLS results. We detect a significantly negative effect of the poverty rate on the vote share of radical left-wing parties. The effect is not huge, but not negligible either. I.e., a rise in the share of households with an income below the poverty line decreases the vote share of radical left-wing parties by 0.26 percentage points or about three percent of the sample mean, respectively. However, the coefficient estimates of the other two deprivation measures, that is, the poverty gap and the median gap, are not statistically different from zero at reasonable levels of significance. Note that it is unlikely that the insignificance of these deprivation measures is due to inefficient estimation, as the Cragg-Donald F statistics are far above the critical values of the weak instrument test by Stock and Yogo (2005).[11]

In contrast, the 2SLS estimates indicate that economic deprivation has a positive impact on the vote share of radical right-wing parties. The estimated effects are statistically significant even at the one percent level of significance and of relevant magnitude. According to the estimates, a one percentage point increase in the poverty rate leads to a rise in the vote share of radical right-wing parties by 0.5 percentage points. In relation to the sample mean, this implies an increase in the vote share by ten percent. The effects of an increase in the poverty gap and median gap are even larger. Here, a one percentage point increase leads to 1.24 and 0.68 percentage points higher vote shares, implying a 25 percent and 14 percent increase in votes, respectively. The fact that a change in the average shortfall from the poverty line has a larger effect on the share of radical right-wing votes than a change in the average shortfall from the median income suggests that people are more prone to support radical right-wing parties the more deprived they are.

### 4.6.2 Extensions and Robustness Test

To test the robustness of our results, we modify our empirical specification in several ways. In a first robustness test, we apply a narrow definition of radical parties that includes only those parties that are entirely under the Office for the Protection of the Constitution's surveillance (cf. Section 4.3.3). With regard to radical left-wing parties, the only party included in the broad definition, but excluded from the narrow definition, is the Left Party. Of the radical right-wing parties, five out of twelve do not meet the narrow definition, among them the AfD. The 2SLS results are presented in Table A4.1 in the appendix.

For left-wing radical parties, we detect a positive effect of all three economic deprivation measures that is significant at every reasonable level of significance. It thus appears that in the baseline specification, the significant negative coefficient estimate for the poverty rate and the insignificant estimates for the poverty gap and median gap are entirely driven by

[11] The critical values for the Stock-Yogo weak IV F-test are 16.38 (ten percent maximal IV size), 8.96 (15 percent), 6.66 (20 percent), and 5.53 (25 percent).

the Left Party. The coefficient estimates indicate that a one percentage point increase in the poverty rate/poverty gap/median gap increases the share of radical left-wing votes by 0.03/0.12/0.06 percentage points, which implies an increase in the vote share by 50/200/100 percent. However, in light of the small vote share radical left-wing parties other than the Left Party received in federal elections, the effects are still far too small to have a meaningful impact.

In contrast, the results we obtain for radical right-wing parties remain qualitatively unchanged when changing the definition of radical parties. The fact that the coefficient estimates become notably smaller compared to the baseline results is most likely due to the exclusion of five out of twelve parties when moving from the broad to the narrow definition, among them the AfD, the most popular right-wing party in recent years.

Second, we investigate how changes in economic deprivation affect the share of votes of established parties. The results are presented in Table A4.2 in the appendix. We detect a significantly negative effect of the poverty gap on the share of votes for established parties. The coefficient estimate of the median gap is negative as well, but just above the ten percent level of significance. It thus appears that the gain in votes for radical parties in response to an increase in economic deprivation comes to the expense of established parties.[12]

Third, we examine whether the effect of economic deprivation differs across West and East Germany. In Section 4.4, we highlighted that economic deprivation is much more prevalent in East Germany, although the West/East divide appears to have decreased over the past decades. At the same time, radical parties at both ends of the political spectrum enjoy greater popularity in East Germany than in West Germany. It is thus interesting to check whether the effect of economic deprivation on the vote share of radical parties varies across the two regions. To this end, we estimate separate coefficients for our deprivation measures across West and East German counties by including two dummy variables, i.e., one dummy that is equal to one for West German counties and one dummy that is equal to one for East German counties, and interacting these dummies with the deprivation measures. The results of the IV estimation are presented in Table 4.4.

According to our estimates, an increase in the poverty gap has a somewhat stronger effect on the support for radical right-wing parties in West Germany than in East Germany. In West German counties, a one percentage point increase in the poverty gap leads to a 1.6 percentage points increase in the vote share for radical right-wing parties, compared to 1.1 percentage points in East German counties. However, for the poverty rate and the median gap, we only find significant estimates for East Germany.

[12] Further analyses suggest that the reduction in the combined vote share of established parties is primarily due to a reduction in the votes for the Social Democratic Party (SPD) and the Green Party, which both lean to the left. The results are available on request.

Table 4.4: Support for Radical Parties in West and East Germany—2SLS

| | Radical Left-wing Parties | | | Radical Right-wing Parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| East × Poverty Rate | -0.556* | | | 0.708*** | | |
| | [0.055] | | | [0.000] | | |
| West × Poverty Rate | 1.269 | | | -0.605 | | |
| | [0.598] | | | [0.688] | | |
| East × Poverty Gap | | -0.113 | | | 1.084*** | |
| | | [0.660] | | | [0.000] | |
| West × Poverty Gap | | 0.920 | | | 1.590** | |
| | | [0.225] | | | [0.040] | |
| East × Median Gap | | | -2.373 | | | 0.912* |
| | | | [0.123] | | | [0.060] |
| West × Median Gap | | | 3.689 | | | 0.339 |
| | | | [0.278] | | | [0.791] |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 0.36 | 5.15 | 0.93 | 0.36 | 5.15 | 0.93 |
| Kleibergen-Paap | 0.20 | 3.21 | 0.49 | 0.20 | 3.21 | 0.49 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level. Broad definition of radical parties.

Finally, we investigate whether the effect of deprivation on the support for radical parties varies across urban and rural areas. It is often argued that people living in rural areas are more prone to support radical parties, especially nationalistic ones. As before, we estimate separate coefficients by interacting the deprivation measures with two dummy variables, taking the value of one for urban or rural counties, respectively.[13] Our results do not support the conjecture that the effect of economic deprivation on the support for radial parties varies across urban and rural areas (see Table 4.5).

## 4.7 The 2017 Election and the Rise of the AfD

The federal election of 2017 marked a new era for the Federal Republic of Germany. For the first time since its foundation in 1949, a radical right-wing party with a nationalistic and xenophobic platform entered the federal parliament. Yet, the vote shares of the AfD were not distributed evenly across German regions. Figure 4.4 illustrates the regional distribution of AfD vote shares at the 2017 federal election.

[13] The classification of urban counties and rural counties is taken from the Federal Institute for Research on Building, Urban Affairs and Spatial Developments. Basis for the classification is the population density.

Table 4.5: Support for Radical Parties in Urban and Rural Counties—2SLS

| | Radical Left-wing Parties | | | Radical Right-wing Parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Rural × Poverty Rate | -0.282** | | | 0.504*** | | |
| | [0.028] | | | [0.000] | | |
| Urban × Poverty Rate | -0.220* | | | 0.479*** | | |
| | [0.095] | | | [0.000] | | |
| Rural × Poverty Gap | | 0.119 | | | 1.287*** | |
| | | [0.583] | | | [0.000] | |
| Urban × Poverty Gap | | 0.342 | | | 1.184*** | |
| | | [0.138] | | | [0.000] | |
| Rural × Median Gap | | | 0.024 | | | 0.693*** |
| | | | [0.894] | | | [0.003] |
| Urban × Median Gap | | | 0.084 | | | 0.670*** |
| | | | [0.639] | | | [0.005] |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 28.16 | 49.33 | 22.50 | 28.16 | 49.33 | 22.50 |
| Kleibergen-Paap | 21.19 | 27.06 | 2.81 | 21.19 | 27.06 | 2.81 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level. Broad definition of radical parties.

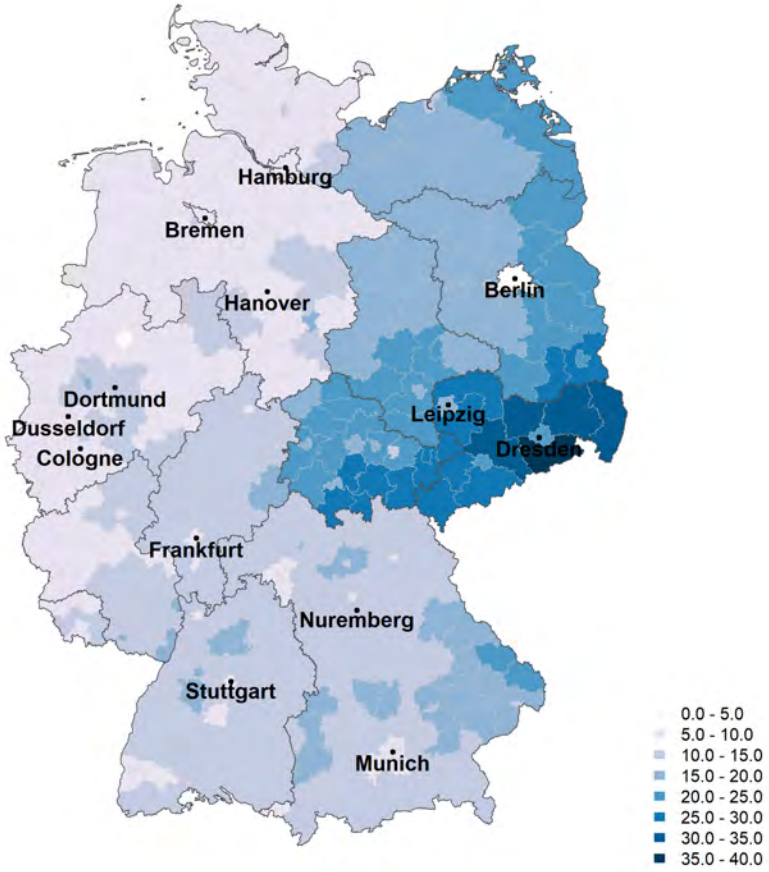
The differences across German counties are quite remarkable: vote shares range from 4.9 percent in Münster (Northrhine-Westphalia) to 35.5 percent in Sächsische Schweiz-Osterzgebirge (Saxony). Most striking are the differences in vote shares between East and West German counties. Whereas the population weighted county average in West Germany is 10.7 percent, it is 22.5 percent, i.e., about twice as high, in East Germany. Additionally, one can also discern regional discrepancies within East and West. In East Germany, vote shares are particularly high along the Polish and Czech border. In West Germany, vote shares are somewhat higher in the South than in the North; but, again, largest in economically weaker regions.

We examine whether and to what extent economic deprivation can explain the observed regional differences in AfD vote shares. For this purpose, we re-estimate our baseline empirical model, but employ the AfD vote share as the dependent variable and only utilize data from the federal election of 2017:

$$Y_{c,2017} = \alpha_c + \beta Deprivation_{c,2017} + \gamma^{'} X_{c,2017} + \epsilon_{c,2017} \tag{4.4}$$

Table 4.6 shows the 2SLS estimates. The results indicate that regional variation in economic deprivation influences the electoral success of the AfD in a statistically significant and sizeable way. According to our estimates, a one percentage point increase in the poverty rate leads, on average, to an increase in the AfD vote share by about 2.0 percentage points, which is

Figure 4.4: AfD Vote Shares across German Counties in 2017




*Notes:* This figure shows AfD vote shares at the 2017 federal election across German counties. Vote shares are measured in percent.

equivalent to a 15 percent increase in votes in relation to the sample mean. An increase in the poverty gap has an even larger effect. If the poverty gap increases by one percentage point, the AfD vote share increases by almost 5.0 percentage points, which implies a 37 percent increase in votes. Thus, the effect of economic deprivation on the vote share of the AfD in the 2017 election is three to four times higher than the general effect of economic deprivation on voting for radical right-wing parties in all federal elections between 1998 and 2017 (see Section 4.6).

As before, we also estimate separate effects for West vs. East Germany and for urban vs. rural areas. The results suggest that the average effect conceals important regional differences. I.e., we find that the effect of economic deprivation on the AfD vote share is about three times larger in East German counties than in West German counties (cf. Table A4.3 in the appendix). In contrast, the effect of economic deprivation on vote shares of all radical right-wing parties is more similar between East and West German counties (see Section 4.6). However, we again

Table 4.6: AfD Vote Shares in German Counties—2SLS

| | AfD Vote Shares | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Poverty Rate | 1.974*** | | |
| | [0.000] | | |
| Poverty Gap | | 4.868*** | |
| | | [0.003] | |
| Median Gap | | | 1.943*** |
| | | | [0.000] |
| Economic Controls | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Mean Dep. Variable | 13.41 | 13.41 | 13.41 |
| N | 396 | 396 | 396 |
| Cragg-Donald | 25.63 | 13.98 | 70.31 |
| Kleibergen-Paap | 21.43 | 12.18 | 54.33 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level.

do not detect any heterogeneous effects between rural and urban counties (cf. Table A4.4 in the appendix).

How can these findings be reconciled with survey evidence suggesting that AfD supporters do not differ from supporters of established parties in terms of income and other socio-demographic characteristics (Bergmann et al., 2017; Hansen and Olsen, 2019)? One possible explanation is that the extent of economic deprivation in a region does not only strengthen the AfD's popularity among the economically deprived, but also among voters from other income groups. There are at least two potential reasons for such a relationship. First, a high level of economic deprivation in close regional proximity may increase economic anxiety among middle and high-income earners as well as the perceived risk of social decline. Economic anxiety, in turn, is found to be an important determinant of the popularity of populist parties (Algan et al., 2017; Guiso et al., 2017). Second, middle and high-income earners may not only care about their own economic situation, but also about the economic conditions in the region in which they are living. A high level of economic deprivation may thus increase dissatisfaction with the political mainstream and make middle and high-income earner more prone to support the populist platform on which the AfD runs.

## 4.8 Conclusion

Arguably, two of the major challenges many industrialized countries have been facing over the past few years are the increase in relative economic deprivation and growing political polarization. Many observers argue that these two phenomena are closely linked, blaming the relative economic deprivation many people experience to be a main factor driving the

increasing popularity of radical parties and movements around the world. This chapter explores whether economic deprivation influences the support for radical parties in a causal way. Using data from Germany, we employ instrumental variable estimation to study the effect of economic deprivation on the share of votes radical left-wing and right-wing parties received in federal elections. Our analysis is conducted at the county-level (NUTS-3) and covers six federal elections held between 1998 and 2017.

The empirical results suggest that regional economic deprivation has a causal and sizeable effect on vote shares of radical parties. This effect is particularly pronounced for radical right-wing parties. I.e., the greater the prevalence of (relative) poverty, the greater the success of nationalistic parties at the polls. Moreover, our results suggest that relative economic deprivation was an important determinant of the electoral success of the AfD (Alternative for Germany), the new nationalist party in Germany, in the federal election of 2017. All in all, our findings provide evidence that the prevalence of relative economic deprivation is an important driver of political polarization, the rise of radical parties and populist movements, and may thus undermine moderate political forces and ultimately threaten political stability.

# Appendix

## Additional Tables

Table A4.1: Support for Radical Parties (Narrow Definition)—2SLS

| | Radical Left-wing Parties | | | Radical Right-wing Parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.032*** | | | 0.039 | | |
| | [0.000] | | | [0.321] | | |
| Poverty Gap | | 0.116*** | | | 0.178** | |
| | | [0.000] | | | [0.019] | |
| Median Gap | | | 0.064*** | | | 0.189*** |
| | | | [0.000] | | | [0.001] |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 0.06 | 0.06 | 0.06 | 2.02 | 2.02 | 2.02 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 56.37 | 98.48 | 44.98 | 56.37 | 98.48 | 44.98 |
| Kleibergen-Paap | 42.25 | 54.33 | 5.64 | 42.25 | 54.33 | 5.64 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level. Narrow definition of radical parties.

## Table A4.2: Established Parties—2SLS

| | Established Parties | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Poverty Rate | 0.076 | | |
| | [0.567] | | |
| Poverty Gap | | -0.810*** | |
| | | [0.001] | |
| Median Gap | | | -0.234 |
| | | | [0.110] |
| Economic Controls | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 83.45 | 83.45 | 83.45 |
| N | 2510 | 2510 | 2510 |
| Cragg-Donald | 56.37 | 98.48 | 44.98 |
| Kleibergen-Paap | 42.25 | 54.33 | 5.64 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level.

Table A4.3: AfD Vote Shares in East and West German Counties—2SLS

| | AfD Vote Shares | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| East × Poverty Rate | 1.030*** | | |
| | [0.000] | | |
| West × Poverty Rate | 0.390* | | |
| | [0.051] | | |
| East × Poverty Gap | | 3.811*** | |
| | | [0.000] | |
| West × Poverty Gap | | 1.238 | |
| | | [0.182] | |
| East × Median Gap | | | 0.946*** |
| | | | [0.000] |
| West × Median Gap | | | 0.291 |
| | | | [0.139] |
| Economic Controls | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Mean Dep. Variable | 13.41 | 13.41 | 13.41 |
| N | 396 | 396 | 396 |
| Cragg-Donald | 17.75 | 7.36 | 37.35 |
| Kleibergen-Paap | 14.03 | 5.47 | 26.91 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level.

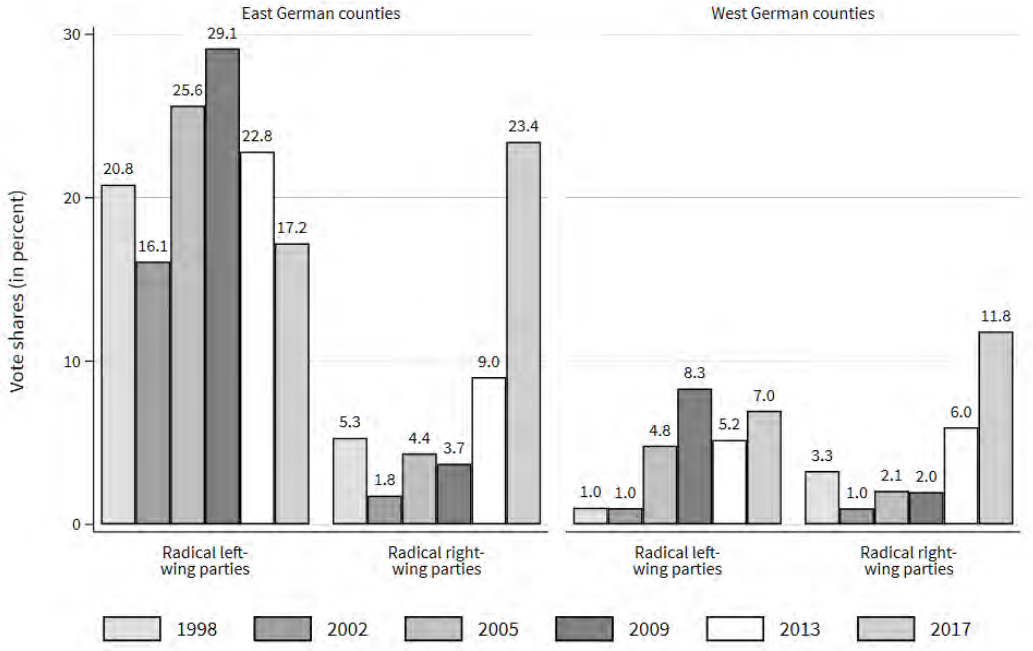
## Table A4.4: AfD Vote Shares in Urban and Rural German Counties—2SLS

| | AfD Vote Shares | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Rural × Poverty Rate | 2.025*** | | |
| | [0.000] | | |
| Urban × Poverty Rate | 2.104*** | | |
| | [0.000] | | |
| Rural × Poverty Gap | | 4.875*** | |
| | | [0.003] | |
| Urban × Poverty Gap | | 4.922*** | |
| | | [0.005] | |
| Rural × Median Gap | | | 1.987*** |
| | | | [0.000] |
| Urban × Median Gap | | | 2.050*** |
| | | | [0.000] |
| Economic Controls | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Mean Dep. Variable | 13.41 | 13.41 | 13.41 |
| N | 396 | 396 | 396 |
| Cragg-Donald | 11.44 | 6.54 | 32.06 |
| Kleibergen-Paap | 10.63 | 6.05 | 26.76 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the county-level.

## Additional Figures

### Figure A4.1: Radical Vote Shares in East and West German Counties




*Notes:* This figure shows the development of the average vote share of radical left-wing and right-wing parties in East and West German counties at federal election from 1998 to 2017. Vote shares are measured in percent.

# 5 Economic Deprivation and Local Fiscal Policy: Evidence from German Cities*


## Abstract

Economic deprivation can be an important determinant of fiscal policy, but the nature of the relationship between redistributive fiscal policies and economic deprivation is theoretically and empirically ambiguous. This chapter aims to add to our understanding of the relationship by estimating the effect of economic deprivation on local policy outcomes. We exploit the specific institutional setting in Germany, which grants local authorities a high degree of fiscal autonomy, to identify the causal effect of economic deprivation on fiscal policy using an instrumental variable approach. We find that in response to an increase in regional economic deprivation local policy makers increase the local business tax and cut spending on public services. Thus, our results are ambiguous regarding the redistributive consequences of increasing economic deprivation. We discuss possible explanations for this ambivalence as well as potential transmission channels.


* This chapter is joint work with Florian Neumeier and Samina Sultan. It is based on our paper 'How Does Economic Deprivation Affect Local Fiscal Policy in Germany?'

## 5.1 Introduction

How governments set their tax rates and the quality of the public services they provide is an important determinant of many outcomes. In terms of economic outcomes, for instance, fiscal policy impacts the level of public debt, investment, and even growth (Aghion et al., 2014; Kneller et al., 1999). But fiscal policy also influences social outcomes, such as social cohesion or mobility (Schneider, 2010). Moreover, fiscal policy can spark political protests, such as the yellow jackets in France who initially protested against an increase in fuel taxes. Thus, it can affect political outcomes as well.

This raises the question what, in turn, determines fiscal policy. One hypothesis is that as societies become more heterogeneous in terms of income, it also becomes more difficult to agree on the provision of public services and redistributive policies, suggesting a negative relationship between income dispersion and redistributive fiscal policies. At the same time, there are hypotheses indicating a positive association between income dispersion and government size, due to a higher demand for redistribution.

In this chapter, we aim to determine the sign of the relationship empirically and thus contribute to the literature. Our goal is to identify the causal influence of economic inequality on local fiscal policy outcomes. In order to estimate the effect of income dispersion on fiscal policy outcomes, we exploit the specific institutional setting in Germany which grants local authorities a high degree of fiscal autonomy. Making use of this rich level in local variation, we combine administrative fiscal data on the universe of German city districts (*Kreisfreie Städte*) with measures of income dispersion from the German Microcensus.

While existing contributions concentrate on general measures of income inequality, like the Gini coefficient, we focus on the effect of economic deprivation on fiscal policy. The Gini coefficient is often criticized for being an imperfect measure of income inequality, as very different income distributions can lead to the same realization of the Gini coefficient. For instance, both an increase in income for the rich and a decrease in income for the poor can lead to a high Gini coefficient. However, it is especially the latter scenario which is of interest here, as increasing economic deprivation, i.e., an increase in inequality driven by the lower bound of the income distribution, aggravates the importance of redistributive fiscal policies. Specifically, we study the effect of three different measures of economic deprivation on fiscal policy, namely the average shortfall of district' citizens relative to the national median (median gap), the poverty line (poverty gap), as well as the poverty rate.

In order to draw conclusions, about the causal influence of economic deprivation on local fiscal policy, we employ instrumental variables (IV) estimation. Following Boustan et al. (2013), we construct instruments for region-specific measures of economic deprivation, which predict changes in regional economic deprivation through national income trends. Thus, our instruments are exogenous to asymmetric regional economic developments, to endogenous

political reactions to growing support for radical parties, as well as to endogenous sorting of individuals into regions.

Our results are ambiguous regarding the distributional consequences of economic deprivation. We find that increasing economic deprivation causes local policy makers to increase the local business tax rate, while we do not find significant effects on the local property tax. Given that the local business tax is likely to be perceived as a progressive tax, whereas the perception of the property tax is more ambivalent, this seems like an attempt to make the tax system more redistributive as economic deprivation increases.

However, aggregate spending on local public services is negatively affected by economic deprivation. In particular, this effect is driven by a spending cut on welfare, schooling, and sport facilities. As these public services are likely to mainly benefit lower income groups, our results suggest a negative relationship between economic deprivation and redistributive fiscal policies on the expenditure side. We discuss possible explanations for these ambiguous results as well as potential transmission channels.

Our study is related to the literature examining the relationship between income dispersion and the size of the state.[1] This relationship has been addressed both theoretically and empirically. The seminal paper by Meltzer and Richard (1981) builds on the median voter theorem. Assuming majority rule, a decrease in the median income relative to the mean income translates into a stronger vote for redistribution. It can therefore be assumed that increasing economic deprivation makes redistributive fiscal policies more likely.

Epple and Romano (1996) construct a model in which both the public and the private sector provide goods and services. In contrast to the standard assumptions of the median voter theorem, this results in non-single peaked preferences, as the decision of whether to choose a public service or its private alternative depends on the level of quality of the services provided. In this setting, the median voter theorem does not generally characterize the voting equilibrium and the level of public provision is below the level preferred by the median income voter as a coalition of poor and rich households favor private over public provision. Thus, within this framework an increase in the number of poor and rich people leads to a decrease in the public provision of goods and lower taxation.

Economic theory can therefore motivate both a positive and a negative relationship between economic deprivation on the one hand and the provision of public services and redistributive policies on the other hand. However, most theoretical models are based on majority rule. Hence, their findings only have limited application in a setting with proportional representation such as Germany.

[1] There are also papers studying other factors that affect local fiscal policy, such as the partisanship of local councils (Riedel et al., 2016).

The empirical literature on income dispersion and fiscal policy can be broadly divided into two branches: the first group uses cross-country data, the second group focuses on sub-national jurisdictions in countries with a federal system. The first group has the advantage that national governments are typically equipped with more far-reaching fiscal competencies than sub-national governments. The advantage of the latter group is that sub-national jurisdictions are typically more comparable with regard to their institutional and political framework, mitigating concerns about endogeneity biased estimates. However, to the best of our knowledge, studies focusing on the sub-national level only exist for the U.S. Moreover, both literature branches typically rely on the Gini coefficient as a measure of income inequality.

For instance, Milanovic (2000) uses data from the Luxembourg Income Study (LIS) to investigate the relationship between income inequality and redistribution for 24 countries with a fixed effects model. He finds a significant positive relation but no evidence that the median voter theorem can explain the redistribution as the income gain of the middle class proves to be independent from its initial income. Attempting to test the relationship between redistribution and components of the Mirrless model, Hannu et al. (2018) also employ LIS data for a sample of 14 countries. Using the optimal tax formula to construct redistributive preferences, they find a positive correlation between factor-income inequality and the extent of redistribution, which in turn has a positive effect on governments' propensity for redistribution.

Employing the same empirical framework, Karabarbounis (2011) uses three different indices of income inequality for 14 OECD countries. He argues that not just the median voter but the demands of various income groups of voters determine redistribution. Establishing a positive relationship between the income of the poor and redistribution as well as a negative relationship between the income of the middle and upper class and redistribution, he coins the term one dollar, one vote politico-economic equilibrium: an increase in the income of a group of citizens relative to the average income results in redistribution tilting towards the bliss point of that group.

Focusing on the sub-national level, our study is most closely related to Boustan et al. (2013). Using an IV approach, they show that a broadening of the income distribution is associated with an increase in tax collection and expenditures in U.S. municipalities and school districts. Moreover, they analyze the impact of a rise in inequality on the composition of local expenditures and find that particularly police, fire protection and infrastructure receive additional funding. Similarly, Corcoran and Evans (2010) find a positive relation between income inequality and educational spending on the school district-level in the U.S. Using different inequality measures, Schwabish (2008) shows that both an increase in the upper as well as the lower end of the income distribution leads to an increase in social spending on non-health and non-educational goods and services in U.S. states.

While the above papers focus on the relationship between income inequality and the size of the public sector, other works concentrate on how income inequality affects tax progressivity and tax structure. For the 434 local governments in Norway, Borge and Rattsø (2004) find

that a more unequal income distribution causes the tax burden to shift from the poll to the property tax. This points to an increase in redistribution as the property tax in Norway is proportional to income. Looking at various determinants of sub-national tax progressivity in the U.S., Chernick (2005) finds that greater inequality in pre-tax income distributions is compensated by more progressive tax systems, but the effect is relatively small.[2]

As the overview above shows, most existing empirical literature on the sub-national level focuses on the U.S. Due to the differences in the political systems, the possibilities for comparison with the German setting are limited. Most importantly, the U.S. uses majority rule, whereas Germany follows the principle of proportional voting, on the federal but also on the local level. This difference is likely to impact results. Freier and Odendahl (2012), for example, show that majority governments spend more on public services and set higher tax rates, which is in line with the results for the U.S. presented above. We therefore test whether fiscal policy differs between divided or unified governments in our data.

The remainder of this chapter is structured as follows. Section 5.2 describes the institutional background of the local government administration in Germany. Section 5.3 presents our data as well as the measures of economic deprivation that we construct. We provide descriptive statistics of our data in Section 5.4. The empirical strategy is explained in Section 5.5. In Section 5.6 we present our results on the relationship between economic deprivation and local fiscal policy. We discuss these results in Section 5.6. Robustness test are presented in Section 5.8. Section 5.9 concludes.

## 5.2 Institutional Background

Due to the federal structure of Germany, power is divided both horizontally as well as vertically between the 16 federal states (*Bundesländer*) and the local authorities, i.e., municipalities (*Gemeinden*), counties (*Kreise*), and city districts (*Kreisfreie Städte*). City districts are large municipalities that constitute their own county, such as Munich or Frankfurt. Municipalities, counties, and city districts constitute the lowest level of the state and administrative structure in Germany. Nevertheless, the German constitution grants them local autonomy within the limits of the law (German Constitution Art. 28).

This right of self-governance includes financial and tax sovereignty. Financial sovereignty entitles local authorities to manage their income and expenditure. Tax sovereignty grants them the right to raise taxes as long as this does not violate higher law. The main tax rates that are determined at the local level are two types of property tax rates as well as the local business tax rate.

[2] As measure for the degree of tax progressivity, Chernick (2005) uses the income tax share compared to the sales and excise tax shares in state and local tax systems.

To be precise, the local authorities can decide on the local scaling factor of the property tax rate and the local business tax rate. Particularly the local business tax is an important income source for local authorities. In 2016, for example, the local business tax contributed 43 percent to the overall tax revenue of the German local authorities. The two property taxes jointly accounted for 14 percent of the overall tax revenue (Statistisches Bundesamt, 2017).[3]

The local business tax rate is determined by multiplying the respective local scaling factor with the basic rate (*Gewerbesteuermesszahl*). The basic rate is defined at the federal level and from 1993 to 2007 was set at 5.0 percent with a decrease to 3.5 percent in 2008. In contrast, the local scaling factor for a given year is voted on by the municipal/city council one year in advance. Thus, changes in the local business tax rate are primarily driven by changes in the local scaling factor, which is determined by the local authorities.[4]

There are two types of property tax rates in Germany: property tax rate A, which applies to agricultural areas, and property tax rate B, which applies to residential property. The property tax rates are again determined by multiplying their respective local scaling factor with their respective basic rate (*Grundsteuermesszahl*), which is determined by federal law (Property tax law §14 and §15) and depend both on the value and the type of property. In our main analysis, we focus on the property tax B as, together with the local business tax, it constitutes the most important local tax instrument in terms of revenue collection.

In contrast to countries like Norway, where the property tax is proportional to income and thereby constitutes a progressive tax, the incidence of the residential property tax in Germany is not straightforward. It is proportional to the value of the property. However, it is, strictly speaking, not a tax on the ownership of property, which would make it more plausible to classify it as progressive tax since higher income groups in Germany are more likely to own property (Dustmann et al., 2018). Instead, it is a tax on the 'right to reside', as it is part of the apportionable service charges. Thus, landlords can pass it on to their tenants. Indeed, Löffler and Siegloch (2018) show that in the long run both the statutory and the economic incidence of the property tax are borne by the tenant. Hence, it seems more likely that the property tax B is a regressive tax.

On the expenditure side, local self-governance includes only those tasks that fall within the local authorities' own sphere of influence, whereas it does not apply to tasks that the federal government or the respective federal state has transferred to the local authority by law. As we are interested in how local authorities react to changes in local inequality, only tasks that fall under their own sphere of influence are of interest here. In contrast, local authorities have no discretion in spending on tasks that do not belong to their area of influence. Instead, they have to spend a specific earmarked amount on these tasks, for which they typically receive grants from the respective federal state.

[3] The remaining sources of tax revenue are the local authorities' share of the income tax (37 percent) and the sales tax (5 percent) as well as other taxes (2 percent).

[4] Note that the tax base and liability criteria of the local business tax are determined at the federal level.

Therefore, we restrict our analysis to those tasks that fall under the local authorities' sphere of influence. These tasks differ in their degree of local autonomy. In the case of voluntary self-government tasks (*freiwillige Selbstverwaltungsaufgaben*), the local authority is free to choose whether and how to carry out the activity. In contrast, local authorities are obliged to fulfill mandatory self-government tasks (*pflichtige Selbstverwaltungsaufgaben*), for instance, determined by federal law, but are free to choose the manner of provision. As the definition of the different spheres of influence as well as the different types of self-government tasks is not precisely and consistently specified across the different constitutions and municipal codes of the federal states, we follow the categorization in Postlep (1987). We also cross-check this definition with the constitutions and municipal codes of the federal states where possible and find a great overlap.

Accordingly, examples for voluntary self-government tasks are cultural activities such as the construction of theaters or sports facilities. Mandatory self-government tasks include schools and waste removal. For the purpose of our analysis, both voluntary and mandatory self-government tasks are of interest as local authorities have at least some degree of autonomy in their provision.[5] To account for the varying definitions of tasks across local authorities, we include regional fixed effects in our regressions (see Section 5.5).

## 5.3 Data Description

To analyze the relationship between economic deprivation and local fiscal policies, we construct a unique panel data set combining local indicators of economic deprivation with data on local government expenditure and taxation. Our panel covers the years 1991 to 2016 for West Germany and 1998 to 2016 for East Germany. Most importantly, to be able to combine the spending and taxation data on the municipality-level with the indicators of economic deprivation on the county-level, we restrict our sample to city districts. These are large municipalities that form their own county. Our final sample includes roughly 104 city districts per year,[6] of which more than 80 percent are in West Germany. Our sample of city districts accounts for roughly 30 percent of the overall German population.

To create our data set, we mainly rely on three data sources. Regional measures of economic deprivation are constructed based on microdata from the German Microcensus (*Mikrozensus*). Data on local taxation are taken from the real property and business tax statistic (*Realsteuervergleich*). Finally, local government expenditure is calculated based on the annual account data of municipalities (*Jahresrechnungsstatistik der Gemeinden und Gemeindeverbände*).

[5] See Section 5.3.4 for more details.

[6] The exact number varies between 87 in 1993 (West Germany only) and 110 in 2005.

### 5.3.1 The German Microcensus

The Microcensus is a household survey that is carried out annually since 1957 by the statistical offices of the German states (*Statistische Landesämter*) and is administered by the Federal Statistical Office (*Statistisches Bundesamt*). It comprises a representative one percent sample of the German population, resulting in a sample size of more than 800,000 persons from almost 400,000 households per year. The Microcensus contains information about various demographic characteristics, including the county of residence, employment status, household size, the age of all household members, and household income, among others.

For our analysis, we use the waves from 1991 to 2016. As the Microcensus is not available for the years 1995 and 1996, there is a gap in our sample for these two years. Besides the large number of variables, one major advantage of the Microcensus is its large sample size, which allows us to construct economic deprivation indicators at the regional level. Moreover, the Microcensus is administered by a federal agency and there is a legal obligation to answer the questions. Therefore, item-non-response is not an issue. Also, answers must be truthful and complete.

We use information on monthly net household income to construct our measures of economic deprivation. To account for differences in household size, we compute equivalized household incomes using the new OECD equivalence scale. In addition, we adjust the income figures for changes in prices using the consumer price index for Germany. Note that the income variable in the Microcensus is interval-censored, i.e., respondents are asked to indicate which income class they belong to. However, the width of the income classes is rather narrow and the number of income classes is large, varying between 18 and 24, depending on the survey year.

In order to obtain continuous household income figures, we apply an imputation approach. That is, we estimate a continuous income figure for each household based on information on a household's income class as well as various socio-demographic characteristics using interval regressions. This imputation technique ensures that the empirical distribution of the continuous income variable matches the shape of the distribution of the income classes. As a result, we obtain a single income figure for each household that is consistent with the observed income limits (see for example Royston, 2008), which we then use to calculate a number of economic deprivation measures at the county-level.

### 5.3.2 Indicators of Economic Deprivation

We employ three different indicators of economic deprivation that account for the relative economic well-being of citizens living in a given county compared to the national average. Our first indicator measures the poverty rate within a county; that is, the share of households within a county living below the national poverty line, $z^{nat}_{pov,t}$, in year $t$. We set the poverty line at 60 percent of the national median income, $z^{nat}_{50,t}$, so that $z^{nat}_{pov,t} = 0.6 \times z^{nat}_{50,t}$.

Our second indicator of economic deprivation is constructed in a similar fashion, but it measures the average shortfall from the national poverty line instead of the share of households living below the poverty line. This indicator is widely known as the poverty gap and is defined by the following formula:

$$Poverty\,Gap_{ct} = 100\frac{1}{n_{ct}}\sum_{j=1}^{q}\frac{z_{pov,t}^{nat} - y_{cjt}}{z_{pov,t}^{nat}} \tag{5.1}$$

where $n_{ct}$ is the number of households from counties $c$ at year $t$ that are included in the Microcensus data, and $y_{cjt}$ is the income of household $j$.

Our final measure of relative economic deprivation is the average shortfall in the incomes of a county's residents from the national median income. We refer to this measure as the median gap. It is constructed as follows:

$$Median\,Gap_{ct} = 100\frac{1}{n_{ct}}\sum_{j=1}^{r}\frac{z_{50,t}^{nat} - y_{cjt}}{z_{50,t}^{nat}} \tag{5.2}$$

### 5.3.3 The Property and Trade Tax Statistic

The property and trade tax statistic collects all information regarding local tax revenues. It is published on a yearly basis by the Federal Statistical Office. Besides recording the local scaling factors of the property taxes and the local business tax, it also includes the respective tax revenues as well as information on the municipality's share of the income and sales tax revenue. As the city states, Bremen, Berlin and Hamburg, have a greater degree of autonomy over their budget, we exclude them from our sample for consistency.

### 5.3.4 Annual Account Data of Municipalities

The municipalities' annual account data for the years 1992 to 2006 is taken from the statistical offices of the German states. The data is available for all federal states except for the city states Berlin, Bremen and Hamburg. Moreover, for the years 1992 to 1997 only data for West Germany is available.

The data contains information on income and expenditure of German municipalities and specifies to which task the cost or income can be assigned. As specified in Section 5.2, we focus on voluntary and mandatory self-government tasks for our analysis, as municipalities have at least some degree of autonomy in their provision. We therefore select those expenditure

items in the municipalities' account data that fit this definition and create sensible clusters to obtain our final list of expenditure variables.[7]

Following this procedure, our final list of expenditure variables includes schools, welfare, health, sports, culture, public education, waste, fire protection, public order, and infrastructure. These variables only cover costs that fall under the definition of voluntary and mandatory self-government tasks of the municipalities. For schooling expenditure this, for instance, means that the variable includes costs for school maintenance and wages for non-teaching staff, but it does not cover wages for teaching staff as these costs incur at the level of the federal states. Moreover, we create an aggregate over all spending on voluntary and mandatory self-government tasks for a given year and municipality, which we label aggregate spending in the following.

While the annual account data offers a good approximation of municipalities' income and expenditure, it also needs to be noted that this data does not cover all forms of financing. For instance, so-called public private partnerships are not included in the data. Thus, if a school or local road is renovated with the funds of a public private partnership, this is not included in the annual account data of the municipality. We can therefore only study the effect of economic deprivation on the officially recorded spending on public services.

### 5.3.5 Control Variables

In our empirical analysis, we include several control variables depicting the demographic, economic, and political situation in a city district. We control for the log of population, the share of foreigners, the dependency ratio, as well as mean income. Furthermore, we control for political polarization, which we define as the districts' vote shares of radical right- and left-wing parties at the latest federal election.[8] To account for the fact that municipalities also have other income sources besides the revenue generated by the local business tax and the property tax, we control for the log of per capita income tax revenue, which the city districts receive from the federal states.[9]

District-level population size is provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Developments (*Bundesinstitut für Bau-, Stadt-, und Raumforschung*, BBSR). The share of foreigners is taken from the German Regional Database (*Regionaldatenbank Deutschland*) as well as the statistical offices of the German states. Federal election

[7] Table A5.2 in the appendix shows our selection of expenditure items and how we combined them to create our final expenditure variables.

[8] The definition for extreme right and extreme left parties in Germany follows Dorn et al. (2020). Note that political polarization might be an insufficient control variable as a rise in economic deprivation could result in political polarization. We therefore rerun our main specification without that control variable. The results do not change.

[9] Note that the share of the income tax revenue that city districts receive is fixed and city districts cannot manipulate it.

outcomes are provided by the federal returning officer (*Bundeswahlleiter*). The dependency ratio and mean income are calculated based on individual responses from the German Microcensus.

## 5.4 Descriptive Statistics

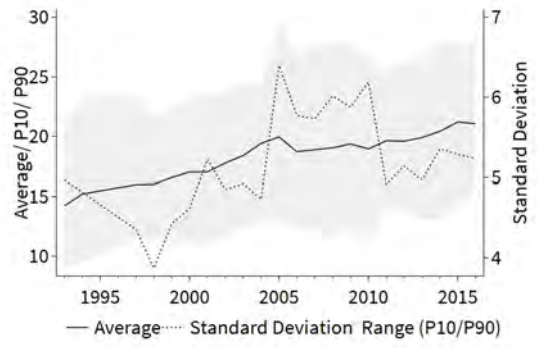
In the following three subsections, we provide graphical descriptions of regional economic deprivation, local taxation, and spending on public services. Table A5.1 in the appendix provides summary statistics for all our dependent and control variables.

### 5.4.1 Regional Economic Deprivation

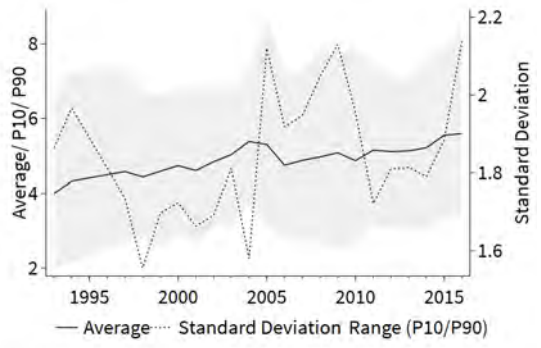
The average development of the three measures of economic deprivation between 1993 and 2016 for the city districts in Germany is shown in Figure 5.1. All three indicators, the poverty rate, the poverty gap, and the median gap evolve rather similarly.

Figure 5.1: Measures of Economic Deprivation

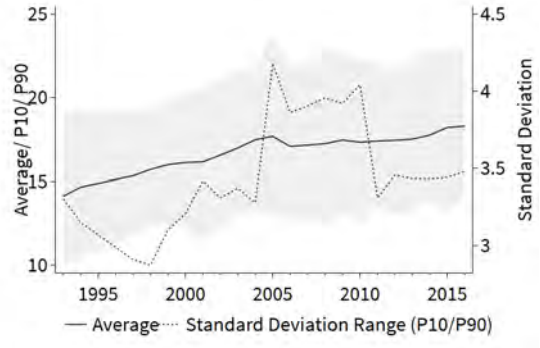
(a) Poverty Rate




(b) Poverty Gap




(c) Median Gap




*Notes:* This figure shows the development of the average poverty rate, the poverty gap, and the median gap of city districts over the sample period 1993 to 2016. Additionally, the standard deviation and the range (P10/P90) for each measure are displayed. Measures of economic deprivation are measured in percent.

There is a steady increase since 1993 with a peak in 2004/2005. The peak could be due to the Hartz reforms, which were implemented at that time and changed the unemployment and social benefits system in Germany. The subsequent decline in unemployment reduces the level of economic deprivation to the 2001 value. The largest drop is in the poverty gap, while the median gap displays the smallest decline. From 2006 the economic deprivation increases again. For the poverty rate and the poverty gap, the increase continues at approximately the same slope as before the peak, while the slope is slightly reduced for the median gap. During the sample period, economic deprivation has therefore become a more urgent problem in German city districts.

### 5.4.2 Local Taxation

Figure 5.2 illustrates the development of the average local scaling factor (hereafter 'local tax rate') for the city districts from 1993 to 2016. The average local business tax rate starts at a level of just over 400 percent in 1993 and increases slowly until 1997, when there is a slight decline. Afterwards the average local business tax remains rather stable until 2009. It then increases to reach approximately 430 percent in 2016 (see Figure 5.2a).

At around 360 percent the level of the average property tax B rate in 1993 is lower than that of the local business tax rate (see Figure 5.2b). However, the average property tax B rate increases continuously throughout the sample period with an increase in the slope in 2009. By 2016 the level of the average property tax B rate is just under 500 percent. Finally, the average property tax A rate increases from just over 250 percent in 1993 to just over 300 percent in 2016 (see Figure 5.2c).

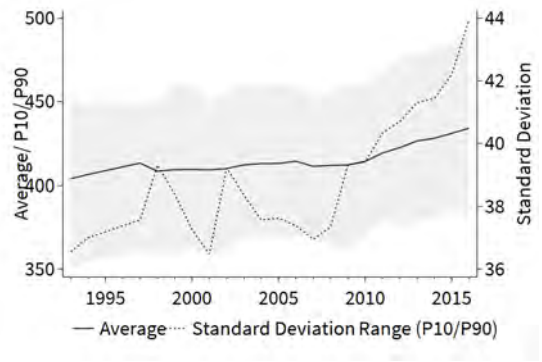
### 5.4.3 Local Government Spending

We use the city districts' annual account data to depict how average per capita spending for voluntary and mandatory self-government tasks in German city districts has evolved over the sample period 1993 to 2006. We show the aggregate trend but also break it down into the individual spending categories. The aggregate average per capita spending is fairly stable over time at around 1,000 Euros as Figure 5.3 shows.

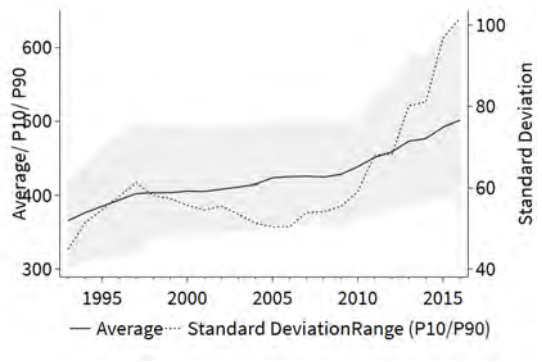
To see whether the composition of spending has changed over time, Figure 5.4 compares the average spending shares of different spending categories in 1993 and 2006. The biggest changes can be observed in the categories waste disposal, welfare, and schooling. While average spending on waste disposal accounts for 25 percent of aggregate spending in 1993, it drops to eleven percent in 2006. On the other hand, from 1993 to 2006, the share of spending on schooling and welfare increases, on average, by four percent and ten percent, respectively. The share of the remaining categories remains fairly stable over the sample period.

Figure 5.2: Local Tax Rates

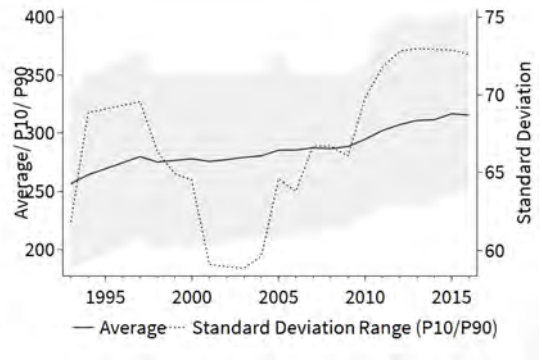
(a) Local Business Tax Rate




(b) Property Tax B Rate




(c) Property Tax A Rate




*Notes:* This figure shows the development of the average local scaling factor for the business tax rate, the property tax B rate, and the property tax A rate of city districts over the sample period 1993 to 2016 in percent. Additionally, the standard deviation and the range (P10/P90) for each measure are displayed.

## 5.5 Empirical Strategy

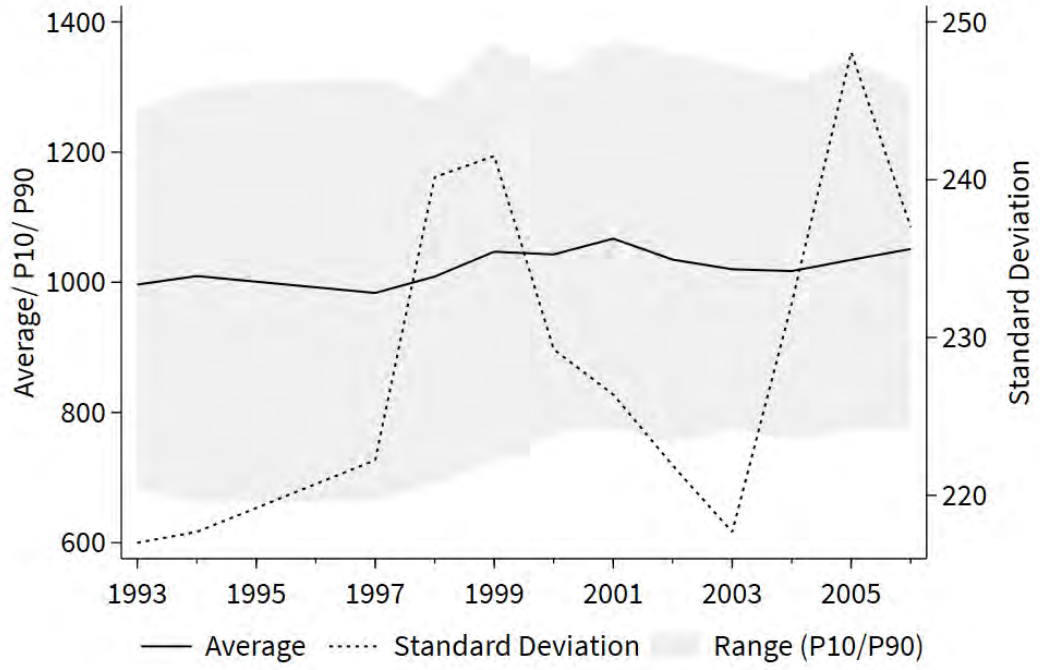
To evaluate the influence of economic deprivation on local taxation and public spending, we estimate the following empirical panel data model first by ordinary least squares (OLS) and then by instrumental variables estimation:

$$Y_{ct} = \alpha_c + \beta Deprivation_{ct} + \gamma' X_{ct} + \delta_t + \epsilon_{ct} \tag{5.3}$$

where the dependent variable, $Y_{ct}$, is either the local scaling factor of the property or business tax in city district $c$ and year $t$ or the city district's expenditures on its mandatory and voluntary self-government tasks.[10] $\alpha_c$ is a county fixed effect that is included to account for time-invariant regional-specific factors that are related to economic conditions and might

[10] To be precise, we use the logged value of per capita spending on mandatory and voluntary self-government tasks.

Figure 5.3: Aggregate per capita Spending on Public Services




*Notes:* This figure shows the development of average aggregate per capita spending on public services in German city districts from 1993 to 2006 in Euros. Public services include only those defined as voluntary and mandatory self-government tasks. Additionally, the standard deviation and the range (P10/P90) are displayed.

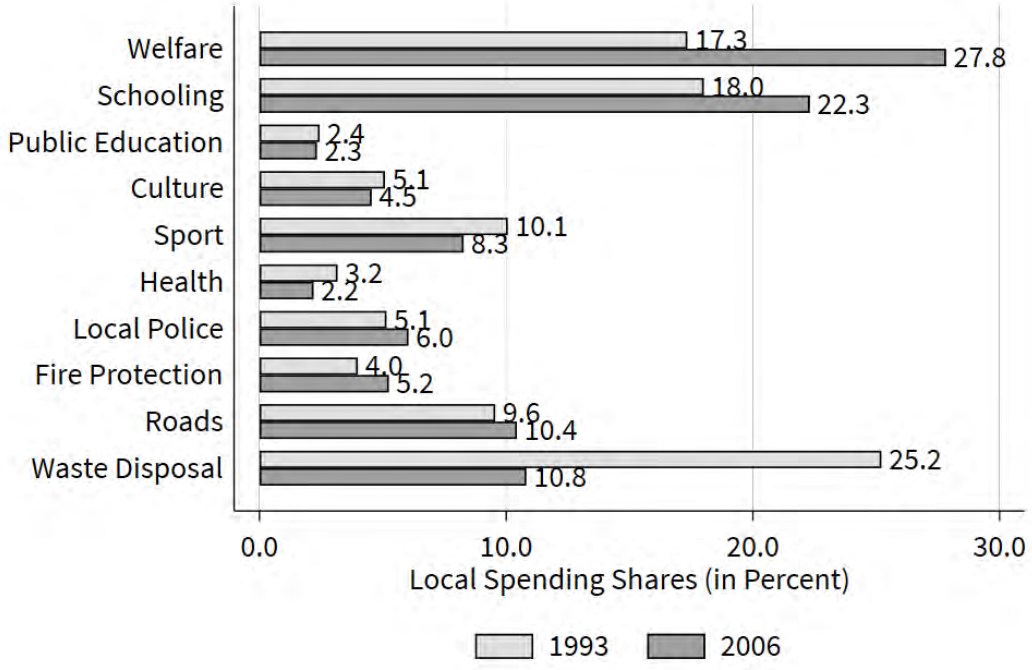
affect fiscal policy, and $\delta_t$ is a year dummy to capture the effect of nation-wide events. We also include several demographic, economic, and political control variables, $X_{ct}$.

Finally, $Deprivation_{ct}$ is a measure of regional economic deprivation. In our empirical analysis, we employ three different measures of economic deprivation: the poverty rate, the poverty gap, and the median gap. Moreover, we consider not only the effect of economic deprivation on the local business tax and property tax rates but also the respective revenues per capita. To account for the skewed distribution of tax revenue, we take logged values.

### 5.5.1 Endogeneity Concerns and Instrumental Variable Approach

Identifying the causal effect of regional economic deprivation on fiscal policy is challenging since there are several confounding factors that are correlated with both fiscal policy and regional economic conditions, such as preferences of local voters or compensatory transfers from the state or federal government (Boustan et al., 2013). We try to control for the first by including the vote share of extreme right- and left-wing parties at the latest federal election in our estimation, and for the latter by including the district's revenue share of the (national) income tax. Nevertheless, we cannot rule out the possibility that there are other relevant variables which we cannot observe and that might distort the results.

Figure 5.4: Composition of Spending on Public Services




*Notes:* This figure shows the average share of per capita spending on the different categories of public services in percent for the years 1993 and 2006 in city districts.

Besides omitted variable bias (OVB), biased estimates as a result of household sorting and reverse causality are another concern. While economic deprivation may induce politicians to adjust local taxes and expenditures, the reverse may also be true. Especially since socio-demographic characteristics as well as preferences for local public good provisions can be decisive for households to settle in a certain region. For example, richer households may prefer to live in regions with higher standards or quality of public services, such as theaters, operas, schools, etc., even though local taxes may be higher. This can lead to a shift in the income distribution.

In order to mitigate concerns regarding biased estimates due to the endogeneity of our covariates, we apply two stage least squares (2SLS) estimation using instrumental variables for our economic deprivation measures. Following Boustan et al. (2013), we use counterfactual (predicted) economic deprivation measures as our instruments for actual economic deprivation. The instruments are constructed as follows. We first compute the average household income for each percentile of the national income distribution for all survey years (1991–2016). Then, we compute the annual national income growth rate for each percentile. Next, we focus on the initial survey year, determine the income percentile each household belongs to based on the national income distribution, and multiply each household's income with the percentile specific national income growth rate. In this way, we obtain hypothetical incomes for all

subsequent sample years for each household we observe in the initial survey year.[11] Finally, we use these hypothetical incomes to compute counterfactual regional economic deprivation measures as instruments.

These economic deprivation measures indicate how regional economic conditions would have developed in the absence of inward and outward migration and whether each household's income would have changed over time in accordance with the percentile-specific national average. Consequently, our instruments only capture changes in the regional income distribution that are driven by national trends and cannot, by design, be influenced by district specific trends such as mobility into and out of regions (Boustan et al., 2013).

Note that the instrumental variables we construct mimic so-called Bartik-style instruments. Goldsmith-Pinkham et al. (2018) discuss the conditions under which these instruments are valid. The authors show that for the exogeneity assumption to hold, differences in initial conditions—here, a district's income distribution in the base year—must be unrelated to changes in (not: levels of) the outcome variable in the following years. To test whether this assumption holds, Goldsmith-Pinkham et al. (2018) propose regressing changes in the outcome variable on the time-invariant variable that indicates the initial conditions interacted with year fixed effects. In principle, this approach resembles an event-study analysis, in which the indicator capturing the conditions in the base year is the (continuous) treatment variable.

In our setup, this implies regressing changes in the tax variables and public spending on the poverty rate, poverty gap, and median gap in the base year interacted with year dummies. The results are presented in Figure A5.1 in the appendix. For the business tax rate and the property tax rate, the event study coefficients are insignificant at every reasonable level of significance, thus indicating the validity of the IV approach. For aggregate spending, we obtain significant event study coefficients in the first three sample years. In later years, however, the coefficients become notably smaller (in absolute terms) and statistically insignificant. We are thus confident that the bias in the IV estimate, when using aggregate spending as the dependent variable, is negligible (if existent at all).

An additional challenge specific to the use of county-level data in Germany is that the number of counties in East Germany has changed considerably after German unification due to various administrative-territorial reforms. For example, from 1990 to 1996, the number of counties in East Germany (excluding East Berlin) dropped from 215 to 111. For this reason, we are forced to use the income distribution of 1997 to construct our instruments for East German counties. This, however, implies that we cannot use observations on East German counties prior to 1998 when employing an instrumental variable approach.

[11] To account for the fact that at especially at the beginning of the 1990s, East and West Germany were still very different, we follow this procedure separately for East and West Germany.

## 5.6 Empirical Results

In this section, we present our empirical results. We start by presenting our results for the business and property tax in Section 5.6.1. Section 5.6.2 presents OLS and 2SLS results for aggregate spending, as well as for the individual spending categories.

### 5.6.1 Local Taxation

When addressing the question how economic deprivation in German city districts affects local taxation, we concentrate on the effect on local business tax and the property tax B in our main analysis, as these are the more important tax instruments in terms of revenue collection.[12] Both the effect on the respective tax rate and the revenue is reported. We distinguish between two sample periods: a full sample from 1993 to 2016 and a restricted sample from 1993 to 2006. The restricted sample corresponds to the period for which the spending data is also available.

#### Full Sample: 1993–2016

For the full sample period from 1993 to 2016, OLS results for the relationship between economic deprivation and the local business tax are presented in Table 5.1. For all three measures of economic deprivation, there is a positive effect on both the local business tax rate (see columns (1), (2), and (3)) and the tax revenue (see columns (4), (5), and (6)).[13] However, this effect is not statistically significant at the ten percent level. Moreover, OLS is likely to produce biased estimates (see Section 5.5.1).

Table 5.2 therefore shows 2SLS estimates for the relationship between economic deprivation and the local business tax. For all three measures of economic deprivation, there is a positive effect on the local business tax rate. For instance, a one percentage point increase in the poverty rate leads to an increase in the local business tax rate of 3.7 percentage points, which translates into an increase of the average local business tax rate by approximately one percent in relation to the sample mean (see column (1)). An increase in the poverty gap has the largest economic effect: a one percentage point increase in the poverty gap causes the local business tax rate to increase by 5.4 percentage points (see column (2)). The median gap has the lowest economic effect.

In comparison to the OLS estimates, the 2SLS coefficients are larger and statistically significant. This shows that OLS estimates are indeed biased, for example, due to an endogeneity problem, sorting, or reverse causality. In this respect, our results are in line with Boustan et al. (2013). We report both the Cragg-Donald and the Kleibergen-Paap F statistics for all three measures

[12] Results for the property tax A are reported in the appendix.
[13] Note that tax revenue is in log per capita.

## Table 5.1: Local Business Tax—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.129 | | | 0.003 | | |
| | [0.391] | | | [0.355] | | |
| Poverty Gap | | 0.049 | | | 0.011 | |
| | | [0.896] | | | [0.225] | |
| Median Gap | | | 0.032 | | | 0.003 |
| | | | [0.892] | | | [0.647] |
| Population (Log) | -62.607** | -64.543** | -64.510** | -0.334 | -0.331 | -0.367 |
| | [0.017] | [0.013] | [0.015] | [0.175] | [0.165] | [0.133] |
| Share Foreigners | 0.109 | 0.119 | 0.119 | 0.009 | 0.009 | 0.009 |
| | [0.820] | [0.804] | [0.805] | [0.176] | [0.163] | [0.165] |
| Dependency Ratio | 0.321 | 0.307 | 0.306 | 0.007* | 0.007* | 0.007 |
| | [0.146] | [0.162] | [0.161] | [0.092] | [0.085] | [0.104] |
| Mean Income | -0.002 | -0.002* | -0.002* | 0.000 | 0.000 | 0.000 |
| | [0.237] | [0.062] | [0.060] | [0.212] | [0.209] | [0.209] |
| Radical Right | 3.405*** | 3.400*** | 3.399*** | -0.012 | -0.012 | -0.013 |
| | [0.007] | [0.008] | [0.007] | [0.307] | [0.327] | [0.302] |
| Radical Left | -1.008** | -1.025** | -1.026** | 0.003 | 0.003 | 0.002 |
| | [0.037] | [0.035] | [0.034] | [0.713] | [0.699] | [0.749] |
| Income Tax Revenue (Log) | 0.013 | -0.139 | -0.121 | 0.176*** | 0.173*** | 0.173*** |
| | [0.997] | [0.972] | [0.976] | [0.000] | [0.000] | [0.000] |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 414.95 | 414.95 | 414.95 | 6.03 | 6.03 | 6.03 |
| $R^2$ | 0.34 | 0.34 | 0.34 | 0.48 | 0.48 | 0.48 |
| N | 2214 | 2214 | 2214 | 2222 | 2222 | 2222 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

## Table 5.2: Local Business Tax—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 3.734** | | | 0.051** | | |
| | [0.037] | | | [0.023] | | |
| Poverty Gap | | 5.453** | | | 0.115*** | |
| | | [0.038] | | | [0.005] | |
| Median Gap | | | 2.936* | | | 0.055** |
| | | | [0.093] | | | [0.040] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 414.95 | 414.95 | 414.95 | 6.03 | 6.03 | 6.03 |
| N | 2214 | 2214 | 2214 | 2222 | 2222 | 2222 |
| Cragg-Donald | 46.08 | 85.72 | 73.77 | 47.40 | 85.19 | 72.94 |
| Kleibergen-Paap | 13.01 | 35.68 | 31.23 | 13.33 | 35.20 | 30.73 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

of economic deprivation. As both exceed the respective critical values of the weak instrument test proposed by Stock and Yogo (2005),[14] we can reject the weak instrument assumption.

Economic deprivation does not only lead to a higher local business tax rate but also to a significant increase in the local business tax revenue. The magnitude and the statistical significance on local business tax revenue is similar for the poverty rate and the median gap: a one percentage point increase in either measure leads to a 0.05 percent increase in local business tax revenue per capita. With an increase in revenue by 0.1 percent, the effect of a one percentage point increase in the poverty gap is even larger and the effect is statistically significant also at the one percent level.

For the property tax B rate and revenue, Table 5.3 shows OLS estimates. The 2SLS estimates are displayed in Table 5.4.

Table 5.3: Property Tax B—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.287 | | | 0.001 | | |
| | [0.334] | | | [0.429] | | |
| Poverty Gap | | 0.086 | | | 0.002 | |
| | | [0.891] | | | [0.248] | |
| Median Gap | | | 0.351 | | | 0.001 |
| | | | [0.430] | | | [0.475] |
| Population (Log) | -5.469 | -9.827 | -7.156 | -0.537*** | -0.538*** | -0.540*** |
| | [0.922] | [0.861] | [0.899] | [0.000] | [0.000] | [0.000] |
| Share Foreigners | 0.396 | 0.419 | 0.415 | -0.001 | -0.001 | -0.001 |
| | [0.699] | [0.685] | [0.687] | [0.521] | [0.535] | [0.533] |
| Dependency Ratio | 0.327 | 0.294 | 0.308 | -0.001 | -0.001 | -0.001 |
| | [0.474] | [0.516] | [0.498] | [0.416] | [0.420] | [0.393] |
| Mean Income | -0.000 | -0.002 | -0.001 | -0.000 | -0.000 | -0.000 |
| | [0.820] | [0.499] | [0.774] | [0.650] | [0.664] | [0.653] |
| Radical Right | 12.147*** | 12.136*** | 12.150*** | 0.019*** | 0.020*** | 0.020*** |
| | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] | [0.000] |
| Radical Left | -1.750** | -1.790** | -1.769** | -0.005*** | -0.005*** | -0.005*** |
| | [0.020] | [0.017] | [0.018] | [0.001] | [0.001] | [0.001] |
| Income Tax Revenue (Log) | -26.786*** | -27.124*** | -26.868*** | -0.042*** | -0.042*** | -0.042*** |
| | [0.007] | [0.006] | [0.007] | [0.009] | [0.008] | [0.008] |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 428.12 | 428.12 | 428.12 | 4.92 | 4.92 | 4.92 |
| $R^2$ | 0.59 | 0.59 | 0.59 | 0.87 | 0.87 | 0.87 |
| N | 2215 | 2215 | 2215 | 2224 | 2224 | 2224 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

The point estimates suggest a positive relationship between economic deprivation and the property tax B. Again, the magnitude of the OLS results is downward-biased in comparison to the 2SLS estimates. However, even for the 2SLS estimates, we do not find a statistically significant effect of any of our economic deprivation measures on the property tax B rate or

[14] The critical values for the Stock-Yogo weak ID F-test are 16.38 (ten percent maximal IV size), 8.96 (15 percent), 6.66 (20 percent), and 5.53 (25 percent).

Table 5.4: Property Tax B—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.889 | | | 0.003 | | |
| | [0.740] | | | [0.637] | | |
| Poverty Gap | | 1.447 | | | -0.000 | |
| | | [0.726] | | | [0.980] | |
| Median Gap | | | -1.355 | | | -0.003 |
| | | | [0.638] | | | [0.723] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 428.12 | 428.12 | 428.12 | 4.92 | 4.92 | 4.92 |
| N | 2215 | 2215 | 2215 | 2224 | 2224 | 2224 |
| Cragg-Donald | 47.61 | 86.45 | 74.76 | 47.23 | 85.12 | 72.76 |
| Kleibergen-Paap | 13.29 | 36.17 | 31.71 | 13.29 | 35.21 | 30.67 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

revenue.[15] Based on the Cragg-Donald and the Kleibergen-Paap F statistics, we can reject the weak instrument assumption for this specification as well.

**Restricted Sample: 1993–2006**

The estimates of the relationship between economic deprivation and local taxation are based on a sample covering the period from 1993 to 2016. However, in the next section, where we focus on the relationship between economic deprivation and public spending, the sample period only ranges from 1993 to 2006, as spending data is not available for later years. To make sure that our results remain stable over time, we re-estimate the tax regressions using only the period from 1993 to 2006. Table 5.5 shows 2SLS estimates for the local business tax.[16]

The point estimates for all three measures of economic deprivation are again positive and statistically significant. In comparison to the full sample, the magnitude of the point estimates has even increased, suggesting that the economic impact of economic deprivation on the local business tax rate is even greater during the years 1993 to 2006. In line with this result, the local business tax revenue increases as well. However, this effect is only statistically significant for the poverty gap and the median gap.

The 2SLS results for the property tax B are summarized in Table 5.6.[17] Similarly to the results for the full sample, the point estimates for all three measures of economic deprivation are

[15] Results for the property tax A rate and revenue are shown in Tables A5.3 and A5.4 in the appendix.
[16] OLS estimates for the restricted sample for the local business tax are shown in Table A5.5 in the appendix.
[17] OLS estimates for the restricted sample for the property tax B are shown in Table A5.6 in the appendix.

Table 5.5: Restricted Sample: Local Business Tax—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 7.510** | | | 0.042 | | |
| | [0.040] | | | [0.135] | | |
| Poverty Gap | | 15.148** | | | 0.164* | |
| | | [0.024] | | | [0.064] | |
| Median Gap | | | 7.774*** | | | 0.062** |
| | | | [0.001] | | | [0.043] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 410.05 | 410.05 | 410.05 | 5.86 | 5.86 | 5.86 |
| N | 1204 | 1204 | 1204 | 1210 | 1210 | 1210 |
| Cragg-Donald | 15.67 | 16.74 | 34.85 | 15.69 | 17.22 | 35.94 |
| Kleibergen-Paap | 5.26 | 5.62 | 15.76 | 5.30 | 5.86 | 16.60 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

positive, both for the property tax B rate and revenue. However, the effects are not statistically significant at the ten percent level.

Table 5.6: Restricted Sample: Property Tax B—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 2.161 | | | 0.007 | | |
| | [0.439] | | | [0.421] | | |
| Poverty Gap | | 4.522 | | | 0.014 | |
| | | [0.446] | | | [0.441] | |
| Median Gap | | | 3.362 | | | 0.010 |
| | | | [0.191] | | | [0.195] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 404.48 | 404.48 | 404.48 | 4.80 | 4.80 | 4.80 |
| N | 1205 | 1205 | 1205 | 1211 | 1211 | 1211 |
| Cragg-Donald | 15.96 | 16.92 | 35.42 | 15.86 | 17.23 | 35.98 |
| Kleibergen-Paap | 5.34 | 5.70 | 15.97 | 5.30 | 5.86 | 16.61 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

### 5.6.2 Spending on Local Public Services

Next, we turn to the expenditure side of the local budget and the question how economic deprivation affects how much city districts spend on certain tasks. Our measures for economic

deprivation are again the poverty rate, the poverty gap, and the median gap. The time period under consideration is 1993 to 2006. Table 5.7 shows OLS results for the effect of economic deprivation on aggregate spending on voluntary and mandatory self-government tasks.

Table 5.7: Aggregate Spending—OLS

| | (1) | (2) | (3) |
|---|---|---|---|
| Poverty Rate | -0.004** | | |
| | [0.041] | | |
| Poverty Gap | | -0.008* | |
| | | [0.067] | |
| Median Gap | | | -0.005 |
| | | | [0.122] |
| Population (Log) | -0.305 | -0.279 | -0.267 |
| | [0.321] | [0.363] | [0.390] |
| Share Foreigners | 0.002 | 0.002 | 0.002 |
| | [0.620] | [0.656] | [0.676] |
| Dependency Ratio | -0.005** | -0.005** | -0.005** |
| | [0.012] | [0.013] | [0.015] |
| Mean Income | -0.000 | 0.000 | -0.000 |
| | [0.928] | [0.743] | [0.910] |
| Radical Right | 0.001 | 0.001 | 0.000 |
| | [0.929] | [0.921] | [0.965] |
| Radical Left | 0.011*** | 0.012*** | 0.012*** |
| | [0.008] | [0.006] | [0.005] |
| Income Tax Revenue (Log) | -0.220** | -0.215** | -0.221** |
| | [0.040] | [0.042] | [0.038] |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.91 | 6.91 | 6.91 |
| $R^2$ | 0.10 | 0.10 | 0.10 |
| N | 1203 | 1203 | 1203 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

For all three measures of economic deprivation, the point estimates are negative. However, the magnitude of the coefficients is low and hence, they are not economically relevant. Moreover, as outlined in Section 5.5.1, OLS estimates are likely to be biased.

Comparing the OLS results with the 2SLS results from Table 5.8 suggests that the OLS estimates are biased towards zero. The 2SLS estimates imply that there is a negative impact of economic deprivation on aggregate spending. For instance, aggregate spending in city districts is reduced by 0.123 percent when the poverty gap increases by one percentage point. This impact is also statistically significant at the five percent level. Only the negative effect of the poverty rate on aggregate spending is statistically insignificant.

Table 5.8 shows how aggregate spending on voluntary and mandatory self-government tasks reacts to economic deprivation. However, it is also of interest how the individual categories are affected. Figure 5.5 therefore plots the 2SLS coefficients for all spending categories by measure of economic deprivation.[18]

[18] Tables A5.7, A5.8, and A5.9 in the appendix also summarize the 2SLS coefficients for the spending categories by measure of economic deprivation.

Table 5.8: Aggregate Spending—2SLS

| | (1) | (2) | (3) |
|---|---|---|---|
| Poverty Rate | -0.023 | | |
| | [0.146] | | |
| Poverty Gap | | -0.123** | |
| | | [0.046] | |
| Median Gap | | | -0.053*** |
| | | | [0.006] |
| Demographic Controls | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.91 | 6.91 | 6.91 |
| N | 1202 | 1202 | 1202 |
| Cragg-Donald | 14.57 | 17.32 | 36.77 |
| Kleibergen-Paap | 4.84 | 5.85 | 17.07 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

Across all three measures of economic deprivation, the pattern for the effect on the categories of spending is fairly similar. For most categories, the effect of economic deprivation is not statistically significant at the ten percent level. However, spending on welfare, schooling, and sport is reduced significantly when any of the three measures of economic deprivation increases by one percentage point. In addition, a one percentage point increase in the poverty gap (median gap) leads to a significant reduction in spending on culture (roads).

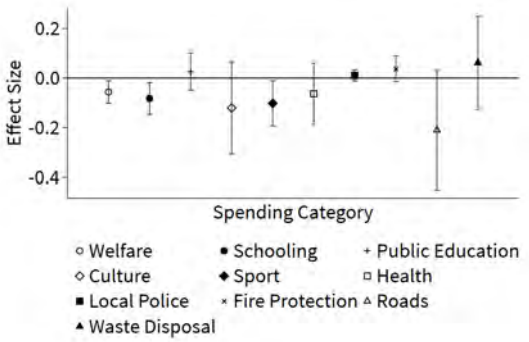
## 5.7 Discussion

What are the implications of our results for the effect of economic deprivation on redistribution? It seems that local politicians actually want to make the tax system more redistributive in response to greater economic deprivation: they raise the local business tax rate, a tax on businesses. While Fuest et al. (2018) show that the actual incidence of the local business tax is shared between corporations and workers, it is unlikely that local politicians consider aspects such as actual tax incidence in their fiscal policy decisions. Instead, it seems more likely that the perception of the local business tax as a tax on corporations is what drives policy makers. Thus, raising the local business tax rate is likely to be viewed by the general public as a redistributive fiscal policy.

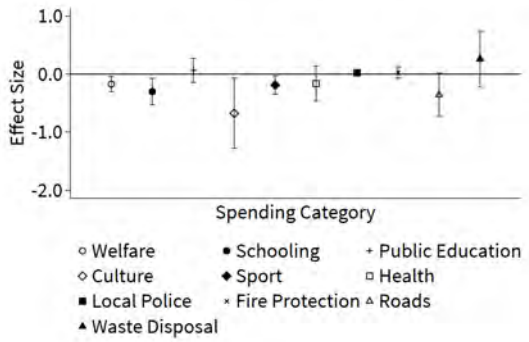
In addition to the positive effect on the local business tax, our results reveal that policy makers do not make statistically significant changes to the property tax B rate in reaction to increasing economic deprivation. The incidence as well as the perception of the property tax are less straightforward than in the case of the local business tax. On the one hand, it is a tax on real estate, and in Germany higher income groups are more likely to own property (Dustmann et al.,

Figure 5.5: Categories of Public Services—2SLS

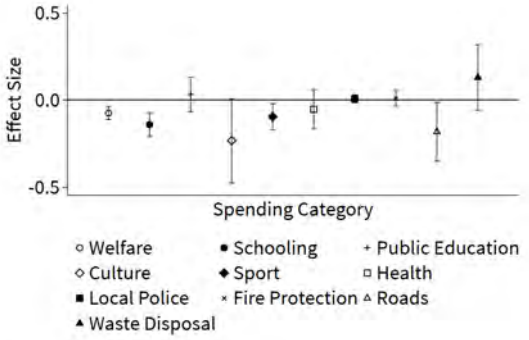
(a) Poverty Rate




(b) Poverty Gap




(c) Median Gap




*Notes:* This figure plots the 2SLS coefficients and the 90 percent confidence interval for all spending categories by measure of economic deprivation. Economic deprivation measures are measured in percent.

2018). On the other hand, as it is part of the apportionable service charges, landlords can pass it on to their tenants. Indeed, there is empirical evidence that in the long run tenants bear the burden of the property tax (Löffler and Siegloch, 2018). Thus, there is some degree of ambiguity in the perception of the property tax B incidence. This might be an explanation for the fact that policy makers hesitate to adjust the property tax B in reaction to increasing economic deprivation. Therefore, our results for the effect of economic deprivation on local taxes rather speak for the intention of local politicians to make the tax system more redistributive. These results are in line with Borge and Rattsø (2004) and Chevalier et al. (2018).

In contrast, our results for spending on local public services suggest that increasing economic deprivation leads to less redistribution. The provision of most public services should benefit the lower income groups in particular as they cannot afford private alternatives. A cut in spending on public services is thus most likely perceived as regressive fiscal policy.

However, different categories of public services also target different income groups. For instance, cultural public services, like theater or opera, tend to be perceived as benefiting higher income groups. Other public services, such as welfare, tend to benefit lower income

groups. Moreover, with respect to some public services, such as schooling, it is unclear who benefits most. One the one hand, one could argue that lower income groups benefit most from high-quality public provision of educational infrastructure. On the other hand, Hayo and Neumeier (2019) show that an investment in human capital is preferred by those with higher education and income. As we find that spending on welfare decreases in reaction to economic deprivation, this fiscal measure is more likely to harm lower income groups. The impact of the decrease in spending on schooling and sport facilities is more controversial. But here, too, it is more likely that higher income groups could replace missing or low quality public provision with private alternatives.

Comparing our work with the most closely related paper by Boustan et al. (2013), the results seem to contradict one another at first glance. Boustan et al. (2013) find that general expenditure is positively related to inequality. However, this overall effect in their paper is mainly driven by increases in the expenditure on police and fire protection, while the coefficients for welfare or health spending are statistically insignificant. Thus, the overall expenditure effect in Boustan et al. (2013) is driven by increases in spending on public services that are generally not considered redistributive. Similarly, the coefficients for police and fire protection in our data are positive, although statistically insignificant.

Finally, it also needs to be noted that our data on local spending does not include all possible forms of financing. For instance, we do not have data on public private partnerships. Hence, we are not able to account for investments in public goods, e.g., the construction of a hospital, that are financed by means of public private partnerships.

Given any shortcoming in the data, an important difference to the previous literature on the sub-national level is that the previous literature has focused mainly on the U.S., which has a majority rule system. In such a setting the assumptions of the median voter theorem seem applicable. Germany, on the other hand, follows the principle of proportional voting. Thus, coalition governments are possible and frequent.

This could affect our results since Freier and Odendahl (2012) show that in municipalities with a majority government spending increases. They argue that majority governments can reach agreement more easily, whereas divided governments in a coalition cannot agree on public spending and may therefore forego it altogether. As coalition governments are frequent in German city districts, this inability to decide could be an explanation for the decrease in spending that we observe. At the same time, the decision on local taxation is more straightforward, as there are only two main tax instruments to decide on. Thus, our ambiguous results regarding the relationship between economic deprivation and redistribution for local taxation and spending could at least partly be due to the particular political system in Germany.

We test this possibility by creating two indicator variables, which are proxies for unified and divided governments: *Unified*, which is equal to one if one party obtains more than 50 percent of the votes in the latest city council election, and *Divided*, which equals one if no party

obtained more than 50 percent of the votes in the latest city council election. We interact both indicator variables with our economic deprivation measures.

As the coefficients for both interaction terms are very similar across the different specifications, we do not find any evidence to support the hypothesis that unified and divided governments make different fiscal policy decisions in reaction to economic deprivation.[19] This also holds for the subcategories of spending, especially for those categories for which we find statistically significant results. Thus, we do not find any evidence to support the hypothesis that divided governments drive our results.

As we find that tax revenue increases while spending goes down, an interesting question is whether policy makers decide to use the excess revenue to reduce debt instead. We therefore plan to extend our analysis to the question of how the level of debt in city districts is affected by economic deprivation. Furthermore, increasing economic deprivation could mean that the earmarked grants which the city districts receive are no longer sufficient, particularly for social welfare. As a consequence, local policy makers could be forced to contribute a part of their own budget to finance some of these tasks which, strictly speaking, do not fall under their own spheres of influence. Due to data availability, we currently cannot test this possibility but plan to do so in the future.

Finally, an open question is how economic deprivation actually translates into fiscal policy measures. For that to happen, policy makers and their voters need to be aware of it. One possible mechanism of transmission is increasing segregation as income heterogeneity in a community increases. Previous literature shows that income inequality seems to be accompanied by spatial inequality as different societal groups in the population are concentrated in different areas (Musterd et al., 2017; Reardon and Bischoff, 2011). Helbig and Jähnen (2018) study segregation in 74 bigger cities in Germany. This is particularly relevant for our study, as we focus on city districts in Germany, which more or less include the cities they analyze. Helbig and Jähnen (2018) note, in fact, that spatial segregation by social groups has increased in these cities in their sample period from 2002 to 2014. For instance, a growing number of receivers of social benefits lives in certain parts of the city. Hence, increasing income inequality and economic deprivation seem to manifest spatially and become apparent in this way. Attentive policy makers could therefore be aware of them and make corresponding political decisions.

## 5.8 Robustness Test

In order to test the robustness of our results, we run additional analyses. First, since it is possible to argue that it takes some time for economic deprivation to translate into actual

[19] 2SLS results for the local business tax, property tax B, and spending can be found in Tables A5.10, A5.11, and A5.12 in the appendix.

fiscal policy, we run a lagged specification of Equation 5.3. More precisely, we lag all the economic deprivation measures and the corresponding instruments by one year but not the control variables. We consider the full sample from 1993 to 2016, when using taxation outcomes.

2SLS estimates for the local business tax are reported in Table A5.14 in the appendix.[20] The point estimates for the local business tax rate and the revenue are positive and statistically significant. Compared to our main specification (see Table 5.2), the effect on the local business tax rate is even higher in the lagged specification. This could indicate that it does indeed take some time for policy makers to react to economic deprivation. Table A5.16 in the appendix summarizes the lagged 2SLS effects for the property tax B.[21] Similar to our main specification, the effects are not statistically significant.

Table A5.18 in the appendix shows the effect of economic deprivation on aggregate spending for public services for the lagged 2SLS specification.[22] Similar to our main specification in Table 5.8, the point estimates suggest a negative relationship. Moreover, the magnitude and the statistical significance is very similar to those in our main specification. Hence, our results are robust to lagging the economic deprivation measures.

Second, to ensure that our results are not driven by outliers in the data, we winsorize our economic deprivation measures at the one percent and five percent level. The results for the local business tax rate and revenue are robust to winsorizing at both levels as Tables A5.19 and A5.20 in the appendix show. The same is true for the property tax B rate and revenue (see Tables A5.21 and A5.22 in the appendix). Lastly, Tables A5.23 and A5.24 in the appendix show the winsorized results for aggregate spending. Again, both the magnitude of the point estimates and their statistical significance are very similar to our main results. This suggests that our results are not driven by outliers in the data.

## 5.9 Conclusion

Fiscal policy determines many important economic, social and political outcomes. Understanding the factors that affect fiscal policy is therefore highly relevant. Income inequality is found to be such a factor (Meltzer and Richard, 1981). However, it remains controversial whether there is a positive or negative relationship between income inequality and redistributive fiscal policy measures.

This is where this chapter aims to make a contribution. First, in contrast to the previous literature, we do not use a general measure of income inequality like the Gini coefficient but focus on inequality driven by the lower bound of the income distribution. Specifically, we

[20] OLS results are shown in Table A5.13 in the appendix.
[21] OLS results are shown in Table A5.15 in the appendix.
[22] OLS results are shown in Table A5.17 in the appendix.

study the effect of three different measures of economic deprivation—the poverty gap, the poverty rate, and the median gap—on fiscal policy. This kind of inequality is of particular importance in the given context as the impact of redistributive measures is even greater when people cannot afford private alternatives due to increasing economic deprivation.

Second, we exploit the specific institutional setting in Germany which grants municipalities and districts a high degree of fiscal autonomy. They can set a number of different taxes and can decide on spending for several public services. We use this rich level in local variation to identify the effect of economic deprivation on fiscal policy. For our analysis, we combine administrative panel data on the universe of German city districts with measures for economic deprivation from the German Microcensus.

Third, using instrumental variables estimation allows us to draw conclusions about the causal influence of economic deprivation on local fiscal policy in Germany. Our instrument predicts changes in regional economic deprivation through national income trends, which allows us to overcome confounding effects like mobility and spatial segregation.

We find that increasing economic deprivation causes local policy makers to increase the local business tax rate, while we do not find significant effects on local property taxes. Given that the local business tax is likely to be perceived as a progressive tax, whereas the perception of the property tax is more ambivalent, this seems to be an attempt to make the tax system more redistributive as economic deprivation increases. On the other hand, aggregate spending on local public services is negatively affected by economic deprivation. This effect is driven in particular by cuts in spending on welfare and schooling. As lower income groups probably benefit most from these public services, our results suggest a negative relationship between economic deprivation and redistributive fiscal policies on the expenditure side.

A possible explanation for our ambiguous findings regarding the relationship between economic deprivation and redistribution may lie in the German political system, which makes coalition governments likely. Such divided governments might not be able to agree on spending on public services, especially as the different parties might serve different voter groups. Decisions on taxation, on the other hand, are more straightforward as there are only two main tax instruments on the local level. We therefore test whether unified or divided city councils make different fiscal policy decision in reaction to increasing economic deprivation. However, our results do not provide any evidence to support this hypothesis. Further explanations could be that city districts use the excess tax revenue to reduce debt or are forced to invest part of their budget in earmarked grants as economic deprivation increases social welfare costs. We plan to consider both options in future research.

In summary, our results suggest that economic deprivation is an important factor determining fiscal policy on the local level. However, there remains some ambiguity as to the nature of the effect, which warrants future research.

# Appendix

## Additional Tables

### Table A5.1: Summary Statistics

| | 1993 - 2016 | | 1993 | | 2006 | | 2016 | |
|---|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | mean | sd | mean | sd |
| ***Economic Deprivation Measures:*** | | | | | | | | |
| Poverty Rate | 18.4 | 5.5 | 14.2 | 5.0 | 18.7 | 5.8 | 21.1 | 5.2 |
| Poverty Gap | 4.9 | 1.9 | 4.0 | 1.9 | 4.7 | 1.9 | 5.6 | 2.1 |
| Median Gap | 16.8 | 3.6 | 14.1 | 3.3 | 17.1 | 3.9 | 18.3 | 3.5 |
| ***Local Taxation:*** | | | | | | | | |
| Business Tax Rate | 415.1 | 39.6 | 403.8 | 36.6 | 414.3 | 37.4 | 434.1 | 43.9 |
| Property Tax B Rate | 428.3 | 71.5 | 365.7 | 44.9 | 424.8 | 50.5 | 501.6 | 101.7 |
| Property Tax A Rate | 288.1 | 68.3 | 256.6 | 61.8 | 285.4 | 63.8 | 315.5 | 72.6 |
| Business Tax Revenues | 500.7 | 343.8 | 392.7 | 150.6 | 595.4 | 423.2 | 689.4 | 408.6 |
| Property Tax B Revenues | 142.5 | 40.5 | 98.5 | 20.5 | 143.0 | 32.9 | 181.7 | 42.9 |
| Property Tax A Revenues | 0.9 | 0.9 | 0.9 | 0.9 | 0.9 | 0.9 | 1.0 | 0.9 |
| ***Local Spending:*** | | | | | | | | |
| Aggr. Spending | 1027.6 | 230.0 | 996.6 | 217.0 | 1051.3 | 237.0 | . | . |
| Welfare | 238.7 | 75.0 | 165.9 | 39.3 | 283.0 | 72.6 | . | . |
| Culture | 61.4 | 56.6 | 49.3 | 45.9 | 48.8 | 43.8 | . | . |
| Schooling | 204.8 | 77.5 | 176.3 | 60.6 | 233.7 | 86.2 | . | . |
| Public Education | 24.8 | 11.4 | 24.1 | 12.4 | 24.1 | 11.0 | . | . |
| Sport | 91.2 | 39.3 | 97.8 | 33.7 | 87.0 | 38.3 | . | . |
| Health | 24.7 | 23.0 | 33.6 | 31.2 | 23.4 | 19.3 | . | . |
| Fire Protection | 48.2 | 18.3 | 39.0 | 15.1 | 53.7 | 20.0 | . | . |
| Local Police | 56.0 | 14.3 | 49.0 | 12.5 | 61.7 | 14.0 | . | . |
| Roads | 103.3 | 53.8 | 92.6 | 37.3 | 110.0 | 59.4 | . | . |
| Waste Disposal | 174.5 | 151.3 | 269.0 | 144.6 | 125.9 | 135.6 | . | . |
| ***Control Variables:*** | | | | | | | | |
| Population (Log) | 11.8 | 0.8 | 11.8 | 0.9 | 11.7 | 0.8 | 11.8 | 0.9 |
| Share Foreigners | 11.4 | 5.6 | 12.4 | 5.1 | 11.2 | 5.5 | 14.1 | 5.5 |
| Dependency Ratio | 32.8 | 3.2 | 31.5 | 3.0 | 32.8 | 3.2 | 33.3 | 3.2 |
| Mean Income | 1473.6 | 212.5 | 1446.2 | 105.1 | 1440.4 | 166.8 | 1590.5 | 163.0 |
| Radical Right | 3.0 | 2.1 | 3.0 | 1.7 | 2.2 | 1.1 | 6.4 | 1.5 |
| Radical Left | 7.8 | 8.5 | 0.4 | 0.2 | 9.1 | 8.1 | 9.5 | 7.1 |
| Income Tax Revenue (Log) | 5.6 | 0.4 | 5.8 | 0.1 | 5.5 | 0.4 | 5.6 | 0.7 |
| Observations | 2231 | | 86 | | 103 | | 94 | |

*Notes:* The poverty rate, the poverty gap, and the median gap are measured in percent. Dependency ratio, foreigners, unemployment and transfer recipients are measured in percent of the population, schooling outcomes are measured in percent of graduates. Tax revenues and spending categories are in Euros per capita terms.

Table A5.2: Construction of Expenditure Variables

| Classification code in annual account data | Variable |
|---|---|
| 11 | Public Order |
| 13 | Fire protection |
| 2 | Schools |
| 32 (321+323), 33 (331+332+333), 34 | Culture |
| 451-458 + 460-468 + 47 + 470 | Welfare |
| 51 + 54 | Health |
| 55 + 56 + 57 + 58 + 59 | Sports |
| 63 + 65 | Infrastructure |
| 70 + 72 | Waste |

*Notes:* The table shows how we mapped expenditure items that fall under the definition of voluntary and mandatory self-government tasks into our final variables based on their classification code in in the annual account data. In our definition of voluntary and mandatory self-government tasks we follow (Postlep, 1987).

## Table A5.3: Property Tax A—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | -0.010 | | | 0.004* | | |
| | [0.973] | | | [0.081] | | |
| Poverty Gap | | 0.142 | | | 0.012** | |
| | | [0.842] | | | [0.029] | |
| Median Gap | | | -0.151 | | | 0.007* |
| | | | [0.743] | | | [0.069] |
| Population (Log) | 71.498* | 72.391* | 70.319* | -0.217 | -0.231 | -0.234 |
| | [0.090] | [0.082] | [0.095] | [0.504] | [0.470] | [0.467] |
| Share Foreigners | -0.214 | -0.215 | -0.213 | -0.009 | -0.009 | -0.009 |
| | [0.830] | [0.828] | [0.831] | [0.112] | [0.117] | [0.119] |
| Dependency Ratio | 0.337 | 0.345 | 0.330 | 0.002 | 0.002 | 0.002 |
| | [0.551] | [0.542] | [0.557] | [0.542] | [0.535] | [0.602] |
| Mean Income | 0.003 | 0.003 | 0.002 | -0.000 | -0.000 | 0.000 |
| | [0.263] | [0.251] | [0.300] | [0.874] | [0.634] | [0.889] |
| Radical Right | 6.003*** | 6.012*** | 5.996*** | 0.016** | 0.017** | 0.016** |
| | [0.000] | [0.000] | [0.000] | [0.046] | [0.041] | [0.047] |
| Radical Left | -0.268 | -0.257 | -0.277 | -0.004 | -0.004 | -0.004 |
| | [0.654] | [0.663] | [0.640] | [0.457] | [0.456] | [0.435] |
| Income Tax Revenue (Log) | 12.749** | 12.779** | 12.646** | 0.101*** | 0.097*** | 0.100*** |
| | [0.018] | [0.018] | [0.019] | [0.001] | [0.002] | [0.002] |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 287.67 | 287.67 | 287.67 | -0.43 | -0.43 | -0.43 |
| $R^2$ | 0.41 | 0.41 | 0.41 | 0.06 | 0.06 | 0.06 |
| N | 2215 | 2215 | 2215 | 2217 | 2217 | 2217 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent, revenue variables are logged per capita.

## Table A5.4: Property Tax A—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 2.738 | | | 0.045** | | |
| | [0.219] | | | [0.037] | | |
| Poverty Gap | | 4.119 | | | 0.080* | |
| | | [0.335] | | | [0.061] | |
| Median Gap | | | 0.119 | | | 0.045 |
| | | | [0.966] | | | [0.111] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 287.67 | 287.67 | 287.67 | -0.43 | -0.43 | -0.43 |
| N | 2215 | 2215 | 2215 | 2217 | 2217 | 2217 |
| Cragg-Donald | 47.61 | 86.45 | 74.76 | 48.02 | 85.00 | 73.81 |
| Kleibergen-Paap | 13.29 | 36.17 | 31.71 | 13.63 | 35.27 | 31.25 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

## Table A5.5: Restricted Sample: Local Business Tax—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.292** | | | 0.001 | | |
| | [0.049] | | | [0.786] | | |
| Poverty Gap | | 0.496 | | | 0.003 | |
| | | [0.120] | | | [0.758] | |
| Median Gap | | | 0.269 | | | 0.001 |
| | | | [0.255] | | | [0.908] |
| Population (Log) | -79.683** | -83.349** | -84.720** | -0.455 | -0.452 | -0.477 |
| | [0.032] | [0.025] | [0.027] | [0.368] | [0.375] | [0.345] |
| Share Foreigners | -0.102 | -0.086 | -0.080 | 0.008 | 0.008 | 0.008 |
| | [0.815] | [0.846] | [0.857] | [0.275] | [0.275] | [0.276] |
| Dependency Ratio | -0.599** | -0.607** | -0.614** | 0.003 | 0.003 | 0.003 |
| | [0.013] | [0.012] | [0.011] | [0.610] | [0.609] | [0.616] |
| Mean Income | -0.001 | -0.001** | -0.001** | 0.000 | 0.000 | 0.000 |
| | [0.138] | [0.033] | [0.039] | [0.141] | [0.166] | [0.158] |
| Radical Right | 0.104 | 0.108 | 0.143 | 0.002 | 0.002 | 0.002 |
| | [0.906] | [0.903] | [0.871] | [0.892] | [0.899] | [0.884] |
| Radical Left | -0.999** | -1.027** | -1.044** | -0.001 | -0.001 | -0.001 |
| | [0.027] | [0.023] | [0.022] | [0.877] | [0.880] | [0.856] |
| Income Tax Revenue (Log) | 14.711 | 14.445 | 14.809 | 0.244 | 0.242 | 0.244 |
| | [0.201] | [0.209] | [0.198] | [0.149] | [0.152] | [0.148] |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 410.05 | 410.05 | 410.05 | 5.86 | 5.86 | 5.86 |
| $R^2$ | 0.19 | 0.19 | 0.19 | 0.34 | 0.34 | 0.34 |
| N | 1204 | 1204 | 1204 | 1210 | 1210 | 1210 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

## Table A5.6: Restricted Sample: Property Tax B—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.472 | | | 0.002** | | |
| | [0.112] | | | [0.042] | | |
| Poverty Gap | | 1.203* | | | 0.006*** | |
| | | [0.084] | | | [0.001] | |
| Median Gap | | | 0.701 | | | 0.003** |
| | | | [0.157] | | | [0.032] |
| Population (Log) | 14.842 | 13.704 | 11.177 | -0.532*** | -0.519*** | -0.539*** |
| | [0.769] | [0.787] | [0.824] | [0.000] | [0.000] | [0.000] |
| Share Foreigners | -0.530 | -0.504 | -0.487 | -0.001 | -0.001 | -0.001 |
| | [0.681] | [0.694] | [0.704] | [0.760] | [0.784] | [0.804] |
| Dependency Ratio | -0.314 | -0.317 | -0.334 | -0.003*** | -0.003*** | -0.003*** |
| | [0.447] | [0.441] | [0.417] | [0.006] | [0.006] | [0.005] |
| Mean Income | -0.003* | -0.003* | -0.003* | -0.000*** | -0.000*** | -0.000*** |
| | [0.054] | [0.067] | [0.054] | [0.001] | [0.005] | [0.001] |
| Radical Right | 5.197*** | 5.163*** | 5.248*** | 0.009** | 0.009** | 0.009** |
| | [0.000] | [0.001] | [0.000] | [0.022] | [0.027] | [0.021] |
| Radical Left | -1.174* | -1.181* | -1.216* | -0.006*** | -0.006*** | -0.006*** |
| | [0.092] | [0.092] | [0.084] | [0.001] | [0.002] | [0.001] |
| Income Tax Revenue (Log) | 43.289*** | 42.652*** | 43.567*** | 0.024 | 0.021 | 0.025 |
| | [0.005] | [0.006] | [0.004] | [0.551] | [0.606] | [0.532] |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 404.48 | 404.48 | 404.48 | 4.80 | 4.80 | 4.80 |
| $R^2$ | 0.46 | 0.47 | 0.46 | 0.82 | 0.83 | 0.82 |
| N | 1205 | 1205 | 1205 | 1211 | 1211 | 1211 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent; revenue variables are logged per capita.

## Table A5.7: Spending Categories: Poverty Rate—2SLS

| | (1) Welfare | (2) Schooling | (3) Public Education | (4) Culture | (5) Sport | (6) Health | (7) Local Police | (8) Fire Protection | (9) Roads | (10) Waste Disposal |
|---|---|---|---|---|---|---|---|---|---|---|
| Poverty Rate | -0.056** | -0.083** | 0.026 | -0.121 | -0.102* | -0.064 | 0.010 | 0.037 | -0.210 | 0.061 |
| | [0.042] | [0.034] | [0.572] | [0.286] | [0.065] | [0.399] | [0.452] | [0.242] | [0.151] | [0.593] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 5.42 | 5.26 | 3.11 | 3.73 | 4.44 | 2.75 | 4.00 | 3.78 | 4.52 | 4.13 |
| N | 1202 | 1202 | 1178 | 1194 | 1202 | 1165 | 1202 | 1202 | 1201 | 1169 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

## Table A5.8: Spending Categories: Poverty Gap—2SLS

| | (1) Welfare | (2) Schooling | (3) Public Education | (4) Culture | (5) Sport | (6) Health | (7) Local Police | (8) Fire Protection | (9) Roads | (10) Waste Disposal |
|---|---|---|---|---|---|---|---|---|---|---|
| Poverty Gap | -0.174** | -0.302** | 0.064 | -0.678* | -0.188* | -0.164 | 0.021 | 0.026 | -0.360 | 0.261 |
| | [0.028] | [0.030] | [0.612] | [0.066] | [0.053] | [0.376] | [0.472] | [0.659] | [0.116] | [0.376] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 5.42 | 5.26 | 3.11 | 3.73 | 4.44 | 2.75 | 4.00 | 3.78 | 4.52 | 4.13 |
| N | 1202 | 1202 | 1178 | 1194 | 1202 | 1165 | 1202 | 1202 | 1201 | 1169 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

## Table A5.9: Spending Categories: Median Gap—2SLS

| | (1) Welfare | (2) Schooling | (3) Public Education | (4) Culture | (5) Sport | (6) Health | (7) Local Police | (8) Fire Protection | (9) Roads | (10) Waste Disposal |
|---|---|---|---|---|---|---|---|---|---|---|
| Median Gap | -0.076*** | -0.141*** | 0.032 | -0.234 | -0.097** | -0.054 | 0.007 | 0.009 | -0.183* | 0.130 |
| | [0.001] | [0.001] | [0.593] | [0.110] | [0.036] | [0.430] | [0.639] | [0.749] | [0.075] | [0.259] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 5.42 | 5.26 | 3.11 | 3.73 | 4.44 | 2.75 | 4.00 | 3.78 | 4.52 | 4.13 |
| N | 1202 | 1202 | 1178 | 1194 | 1202 | 1165 | 1202 | 1202 | 1201 | 1169 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

## Table A5.10: Divided Government: Local Business Tax—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate × Unified | 3.067** | | | 0.038*** | | |
| | [0.023] | | | [0.008] | | |
| Poverty Rate × Divided | 2.373* | | | 0.042*** | | |
| | [0.066] | | | [0.001] | | |
| Poverty Gap × Unified | | 8.410*** | | | 0.092** | |
| | | [0.007] | | | [0.021] | |
| Poverty Gap × Divided | | 5.612** | | | 0.106*** | |
| | | [0.043] | | | [0.002] | |
| Median Gap × Unified | | | 2.429 | | | 0.044* |
| | | | [0.247] | | | [0.052] |
| Median Gap × Divided | | | 1.890 | | | 0.059*** |
| | | | [0.402] | | | [0.008] |
| Controls Interacted | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 415.00 | 415.00 | 415.00 | 6.03 | 6.03 | 6.03 |
| N | 2217 | 2217 | 2217 | 2225 | 2225 | 2225 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

## Table A5.11: Divided Government: Property Tax B—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate × Unified | 2.373 | | | 0.012*** | | |
| | [0.166] | | | [0.006] | | |
| Poverty Rate × Divided | 1.515 | | | 0.010** | | |
| | [0.343] | | | [0.020] | | |
| Poverty Gap × Unified | | 5.767 | | | 0.027** | |
| | | [0.197] | | | [0.014] | |
| Poverty Gap × Divided | | 1.606 | | | 0.016 | |
| | | [0.655] | | | [0.110] | |
| Median Gap × Unified | | | 2.932 | | | 0.016** |
| | | | [0.261] | | | [0.013] |
| Median Gap × Divided | | | 1.229 | | | 0.013** |
| | | | [0.642] | | | [0.043] |
| Controls Interacted | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 428.14 | 428.14 | 428.14 | 4.92 | 4.92 | 4.92 |
| N | 2218 | 2218 | 2218 | 2227 | 2227 | 2227 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

Table A5.12: Divided Government: Aggregate Spending—2SLS

| | (1) | (2) | (3) |
|---|---|---|---|
| Poverty Rate × Unified | -0.030 | | |
| | [0.121] | | |
| Poverty Rate × Divided | -0.026* | | |
| | [0.065] | | |
| Poverty Gap × Unified | | -0.194** | |
| | | [0.016] | |
| Poverty Gap × Divided | | -0.146** | |
| | | [0.025] | |
| Median Gap × Unified | | | -0.077*** |
| | | | [0.004] |
| Median Gap × Divided | | | -0.070*** |
| | | | [0.001] |
| Controls Interacted | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.91 | 6.91 | 6.91 |
| N | 1202 | 1202 | 1202 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent.

## Table A5.13: Lagged: Local Business Tax—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| L.Poverty Rate | 0.098 | | | 0.004 | | |
| | [0.536] | | | [0.235] | | |
| L.Poverty Gap | | 0.336 | | | 0.009 | |
| | | [0.410] | | | [0.296] | |
| L.Median Gap | | | 0.111 | | | 0.002 |
| | | | [0.656] | | | [0.665] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 415.90 | 415.90 | 415.90 | 6.05 | 6.05 | 6.05 |
| $R^2$ | 0.34 | 0.34 | 0.34 | 0.47 | 0.47 | 0.47 |
| N | 2016 | 2016 | 2016 | 2023 | 2023 | 2023 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent. The economic deprivation measures and the corresponding instrument are lagged by one year.

## Table A5.14: Lagged: Local Business Tax—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| L.Poverty Rate | 3.480** | | | 0.055** | | |
| | [0.020] | | | [0.025] | | |
| L.Poverty Gap | | 7.551** | | | 0.139*** | |
| | | [0.018] | | | [0.007] | |
| L.Median Gap | | | 4.523** | | | 0.075* |
| | | | [0.033] | | | [0.053] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 415.90 | 415.90 | 415.90 | 6.05 | 6.05 | 6.05 |
| N | 2015 | 2015 | 2015 | 2022 | 2022 | 2022 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent. The economic deprivation measures and the corresponding instrument are lagged by one year.

## Table A5.15: Lagged: Property Tax B—OLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| L.Poverty Rate | 0.310 | | | 0.001 | | |
| | [0.355] | | | [0.469] | | |
| L.Poverty Gap | | 0.069 | | | 0.000 | |
| | | [0.927] | | | [0.778] | |
| L.Median Gap | | | 0.518 | | | 0.000 |
| | | | [0.304] | | | [0.762] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 432.32 | 432.32 | 432.32 | 4.95 | 4.95 | 4.95 |
| $R^2$ | 0.57 | 0.57 | 0.57 | 0.84 | 0.84 | 0.84 |
| N | 2016 | 2016 | 2016 | 2025 | 2025 | 2025 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent. The economic deprivation measures and the corresponding instrument are lagged by one year.

## Table A5.16: Lagged: Property Tax B—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| L.Poverty Rate | 3.020 | | | 0.001 | | |
| | [0.226] | | | [0.855] | | |
| L.Poverty Gap | | 5.639 | | | -0.009 | |
| | | [0.262] | | | [0.468] | |
| L.Median Gap | | | 1.584 | | | -0.009 |
| | | | [0.675] | | | [0.353] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 432.32 | 432.32 | 432.32 | 4.95 | 4.95 | 4.95 |
| N | 2015 | 2015 | 2015 | 2024 | 2024 | 2024 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent. The economic deprivation measures and the corresponding instrument are lagged by one year.

Table A5.17: Lagged: Aggregate Spending—OLS

| | (1) | (2) | (3) |
|---|---|---|---|
| L.Poverty Rate | -0.001 | | |
| | [0.605] | | |
| L.Poverty Gap | | 0.000 | |
| | | [1.000] | |
| L.Median Gap | | | 0.000 |
| | | | [0.947] |
| Demographic Controls | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.92 | 6.92 | 6.92 |
| $R^2$ | 0.10 | 0.10 | 0.10 |
| N | 1010 | 1010 | 1010 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent. The economic deprivation measures and the corresponding instrument are lagged by one year.

## Table A5.18: Lagged: Aggregate Spending—2SLS

| | (1) | (2) | (3) |
|---|---|---|---|
| L.Poverty Rate | -0.018 | | |
| | [0.347] | | |
| L.Poverty Gap | | -0.108* | |
| | | [0.087] | |
| L.Median Gap | | | -0.049* |
| | | | [0.052] |
| Demographic Controls | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.92 | 6.92 | 6.92 |
| N | 1009 | 1009 | 1009 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent. The economic deprivation measures and the corresponding instrument are lagged by one year.

## Table A5.19: Winsorized 1%-Level: Local Business Tax—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 3.840** | | | 0.053** | | |
| | [0.037] | | | [0.025] | | |
| Poverty Gap | | 5.763** | | | 0.122*** | |
| | | [0.040] | | | [0.006] | |
| Median Gap | | | 3.151 | | | 0.059** |
| | | | [0.103] | | | [0.047] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 414.95 | 414.95 | 414.95 | 6.03 | 6.03 | 6.03 |
| N | 2214 | 2214 | 2214 | 2222 | 2222 | 2222 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent and are winsorized at the one percent-level.

## Table A5.20: Winsorized 5%-Level: Local Business Tax—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 4.739** | | | 0.065** | | |
| | [0.042] | | | [0.032] | | |
| Poverty Gap | | 6.811** | | | 0.144*** | |
| | | [0.043] | | | [0.009] | |
| Median Gap | | | 4.083 | | | 0.077* |
| | | | [0.113] | | | [0.064] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 414.95 | 414.95 | 414.95 | 6.03 | 6.03 | 6.03 |
| N | 2214 | 2214 | 2214 | 2222 | 2222 | 2222 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent and are winsorized at the five percent-level.

## Table A5.21: Winsorized 1%-Level: Property Tax B—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 0.914 | | | 0.003 | | |
| | [0.740] | | | [0.638] | | |
| Poverty Gap | | 1.529 | | | -0.000 | |
| | | [0.726] | | | [0.980] | |
| Median Gap | | | -1.453 | | | -0.003 |
| | | | [0.637] | | | [0.722] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 428.12 | 428.12 | 428.12 | 4.92 | 4.92 | 4.92 |
| N | 2215 | 2215 | 2215 | 2224 | 2224 | 2224 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent and winsorized at the one percent-level.

## Table A5.22: Winsorized 5%-Level: Property Tax B—2SLS

| | Tax Rate | | | Tax Revenue | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty Rate | 1.124 | | | 0.004 | | |
| | [0.740] | | | [0.638] | | |
| Poverty Gap | | 1.803 | | | -0.000 | |
| | | [0.725] | | | [0.981] | |
| Median Gap | | | -1.876 | | | -0.003 |
| | | | [0.636] | | | [0.721] |
| Demographic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Variable | 428.12 | 428.12 | 428.12 | 4.92 | 4.92 | 4.92 |
| N | 2215 | 2215 | 2215 | 2224 | 2224 | 2224 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent and winsorized at the five percent-level.

## Table A5.23: Winsorized 1%-Level: Aggregate Spending—2SLS

| | (1) | (2) | (3) |
|---|---|---|---|
| Poverty Rate | -0.025 | | |
| | [0.140] | | |
| Poverty Gap | | -0.137** | |
| | | [0.033] | |
| Median Gap | | | -0.059*** |
| | | | [0.006] |
| Demographic Controls | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.91 | 6.91 | 6.91 |
| N | 1202 | 1202 | 1202 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent and winsorized at the one percent-level.

## Table A5.24: Winsorized 5%-Level: Aggregate Spending—2SLS

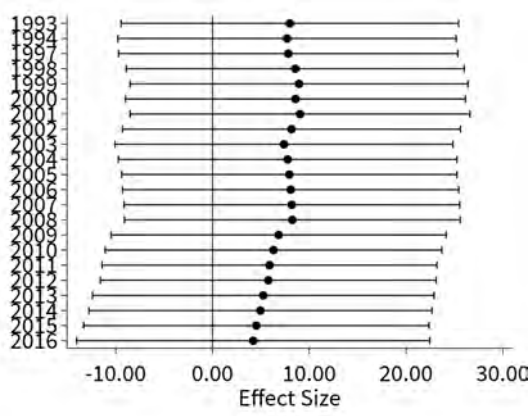
| | (1) | (2) | (3) |
|---|---|---|---|
| Poverty Rate | -0.035 | | |
| | [0.166] | | |
| Poverty Gap | | -0.177** | |
| | | [0.038] | |
| Median Gap | | | -0.085** |
| | | | [0.015] |
| Demographic Controls | Yes | Yes | Yes |
| Economic Controls | Yes | Yes | Yes |
| Political Polarization | Yes | Yes | Yes |
| Income Tax | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Mean Dep. Variable | 6.91 | 6.91 | 6.91 |
| N | 1202 | 1202 | 1202 |

*Notes:* p-values in brackets; * p < 0.1, ** p < 0.05, *** p < 0.01; standard errors are clustered at the city district-level. The poverty rate, the poverty gap and the median gap are in percent and winsorized at the five percent-level.

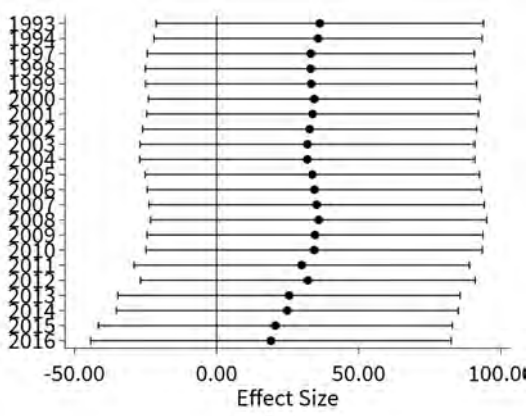
## Additional Figures

### Figure A5.1: Bartik Instrument Test: Poverty Gap

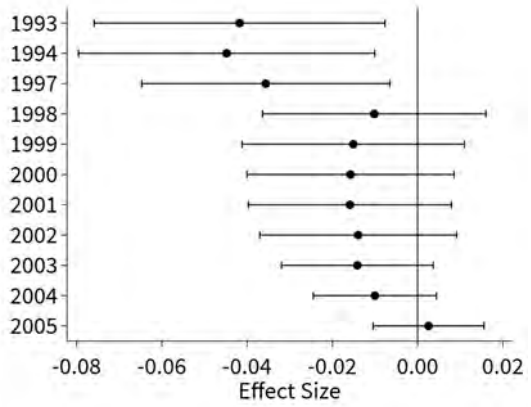
(a) Business Tax Rate




(b) Property Tax Rate B




(c) Aggregate Spending




*Notes:* This figure plots the results of the Bartik-instrument test for the poverty gap. Circles represent point estimates, black lines represent 90 percent confidence intervals.

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