RUHR
ECONOMIC PAPERS

Lukas Hörnig

Regional Employment Effects of the Hartz-Reforms

RUB RWI #1033

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Editors

Prof. Dr. Thomas K. Bauer
RUB, Department of Economics, Empirical Economics
Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de

Prof. Dr. Ludger Linnemann
Technische Universität Dortmund, Department of Business and Economics
Economics – Applied Economics
Phone: +49 (0) 231/7 55-3102, e-mail: : Ludger.Linnemann@tu-dortmund.de

Prof. Dr. Volker Clausen
University of Duisburg-Essen, Department of Economics
International Economics
Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de

Prof. Dr. Ronald Bachmann, Prof. Dr. Manuel Frondel, Prof. Dr. Torsten Schmidt, Prof. Dr. Ansgar Wübker
RWI, Phone: +49 (0) 201/81 49-213, e-mail: presse@rwi-essen.de

Editorial Office

Sabine Weiler
RWI, Phone: +49 (0) 201/81 49-213, e-mail: sabine.weiler@rwi-essen.de

Ruhr Economic Papers #1033

Responsible Editor: Thomas K. Bauer

ISSN 1864-4872 (online) – ISBN 978-3-96973-202-1

Ruhr Economic Papers #1033

Lukas Hörnig

Regional Employment Effects of the Hartz-Reforms

Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at **http://dnb.dnb.de**

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

http://dx.doi.org/10.4419/96973202
ISSN 1864-4872 (online)
ISBN 978-3-96973-202-1

Lukas Hörnig[1]

Regional Employment Effects of the Hartz-Reforms

Abstract

Between 2003 and 2005, the German government passed an unprecedented package of labor market reforms, commonly known as the Hartz-reforms. This led to a "labor market miracle" with sharply declining unemployment rates. This paper examines these reforms at the regional level and provides a comprehensive picture of whether the reforms have exacerbated or reduced regional disparities. I apply a regional difference-in-differences framework commonly used in the minimum wage evaluation literature to analyze the effect of the reforms on employment at the county level. The empirical results show that while all counties benefited from the Hartz-reforms, more prosperous counties derived a stronger benefit than those with high unemployment rates. The evidence is stronger for West Germany than for East Germany. Overall, the reforms have not improved economic performance homogeneously, but have actually increased regional disparities.

JEL-Codes: R11, J48, O47

Keywords: Regional growth; policy evaluation; regional convergence; Hartz-reforms

August 2023

1 Lukas Hörnig, RWI and RUB. – I am grateful to participants of the 10th European Meeting of the UEA, the 13th Workshop on Labor Economics IAAEU and the 15th BiGSEM Workshop on Economics and Management for helpful comments and suggestions. I also thank Thomas Bauer and Sandra Schaffner for their support. All remaining errors are mine. – All correspondence to: Lukas Hörnig, RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, e-mail: lukas.hoernig@rwi-essen.de

1 Introduction

While some praise Germany's rise from "sick man of Europe" to "world export champion" as a "labor market miracle", others bemoan social cutbacks and an ever-widening gap between rich and poor in Germany. Both views are closely linked to the "Modern Services in the Labor Market" laws, better known as the Hartz-reforms, named after the head of the commission that drafted them. These consisted of various deregulations regarding (marginal) employment to stimulate labor demand, as well as cuts in unemployment benefits and eligibility periods. In addition, job-seeker guidance was improved in order to increase the quality of job matches and reduce unemployment spells. The most prominent element of the reform was the abolition of unemployment assistance, which guaranteed the long-term unemployed a transfer of 50 % of their last net income for an unlimited eligibility period. After the reforms, the long-term unemployed received a flat-rate transfer of €345 in West Germany and €331 in East Germany. More than fifteen years after the last part of the reforms came into force, they are still part of the public debate.

This paper analyzes whether the Hartz-reforms have promoted employment at the county level, an explicit goal of the reforms. A considerable literature focuses on the effects of the reforms on unemployment. These studies show that the reforms reduced unemployment, although the estimated magnitude of the effect varies between 2.8 and less than 0.1 percentage points (e.g. Krause and Uhlig, 2012; Launov and Wälde, 2013, 2016). Since the goal of the reform was to get the unemployed into work and not to push them out of the labor force, I expect a positive effect on employment. However, it remains unclear how the reforms affected the regional distribution of employment growth across Germany.

To evaluate the effect of the Hartz-reforms on regional employment growth, I follow an approach commonly used in the minimum wage literature and pioneered by Card (1992). In the context of a national minimum wage introduced at the same time, there is no natural control group to estimate the counterfactual. However, Card recognized that there is still regional variation in treatment intensity, *i.e.*, the proportion of workers earning less than the minimum wage. He calls this treatment intensity the *bite* of the minimum wage. Regional variation in the bite can then be exploited by estimating a difference-in-differences (DiD) in treatment intensity. Applications of this methodology outside of minimum wage evaluation are still scarce.

To apply this methodological framework to the Hartz-reforms, I argue that the pre-reform unemployment rate of the counties captures the extent to which they were affected by the reforms[1], since the reforms were designed to push the unemployed into employment. In their interaction with the unemployed, Federal Employment Agency clerks should establish the principle of "challenge and promotion" (*Fördern und Fordern*). This is achieved by improving the client-clerk ratio in the Federal Employment Agency and improving the effectiveness of

[1]The analysis is repeated at the local labor market level and yields similar results.

the active labor market measures (Jacobi and Kluve, 2007). In addition, social benefits are less focussed on status protection and more on providing the minimum standard of subsistence. This leads to a reduction in the average transfer benefits for former recipients of the unemployment assistance for the long-term unemployed and to a shortened entitlement period in the short-term unemployment benefit scheme (Bofinger et al., 2005).[2] In addition, the use of sanctions against the refusal of job and training offers is expanded.

While these measures are tailored to activate the unemployed, one could argue that the unemployment rate does not perfectly capture the bite of the reforms, as the reforms also target labor demand through labor market deregulation, and employed people are also (indirectly) affected by the reforms through a potential loss of insurance following the Hartz-reforms. Thus, employees may become more attached to their jobs in response to the reforms. All in all, I argue that it is indeed the unemployed who are most affected, and that the unemployment rate is therefore a good proxy for the affectedness of the counties. It can even be argued that it is only a subset of the unemployed who are most affected: Both Goebel and Richter (2007) and Krebs and Scheffel (2013) show that it is the long-term unemployed who have lost the most from the Hartz-reforms.

There is an extensive literature evaluating the effects of the Hartz-reforms, both for individual components of the reforms and for the reform package as a whole. This literature consists of two types of studies. Structural macroeconomic papers, which explicitly model particular facets of the Hartz-reforms (e.g. Hartung et al., 2018; Krause and Uhlig, 2012; Krebs and Scheffel, 2013; Launov and Wälde, 2013, 2016), find overall that the Hartz III and IV laws reduced unemployment, but differ in the magnitude of the effect and provide mixed evidence on the evolution of wages. Reduced-form approaches exploiting discontinuities or structural breaks (e.g. Fahr and Sunde, 2009; Klinger and Rothe, 2012; Hertweck and Sigrist, 2013; Price, 2016; Tazhitdinova, 2020) find a small overall decrease in unemployment due to the Hartz-reforms. However, both strands of the literature still lack an investigation of the regional effects at a small scale. The only exception, to my knowledge, is the paper by Hillmann (2009), which shows an increased matching efficiency after the Hartz-reforms at the level of local employment agencies. This regional perspective is important for two reasons: First, the effect of the reforms found in previous studies may not be valid for all German regions. Therefore, I analyze whether unemployed people are more likely to leave unemployment in prosperous or economically weak regions. Second, the regional level is interesting in its own right, as there is growing concern not only about inequality between individuals, but also between regions.

This study contributes to the literature in three ways. First, I apply a common method from the minimum wage evaluation literature to evaluate the Hartz reforms. To my knowledge, I am the first to measure the bite of a nationwide labor market reform aimed at activating the unemployed using regional unemployment rates. This approach is an extension of the

[2]Former recipients of the social assistance, however, received slightly higher transfers (Bofinger et al., 2005).

econometric toolkit, as it allows drawing conclusions in an environment where there is no other feasible reduced form approach due to a missing control group. In addition, it requires relatively few theoretical assumptions compared to structural approaches. Second, the literature on the determinants of regional disparities focuses, *inter alia*, on differences in the nature of and access to markets (Gallup et al., 1999; Diamond and Renfrew, 1997; Bloom et al., 1998; Redding and Sturm, 2008; Davis and Weinstein, 2003), agglomeration externalities (Eeckhout et al., 2014; Gaubert, 2018), and migration (Granato et al., 2015; Niebuhr et al., 2012), but rarely examines the regional effects of a nationwide policy shock. Finally, the regional perspective improves our understanding of the Hartz reforms by allowing us not only to estimate the nationwide effect of the reforms, but also to localize where the reforms actually increased employment.

The remainder of this paper is as follows. Section 2 introduces the legal framework of the German welfare regime before and after the Hartz-reforms. Section 3 describes the data and Section 4 the empirical strategy. Section 5 presents the main results and Section 6 concludes.

2 Institutional Background

Before the Hartz reforms, the German welfare state aimed at protecting status rather than providing a minimum standard of living (Jacobi and Kluve, 2007) and was described as a "frozen welfare state" resistant to reform (Manow and Seils, 2000). However, the Hartz reforms that came into force between 2003 and 2005 represent a paradigm shift towards the provision of a minimum standard of subsistence (Eichhorst et al., 2010).

2.1 Old System of Unemployment Insurance and Social Assistance

The old system of unemployment insurance and social assistance consisted of three support programs, *i.e.* the unemployment insurance benefit (UB), unemployment assistance (UA), and social assistance (SA). Workers were entitled to UB if they had been employed in a job subject to social insurance for at least one year. The UB transfer amounted to 67 % of the last net income (60 % if there were no children living in the household).[3] The Federal Employment Office (FEB, *Bundesanstalt für Arbeit*) administered eligibility and the provision of active labor market measures. Eligibility for UB expired after 32 months and was replaced by eligibility for UA. However, eligibility for UA was conditional on need. The eligibility period was unlimited and was also administered by the FEB, which provided similar active labour market measures as for UB recipients. While UB was financed by contributions from workers and employers, UA was financed by taxes. It covered 57 % (53 % if there were no children in the household) of last net earnings. The last element, the SA, was a provision of a minimum standard of living for anyone who could not rely on income, public or family transfers. Thus, it represented the safety net for people who did not have enough work experience to qualify for the two unemployment transfer

[3]UB was limited to a maximum of €4,250 per month.

schemes or whose transfers were insufficient. Another difference from the UB and the UA is that it was financed and administered by the municipalities and not by the FEB. (Eichhorst et al., 2010; Jacobi and Kluve, 2007)

At the time of the Hartz reforms, Germany was considered the "sick man of Europe" with an unemployment rate of 8.8 %. This was higher than the EU-average[4] of 7.7 %, and higher than all of its neighbors except Poland (Eurostat, 2020). One reason for Germany's poor performance was considered to be its welfare system, for example the unlimited eligibility period of UA (Siebert, 1997). Nickell et al. (2005) and Caliendo et al. (2013) show that longer eligibility periods lead to longer unemployment spells. This is *inter alia* due to reduced search efforts (Krueger and Mueller, 2010) and a lower willingness to accept job offers (Lalive, 2007; Lalive et al., 2006) in response to longer eligibility periods.[5] Incentives to work were further weakened by the high marginal tax rates on the additional earned income of benefit recipients (Jacobi and Kluve, 2007). In addition, labor demand was weakened by rising labor costs due to employers' social security contributions, which partly finance the German welfare system (Streeck and Trampusch, 2005). In contrast, Bauer and Riphahn (2002) find that the employment effects of such costs are minimal.

2.2 Hartz-Reforms

The Hartz reforms were divided into four laws that came into force at the beginning of the years 2003 to 2005. The reforms aimed to reduce the duration of unemployment by establishing the principle of "challenge and promotion" (*Fördern und Fordern*). This was achieved by improving the client-clerk ratio in the federal employment agency and by improving active labor market measures. In addition, the UA was revised so that it no longer provides status maintenance, but rather guarantees a minimum subsistence level. The reform also targeted labor demand by deregulating aspects of the labor market, such as restrictions on marginal employment. (Jacobi and Kluve, 2007)

The first two Hartz laws took effect on January 1, 2003. Hartz I deregulated temporary work.[6] Hartz II contained further subsidies for marginal employment. The most important change was the increase of the maximum income for mini-jobs, a special form of marginal employment, according to which one can work without paying income tax and social security contributions as long as one does not exceed a certain income threshold. This threshold was raised from 325 € to 400 € per month (Krebs and Scheffel, 2013). Another new form of employment were the midi-jobs, in which an income of up to 800 € could be earned, which

[4] As of its member states in 2004, *i.e.* Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden, and the United Kingdom.

[5] In contrast, Schmieder et al. (2012) finds only modest effects of longer eligibility periods on unemployment spells for Germany between 1987 and 2004.

[6] The regulations that were repealed concerned reemployment, fixed-term contracts, and the maximum duration of employment for temporary workers. At the same time, a new regulation was introduced, according to which temporary workers must be paid and treated in the same way as permanent workers if there is no deviating collective agreement for the temporary workers.

was subject to a linear tax. In addition, the introduction of vouchers for professional training courses and increased sanctions for quitting a job, refusing acceptable job offers, and leaving training programs or temporary jobs were aimed directly at the unemployed as part of the "challenge" principle (Jacobi and Kluve, 2007; Launov and Wälde, 2016).

The third reform package, Hartz III, which followed in 2004, reorganized the FEB and renamed it the Federal Employment Agency (FEA). Most importantly, the amount of counseling time per unemployed person was increased by assigning each unemployed person to a caseworker and ranking the cases according to importance (Krebs and Scheffel, 2013; Launov and Wälde, 2016). The reorganization was further intensified by allowing the FEA to outsource placement services to private agencies and allowing clients to choose private providers at the FEA's expense. Competition between private and public providers was meant to improve the quality of services (Jacobi and Kluve, 2007; Krebs and Scheffel, 2013).

Hartz IV came into force on January 1, 2005 as the last step of the Hartz reforms and completed the paradigm shift in the welfare regime from status maintenance to basic income support with strong activation elements (Eichhorst et al., 2010). It created the new flat-rate unemployment benefit II, which replaced the two previous transfer schemes for the long-term unemployed, *i.e.* UA and SA. The standard rate during its introduction was 345 € in West Germany and 331 € in East Germany per month, which was lower than the average of the old UA (Arntz et al., 2007). Since the second year of its introduction, the level of unemployment benefit II has been the same in East and West Germany. All persons over the age of 15 who are able to work and are in need of assistance are eligible.[7] The old UB, which was aimed at the short-term unemployed, was replaced by the similar Unemployment Benefit I. This reform package mainly affected the unemployed, as it changed the transfer system, but the employed also experienced a loss due to the lower insurance effect of the new UB II (Krebs and Scheffel, 2013). However, it is the long-term unemployed who lost the most from this part of the reform (Goebel and Richter, 2007; Krebs and Scheffel, 2013).

Overall, the unemployment rate captures the bite of the reforms well within the county, because the Hartz reforms targeted the unemployed by improving job search assistance and simultaneously reducing the reservation wage for many unemployed with increased sanctions and (at least for most recipients) reduced transfer payments. With regard to the last reform package, it is the subgroup of the long-term unemployed that is particularly affected, since former UB recipients lost the most with the transition to UB II (Goebel and Richter, 2007; Krebs and Scheffel, 2013). Therefore, I repeat the analysis using the share of long-term unemployed as an alternative definition of the reform's bite within the counties, which leads to qualitatively robust results.

[7] If the beneficiary forms a needs-based community with others, these others (e.g. partners, parents or children) can also receive benefits (Arntz et al., 2007). In addition, increased needs can be registered in the case of pregnancy, single parenthood and severe disability (§21 SGB II).

3 Data

Data for labor market outcomes at the county level were collected by the Federal Employment Agency and distributed by the Federal and State Statistical Offices (2020). The time series covers the period from 1990 to 2018, although data for East Germany are only available from 1996 onwards. As several county reforms took place during this period, especially in East Germany, the data were adjusted to the current county boundaries using the conversion factors of the BBSR (unpublished). I restrict the analysis to the period from 1996 to 2008, which is the longest complete period available without the confounding effects of the financial crisis. Adding covariates further restricts the period to 2000-2008, as the sectoral employment shares do not cover the period before 2000.

Figure 1 shows the regional distribution of the county level unemployment rate in 2002, *i.e.* the bite of the reform.[8] The 2002 level is chosen because it is the last period before the reforms took effect and thus best measures the regional bite without confounding effects of the reform, assuming there are no anticipation effects. The average unemployment rate for all counties is 10.4 %, with a large regional variation. The county values range from 3.9 % to 24.44 %, with the most pronounced differences between West and East Germany. Even 12 years after reunification, the inner-German border can still be seen in the higher unemployment rates in East Germany. However, there are also differences within West and East Germany. Relatively higher unemployment rates are found in the west and north of West Germany, *i.e.* the Ruhr area and the North Sea coastal region, while lower levels and variations are found in the economically prosperous south of Germany.

[8] As the institutional setting also suggests that the long-term unemployment rate is the better measure of the bite of the reforms, Figure A.1 shows its regional distribution. One could further argue that the better geographical level is the local labor market level. Therefore, the presentation of the unemployment rate at this level is repeated in Figure A.2. The patterns are similar in both figures, especially the persistent East-West difference.

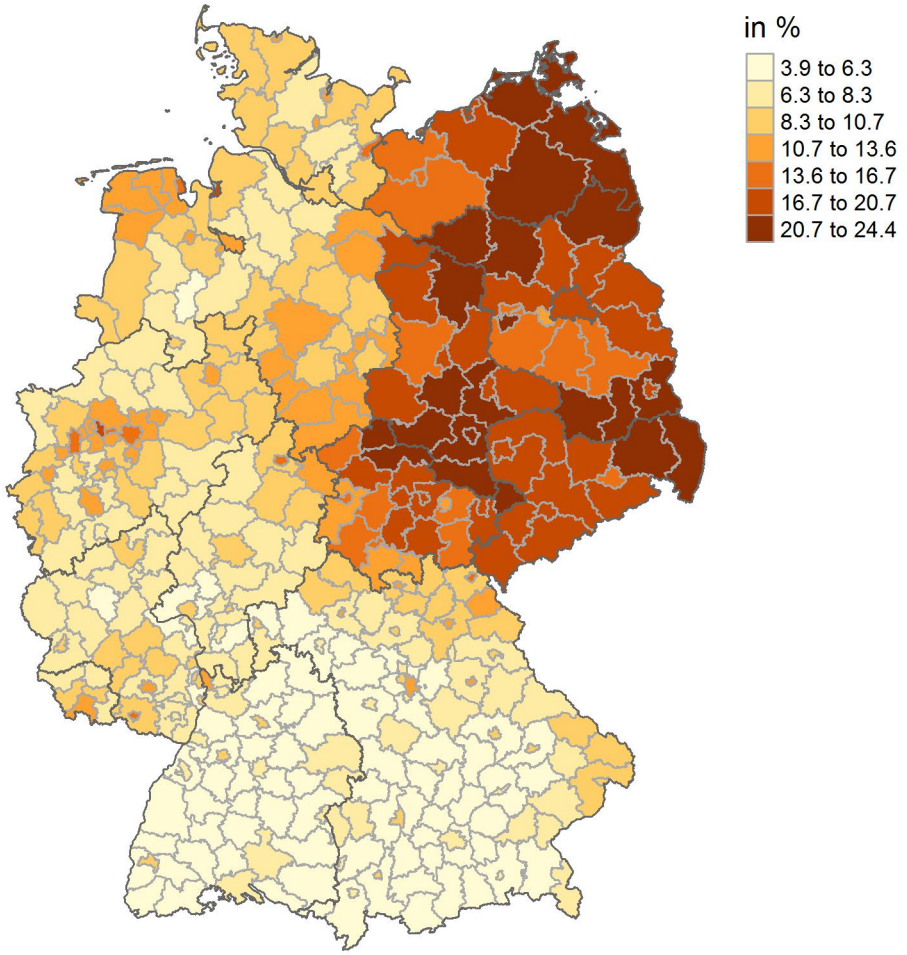


Figure 1: Unemployment Rate at County Level (© GeoBasis-DE / BKG 2020)

Figure 2 shows the average employment growth rates over the counties. The shaded area indicates the range of $\pm$ one standard deviation around the average growth. Employment growth follows an undulating pattern over the period examined here. Starting from negative employment growth rates in 1997, they increased until 2000. After the bursting of the dotcom bubble, growth rates began to decline and reached their minimum in 2003, when the Hartz reforms were first implemented. Since then, employment growth rates have increased steadily, reaching positive employment growth rates in 2006.[9]

[9]More summary statistics, including covariates, are reported in Table A.1.

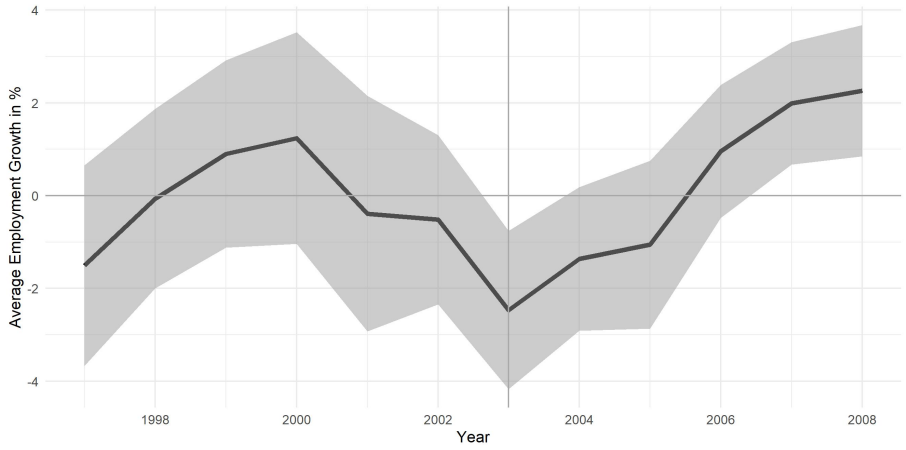


Figure 2: Average Employment Growth at County Level ± one Standard Deviation

4 Empirical Strategy

Since the Hartz reforms are implemented at the national level, there is no legislative variation that would allow the identification of a reduced-form estimate. However, there is regional variation in the extent to which regions were affected by the reforms. Taking advantage of this, I follow the seminal work of Card (1992), who analyzes an increase in the federal minimum wage in the U.S.[10] Card exploits regional differences in local labor market characteristics to generate the intensity with which the minimum wage increase affects local labor markets, *i.e.* how much wages have to adjust after the reforms. He calls this intensity of treatment the "bite" of the reforms, which varies regionally.

Adopted to the setting examined here, the treatment intensity, i.e. the bite of the Hartz reforms, is the share of people in the counties affected by the reform. Although the labor market reform did not only affect the unemployed, as it simultaneously stimulated the demand for labor and also affected employed workers (*e.g.* through deregulations in marginal employment), it is the unemployed who are most affected. This is underlined by the slogan "challenge and promotion", which directly targeted the unemployed in order to reduce the high unemployment rate in Germany.

To apply this framework to the analysis of the impact of the Hartz reforms on employment at the county level, I use a DiD of treatment intensity that captures the bite of the reforms. I run OLS estimations on variants of the following baseline specification

$$logE_{it} = \alpha + \beta(post_t \times bite_i) + \theta X_{it} + \delta_i + \pi_t + \epsilon_{it} \tag{1}$$

where the dependent variable $logE_{it}$ is the log of the number of employees in county i at time

[10]There is a large literature evaluating minimum wage policy around the world following the approach of Card (1992). For exemplary applications in Germany see Caliendo et al. (2018); Ahlfeldt et al. (2018), in the US see Dube et al. (2010); Burkhauser et al. (2000), and in the UK see Dolton et al. (2010); Stewart (2002).

t[11], $post_t$ is a dummy for observations after the start of the reforms in 2003, and $bite_t$ is the respective unemployment rate in percent in 2002 in county i. In addition, county and year fixed effects are included, as well as covariates, namely mean county population, age[12], GDP per capita, and employment shares in agriculture, production, manufacturing, construction, trade, and finance (with the public sector as the reference group) at the county level to control for local demographics, wealth, and economic structure. Finally, ϵ_{it} is the error term.

In order to identify the causal effect of the Hartz reforms on employment using a DiD identification strategy, employment trends in the absence of the reforms must be the same. Whether this is true cannot be tested because the counterfactual employment development in the absence of the reforms is not observable. However, following Card and Krueger (2000), it is common practice to show the time series of the dependent variable before the introduction of the reforms. Since the bite measure is continuous, I compute means by bite intensity groups for better visibility. I define the low (high) bite group as counties with an unemployment rate below (above) the median bite.

Figure 3 compares the evolution of average log employment in these bite groups from 1996 to 2008 separately for West and East Germany. This shows different patterns for the two regions[13]. Panel A shows the development for West Germany. Both groups follow a wave-like pattern with increasing log employment levels until shortly before the reforms. Levels decline until 2005, when the last reform package came into effect. From then on, employment levels rise again in both bite groups. Panel B shows a different development in East Germany before the reforms. There is a persistent decline in employment levels in both bite groups until 2005. Overall, the rather parallel trends before 2003 support the validity of the identification strategy, while the rather similar developments after the reforms suggest that the Hartz reforms did not affect the counties very differently.[14]

[11] I repeat the analysis using the employment rate as the outcome, see Tables A.3, which yields qualitatively similar results for the interaction term.

[12] Mean county age is added to the regressions both linearly and squared. This allows for a more flexible relationship between county demographics and employment growth. This is important because the age groups at both ends of the age distribution are not part of the labor force.

[13] Different trends in the evolution of log employment are not surprising, as there are persistent differences in economic and demographic characteristics between West and East Germany (see Table A.1). Therefore, different effects of the Hartz reforms are to be expected and the analysis is performed separately for West and East Germany.

[14] Figure A.4 replicates this graph analogously with the bite (long) measure, confirming the parallel movement before the reforms, but suggesting larger treatment effects in East Germany.

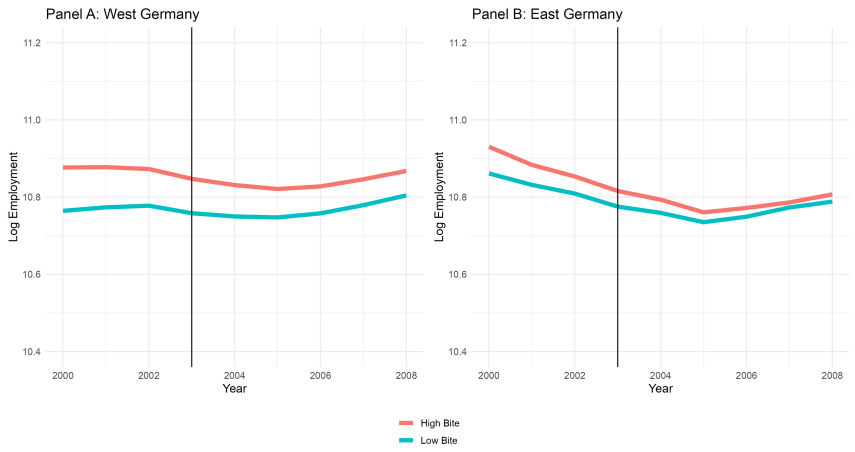


Figure 3: Average Log Employment by Bite below/above Median by Region

Bias can arise from the geographic level of analysis. If there are spatial spillovers, for example, if people in a high-bite county respond to the reform by moving to a low-bite county, the coefficients could be biased downward. To address such concerns, the analysis is repeated at the local labor market level. Here, the units of observation are more independent. I follow the definition of the local labor market in Breidenbach et al. (2018), which relies mainly on commuting flows. Suggestive evidence for the validity of the common trends assumption at the local labor market level is provided by Figure A.3, which mirrors the county level figures. The regression results are also similar at the different geographical levels.

Another possible source of biased inference is the problem of serial correlation, which can arise in DiD settings due to the persistence of treatment variables within groups and over time, or when long time series of dependent variables are used (Bertrand et al., 2004). This may be a concern here, as I am studying a period of over a decade. To avoid this problem, Bertrand et al. (2004) suggests clustering the standard errors at the regional level of interest. Therefore, I cluster the standard errors in all regressions at the local labor market level.

I repeat the analysis using a novel empirical strategy that is more data-driven with respect to the common trend requirement. I apply the synthetic difference-in-differences design recently developed by Arkhangelsky et al. (2021). This method combines the standard DiD design and the idea behind synthetic control methods, *i.e.* synthetic DiD applies unit and time fixed effects as in the standard DiD and weights observations to match pre-treatment outcome trends similar to synthetic control methods. The unit weights force the average outcome of the treated units to be approximately parallel to the weighted average of the control units. Furthermore, the synthetic DiD assigns not only *unit* weights but also *time* weights. These time weights are designed so that the average post-treatment outcome for each of the control units differs by a constant from the weighted average of the pre-treatment outcomes for the same control units.

5 Results

Table 1 shows the regression results from different model specifications, where the sample is divided into West German and East German subsamples in columns (1) to (5) and (6) to (10), respectively. Columns (1) and (6) show the baseline specification without any control variables that are added subsequently. First, county and year fixed effects are included in columns (2) and (7). The inclusion of these fixed effects leads to the omission of the uninteracted *bite* and *post* dummies due to multicollinearity. Including the control variables reduces the sample because they are not available before 2000. Therefore, I first repeat the same specification as in columns (2) and (7) with the restricted sample in columns (3) and (8). Columns (4) and (9) show the full specification including the two-way fixed effects and the covariates. Finally, in columns (5) and (10), the year fixed effects are replaced by a trend variable.

The after-coefficients of the baseline specifications in columns (1) and (6) for West and East Germany imply that average employment increases by 7.6 % in West Germany and 9.1 % in East Germany after the Hartz reforms take effect. To see how the reform affects regional employment through its bite, I focus on the interaction of the bite with the after dummy. The coefficient of this interaction term is negative and statistically different from zero in all but the last two specifications for the East German subsample. Here, the coefficient remains negative but is not significant at conventional levels of significance. Quantitatively, a one standard deviation increase (2.65 in West Germany, 3.14 in East Germany) in *bite* reduces employment in West Germany by between 0.8 and 2.65 %, based on the lowest and highest point estimates. In East Germany this effect ranges from -0.13 to -3.77 %. The regression specifications that include a trend instead of the year fixed effects (columns 5 and 10) yield the smallest effect size in magnitude. This may be because regional trends could absorb much of the variation in employment. Moreover, only the West German sample produces statistically significant coefficients in this specification.

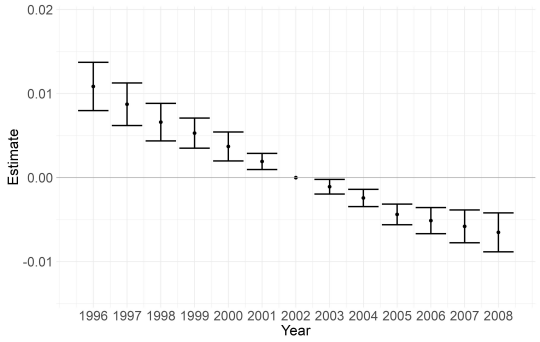
To summarize, while average employment increases by 7.6 % in West Germany and 9.1 % in East Germany after the Hartz-reforms came into force, the effect of *bite* works in the opposite direction. Based on the coefficients of Table 1 columns (4) and (9) (*i.e.*, where the full set of controls is included), it follows that the negative *bite* effect outweighs the positive average effect when the *bite* is greater than 25.3 % in West and 45.5 % in East Germany c.p.[15] This is not the case in any of the counties – in either West or in East Germany. Thus, while the Hartz-reforms did boost employment overall, employment increased most in counties, that were already prosperous.

[15] In West Germany: $0.076 + bite \times (-0.003) < 0$.
In East Germany: $0.091 + bite \times (-0.002) < 0$.

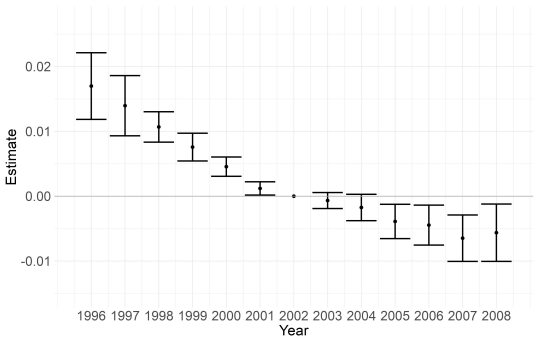
	Dependent variable: Log Number Employed									
	West Germany					East Germany				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.076***				−0.006	0.091***				−0.033
	(0.011)				(0.006)	(0.032)				(0.025)
Bite	0.024					0.033*				
	(0.018)					(0.019)				
After x Bite	−0.010***	−0.010***	−0.006***	−0.003***	−0.003***	−0.012***	−0.012***	−0.006***	−0.002*	−0.0004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
County FE	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	no	no	yes	yes	yes	no
Trend	no	no	no	no	yes	no	no	no	no	yes
Controls	no	no	no	yes	yes	no	no	no	yes	yes
Time	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008
Average Bite [S.D.]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]
Observations	4,212	4,212	2,916	2,916	2,916	1,001	1,001	693	693	693

Notes: The table reports estimates from the DiD in continuous treatment regressions on the county level separately for the West and East German subsamples in columns (1) to (5) and (6) to (10), respectively. The dependent variable in all columns is the log number employed. Covariates are added stepwise by column as indicated. In columns (5) and (10) year fixed effects are replaced with a linear time trend. *Bite* is the unemployment share in 2002. *post* is a dummy that takes the value 1 after 2003, when the reform starts, and 0 otherwise. Covariates include GDP, employment shares of sectors (agriculture, production, manufacturing, construction, trade, finance), mean age (linearly and squared), and population. Standard errors in brackets are clustered at the local labor market region. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

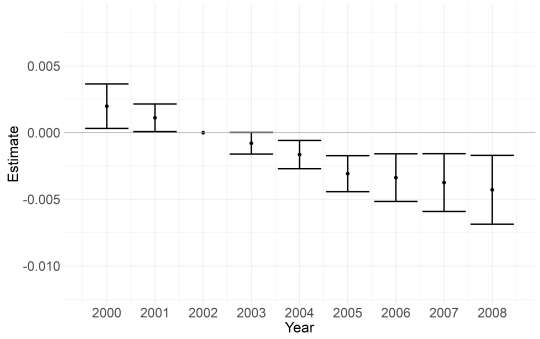
Table 1: Effect of the Reform on the Log Number Employed at County Level



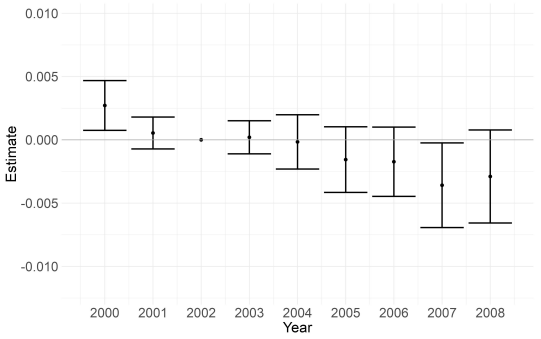
Panel A.1: West Germany without Controls



Panel A.2 East Germany without Controls



Panel B.1: West Germany with Controls



Panel B.2 East Germany with Controls

Notes: Dependent variable is the log number employed. Unit of analysis: counties. Estimation methods: TWFE regressions without and with covariates in Panel A and B, respectively. Sample period: 1996–2008 (covariates start only in 2000). Standard errors: clustered at LLM level.

Figure 4: Yearly Interactions of Bite on Log Employment

To provide evidence that these results are not driven by pre-trends depending on *bite*, I adapt Equation 1 to a more dynamic panel event study design. Namely, *bite* in Equation 2 is interacted not only with the *post* dummy, but with each *year*. I report the estimated coefficients β_t of these interactions in Figure 4.

$$logE_{it} = \alpha + \beta_t \sum (T \times bite_i) + \sum \delta County_i + \theta X_{it} + \epsilon_{it} \tag{2}$$

Panel A of Figure 4 shows the coefficients β_t from this specification without covariates for West and East Germany separately. The specifications show strong pre-trends. Panel B shows the coefficients conditional on covariates. This reduces the pre-trends in both East and West Germany, but also shifts the treatment effects towards zero. The coefficients from 2004 onwards remain statistically significant in the West German sample. In terms of magnitude, the effect size increases steadily but moderately. In the East German sample, the effects are also negative from 2004, but only statistically significant in 2007.

To further examine the effect on regional disparities within West and East Germany, I repeat the regressions of Table 1, replacing the interaction of the *post* dummy with the *bite* variable with interactions of the dummy with four *bite* variables. These split *bite* variables take the value of *bite* for the corresponding quartile, and zero otherwise. Thus, these regressions allow to

estimate different treatment effects by *bite* level.

The quantitative interpretation is based on columns (4) and (9) of Table 2 for West and East Germany, respectively. Figure 4 motivates this choice, as it shows that the inclusion of the covariates is necessary to control for pre-trends. The interactions by *bite* quartiles show different results between West and East Germany. While all the splitted terms are not statistically significant different from zero in East Germany when including the control variables, there is evidence of heterogeneity in West Germany. The negative effect of *bite* seems to be driven by regions in the highest *bite* quartile. Although the coefficients are similar in magnitude, the only statistically significant coefficient is that of the interaction of *post* and *bite* in the highest quartile. This pattern is confirmed when the *year* fixed effects are replaced by a *trend* variable, see column (5). A similar pattern is also found in column (2), where two-way fixed effects are included, but no covariates. The negative effect is then present in the third and fourth quartile interactions. Economically lagging regions benefited less from the employment effects of the Hartz reform than already prosperous regions, as the negative effect of *bite* in West Germany is driven by counties with high *bite* values.

One potential explanation for the heterogeneous employment effect could be internal migration. An explicit secondary goal of the reform was to encourage internal migration so that the unemployed would move to regions where they could find jobs. Unfortunately, I do not have adequate data on internal migration. In Table A.2, I show the results of repeating the analyses with population as the dependent variable. It can be seen that a higher *bite* decreases population. Although population is not the perfect measure, it at least suggests that people leave high-*bite* regions to find employment elsewhere. Thus, the uneven effect of the reform on employment could have an overall welfare-enhancing effect if it led to a more efficient geographic distribution of the labor force. However, this classical view has recently been challenged by Bilal (2023), who argues that productive employers overvalue locating near one another, leading to high spatial unemployment differentials and suboptimal aggregate welfare, *i.e.* on the contrary promoting place-based policies for lagging regions.

	Dependent variable: Log Number Employed									
	West Germany					East Germany				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.074*				−0.006	0.052				−0.014
	(0.039)				(0.022)	(0.078)				(0.054)
After x Bite_{Q1}	−0.009	−0.009	−0.004	−0.003	−0.004	−0.009	−0.009	−0.005	−0.001	−0.001
	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
After x Bite_{Q2}	−0.009*	−0.009	−0.004	−0.003	−0.003	−0.010**	−0.010**	−0.006*	−0.002	−0.002
	(0.005)	(0.006)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
After x Bite_{Q3}	−0.010**	−0.010**	−0.006*	−0.003	−0.003	−0.010**	−0.010**	−0.006*	−0.002	−0.002
	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)
After x Bite_{Q4}	−0.009***	−0.009***	−0.005**	−0.003*	−0.004*	−0.010***	−0.010***	−0.005**	−0.001	−0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)
County FE	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	no	no	yes	yes	yes	no
Trend	no	no	no	no	yes	no	no	no	no	yes
Controls	no	no	no	yes	yes	no	no	no	yes	yes
Time	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008
Observations	4,212	4,212	2,916	2,916	2,916	1,001	1,001	693	693	693

Notes: The table reports estimates from the DiD in continuous treatment regressions on the county level separately for the West and East German subsamples in columns (1) to (5) and (6) to (10), respectively. The dependent variable in all columns is the log number employed. Covariates are added stepwise by column as indicated. In columns (5) and (10) year fixed effects are replaced with a linear time trend. *Bite* is the unemployment share in 2002 and splitted by its quartiles into four variables. *post* is a dummy that takes the value 1 after 2003, when the reform starts, and 0 otherwise. Covariates include GDP, employment shares of sectors (agriculture, production, manufacturing, construction, trade, finance), mean age (linearly and squared), and population. Standard errors in brackets are clustered at the local labor market region. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 2: Heterogeneous Effect of the Reform on the Log Number Employed at County Level

5.1 Robustness Checks

One possible concern is that these results are driven by other factors, such as demographic change. The level of employment could be decreasing because the population is decreasing. Therefore, I repeat the regressions, but with the employment rate as the outcome variable. Second, as discussed in the description of the reforms in Section 2, the long-term unemployment rate might better capture the true bite of the reforms. Third, one might argue that the county level is not the appropriate geographic level because employment responses to a labor market reform may include commuting to neighboring counties or other spillovers. Finally, I implement the novel synthetic DiD design developed by Arkhangelsky et al. (2021).

Table A.3 shows the results of the regressions with the employment rate as the outcome variable. The interaction of the *post* dummy with *bite* remains negative and statistically significant for the West German sample. For East Germany I find no statistically significant coefficient on the interaction. Repeating the regressions with the long-term unemployment rate as a proxy for the *bite* of the reforms in Table A.4 yields qualitatively identical results as with the other measure of *bite*. The only difference is that the coefficient on the interaction is no longer statistically significant in the specification with covariates in East Germany, although the coefficient remains the same in magnitude. To control for spatial spillovers, I repeat the regressions at the local labor market level (see Table A.5). The results are qualitatively identical to those at the county level and are also similar in magnitude for the West German sample. As in the exercise with long-term unemployment as the bite measure, the regression results for the East German sample at the LLM level in columns (6) to (8) show qualitatively the same results. It is only when control variables are included that the interaction is no longer statistically significant. This could be partly due to the small number of observations, since East Germany is smaller anyway and the higher aggregate level leads to even fewer observations. However, the three treatment effects that remain statistically significant (see columns 6, 7 and 8) point in the same direction as the corresponding county-level regressions (see Table 1). Overall, the results are confirmed by these three robustness checks, although the evidence is stronger for West Germany than for East Germany.

When the estimation is repeated using the synthetic DiD approach, the results are qualitatively the same. Figure A.5 illustrates this estimation for West and East Germany in Panels (a) and (b), respectively. The point estimate is a reduction of 1.3 % for both regions, which is slightly larger than the most closely related classical DiD specifications. The time weights indicate that only 2002 is used to match pre-treatment outcomes, while many units are given a non-zero weight[16].

[16]Unit weights underlying the SDiD estimation are shown in Figure A.6.

6 Conclusion

Have the Hartz reforms boosted employment across Germany? Although the reforms have been controversially discussed by the public and a large evaluation literature exists, evidence on the effects of the reforms at the regional level is scarce. This paper contributes to the empirical evidence on the employment effects of the reforms at the county level. I apply a continuous difference-in-differences framework, pioneered by Card (1992) and widely used in the minimum wage context, using county-level unemployment rates prior to the reform to proxy for the bite of the reforms in the regional labor market.

The results show that the Hartz reforms boosted employment, but with important heterogeneities. Employment levels increased more in economically prosperous regions than in economically lagging regions. An increase in the reform's bite decreases employment in both West and East Germany. To analyze the effects of regional disparities in more detail, I repeat the analysis with split bite variables. This exercise suggests different results for West and East Germany. While the effect of bite is relatively homogeneous in East Germany, there is evidence of heterogeneity in West Germany. The negative effect of bite seems to be driven by regions in the highest bite quartile. However, the regional heterogeneity is not large enough to cause an overall negative effect of the reform. Thus, while the Hartz reforms boosted employment overall, employment increased most in counties that were already prosperous. Since internal migration of the unemployed toward regions where they can take a job was an explicit goal of the reform, this heterogeneity is not surprising and may even be welfare enhancing if the moves lead to a more efficient distribution of the labor force.

References

Ahlfeldt, Gabriel M, Duncan Roth, and Tobias Seidel, "The regional effects of Germany's national minimum wage," *Economics Letters*, 2018, *172*, 127–130.

Arkhangelsky, Dmitry, Susan Athey, David A Hirshberg, Guido W Imbens, and Stefan Wager, "Synthetic difference-in-differences," *American Economic Review*, 2021, *111* (12), 4088–4118.

Arntz, Melanie, Markus Clauss, Margit Kraus, Reinhold Schnabel, Alexander Spermann, and Jürgen Wiemers, "Arbeitsangebotseffekte und Verteilungswirkungen der Hartz-IV-Reform," IAB-Forschungsbericht 200710, Institut für Arbeitsmarkt- und Berufsforschung (IAB), Nürnberg 2007.

Bauer, Thomas and Regina T Riphahn, "Employment effects of payroll taxes-an empirical test for Germany," *Applied Economics*, 2002, *34* (7), 865–876.

BBSR, "Referenzschlüssel Kreise von 1990 bis 2018," Technical Report, Bundesamt für Bauwesen und Raumordnung unpublished.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How much should we trust differences-in-differences estimates?," *The Quarterly journal of economics*, 2004, *119* (1), 249–275.

Bilal, Adrien, "The Geography of Unemployment," *The Quarterly Journal of Economics*, 03 2023, *138* (3), 1507–1576.

Bloom, David E, Jeffrey D Sachs, Paul Collier, and Christopher Udry, "Geography, demography, and economic growth in Africa," *Brookings papers on economic activity*, 1998, *1998* (2), 207–295.

Bofinger, Peter, Wolfgang Franz, Bert Rürup, Beatrice Weder di Mauro, and Wolfgang Wiegard, *Die Chance nutzen-Reformen mutig voranbringen. Jahresgutachten 2005/06*, Sachverständigenrat zur Begutachtung der Gesamtwirtschaftlichen Entwicklung, 2005.

Breidenbach, Philipp, Rüdiger Budde, Lea Eilers, Matthias Kaeding, and Sandra Schaffner, "Überprüfung des Zuschnitts von Arbeitsmarktregionen für die Neuabgrenzung des GRW-Fördergebiets ab 2021. Endbericht: Studie im Auftrag des Bundesministeriums für Wirtschaft und Energie (BMWi)," Technical Report, RWI Projektberichte 2018.

Burkhauser, Richard V, Kenneth A Couch, and David C Wittenburg, "A reassessment of the new economics of the minimum wage literature with monthly data from the Current Population Survey," *Journal of Labor Economics*, 2000, *18* (4), 653–680.

Caliendo, Marco, Alexandra Fedorets, Malte Preuss, Carsten Schröder, and Linda Wittbrodt, "The short-run employment effects of the German minimum wage reform," *Labour Economics*, 2018, *53*, 46–62.

_ , **Konstantinos Tatsiramos, and Arne Uhlendorff**, "Benefit duration, unemployment duration and job match quality: a regression-discontinuity approach," *Journal of applied econometrics*, 2013, *28* (4), 604–627.

Card, David, "Using regional variation in wages to measure the effects of the federal minimum wage," *Ilr Review*, 1992, *46* (1), 22–37.

_ **and Alan B Krueger**, "Minimum wages and employment: a case study of the fast-food industry in New Jersey and Pennsylvania: reply," *American Economic Review*, 2000, *90* (5), 1397–1420.

Davis, Donald R and David E Weinstein, "Market access, economic geography and comparative advantage: an empirical test," *Journal of International Economics*, 2003, *59* (1), 1–23.

Diamond, Jared and Colin Renfrew, "Guns, germs, and steel: The fates of human societies," *Nature*, 1997, *386* (6623), 339–339.

Dolton, Peter, Chiara Rosazza Bondibene, and Jonathan Wadsworth, "The UK national minimum wage in retrospect," *Fiscal studies*, 2010, *31* (4), 509–534.

Dube, Arindrajit, T William Lester, and Michael Reich, "Minimum wage effects across state borders: Estimates using contiguous counties," *The review of economics and statistics*, 2010, *92* (4), 945–964.

Eeckhout, Jan, Roberto Pinheiro, and Kurt Schmidheiny, "Spatial sorting," *Journal of Political Economy*, 2014, *122* (3), 554–620.

Eichhorst, Werner, Maria Grienberger-Zingerle, and Regina Konle-Seidl, "Activating Labor Market and Social Policies in Germany: From Status Protection to Basic Income Support.," *German Policy Studies/Politikfeldanalyse*, 2010, *6* (1).

Eurostat, "Arbeitslosendaten nach Geschlecht und Alter - jährliche Daten," Technical Report, European Statistical Office 2020.

Fahr, René and Uwe Sunde, "Did the Hartz reforms speed-up the matching process? A macro-evaluation using empirical matching functions," *German economic review*, 2009, *10* (3), 284–316.

Federal and State Statistical Offices, "Arbeitslose nach ausgewählten Personengruppen sowie Arbeitslosenquoten - Jahresdurchschnitt - regionale Tiefe: Kreise und krfr. Städte. (13211-02-05-4)," Technical Report, Statistisches Bundesamt 2020.

Gallup, John Luke, Jeffrey D Sachs, and Andrew D Mellinger, "Geography and economic development," *International regional science review*, 1999, *22* (2), 179–232.

Gaubert, Cecile, "Firm sorting and agglomeration," *American Economic Review*, 2018, *108* (11), 3117–53.

Goebel, Jan and Maria Richter, "Nach der Einführung von Arbeitslosengeld II: deutlich mehr Verlierer als Gewinner unter den Hilfeempfängern," *Diw Wochenbericht*, 2007, *74* (50), 753–761.

Granato, Nadia, Anette Haas, Silke Hamann, and Annekatrin Niebuhr, "The impact of skill-specific migration on regional unemployment disparities in Germany," *Journal of Regional Science*, 2015, *55* (4), 513–539.

Hartung, Benjamin, Philip Jung, and Moritz Kuhn, "What hides behind the German labor market miracle? Unemployment insurance reforms and labor market dynamics," 2018.

Hertweck, Matthias S and Oliver Sigrist, "The aggregate effects of the Hartz reforms in Germany," *SOEPpaper*, 2013.

Hillmann, Katja, "Does the Hartz IV Reform have an Effect on Matching Efficiency in Germany? A Stochastic Frontier Approach," *Munich Personal RePEc Archive*, 2009.

Hlavac, Marek, "Stargazer: Well-formatted regression and summary statistics tables," *R package version*, 2018, *5* (1).

Jacobi, Lena and Jochen Kluve, "Before and after the Hartz reforms: The performance of active labour market policy in Germany," *Journal for Labour Market Research*, 2007, *40* (1), 45–64.

Klinger, Sabine and Thomas Rothe, "The impact of labour market reforms and economic performance on the matching of the short-term and the long-term unemployed," *Scottish journal of political economy*, 2012, *59* (1), 90–114.

Krause, Michael U and Harald Uhlig, "Transitions in the German labor market: Structure and crisis," *Journal of Monetary Economics*, 2012, *59* (1), 64–79.

Krebs, Tom and Martin Scheffel, "Macroeconomic evaluation of labor market reform in Germany," *IMF Economic Review*, 2013, *61* (4), 664–701.

Krueger, Alan B and Andreas Mueller, "Job search and unemployment insurance: New evidence from time use data," *Journal of Public Economics*, 2010, *94* (3-4), 298–307.

Lalive, Rafael, "Unemployment benefits, unemployment duration, and post-unemployment jobs: A regression discontinuity approach," *American Economic Review*, 2007, *97* (2), 108–112.

_ , Jan Van Ours, and Josef Zweimüller, "How changes in financial incentives affect the duration of unemployment," *The Review of Economic Studies*, 2006, *73* (4), 1009–1038.

Launov, Andrey and Klaus Wälde, "Estimating incentive and welfare effects of nonstationary unemployment benefits," *International Economic Review*, 2013, *54* (4), 1159–1198.

_ **and** _ , "The employment effect of reforming a public employment agency," *European Economic Review*, 2016, *84*, 140–164.

Manow, Philip and Eric Seils, "Adjusting Badly the German Welfare State, Structural Change, and the Open Economy," in "Welfare and Work in the Open Economy Volume II: Diverse Responses to Common Challenges in Twelve Countries," Oxford University Press, September 2000.

Nickell, Stephen, Luca Nunziata, and Wolfgang Ochel, "Unemployment in the OECD since the 1960s. What do we know?," *The Economic Journal*, 2005, *115* (500), 1–27.

Niebuhr, Annekatrin, Nadia Granato, Anette Haas, and Silke Hamann, "Does labour mobility reduce disparities between regional labour markets in Germany?," *Regional Studies*, 2012, *46* (7), 841–858.

Price, Brendan, "The duration and wage effects of long-term unemployment benefits: Evidence from Germany's Hartz IV reform," *MIT Working Paper*, 2016.

Redding, Stephen J and Daniel M Sturm, "The costs of remoteness: Evidence from German division and reunification," *American Economic Review*, 2008, *98* (5), 1766–97.

Schmieder, Johannes F, Till Von Wachter, and Stefan Bender, "The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years," *The Quarterly Journal of Economics*, 2012, *127* (2), 701–752.

Siebert, Horst, "Labor market rigidities: at the root of unemployment in Europe," *Journal of Economic perspectives*, 1997, *11* (3), 37–54.

Stewart, Mark B, "Estimating the impact of the minimum wage using geographical wage variation," *oxford Bulletin of Economics and Statistics*, 2002, *64*, 583–605.

Streeck, Wolfgang and Christine Trampusch, "Economic reform and the political economy of the German welfare state," *German Politics*, 2005, *14* (2), 174–195.

Tazhitdinova, Alisa, "Do only tax incentives matter? Labor supply and demand responses to an unusually large and salient tax break," *Journal of Public Economics*, 2020, *184*, 104162.

A Appendix

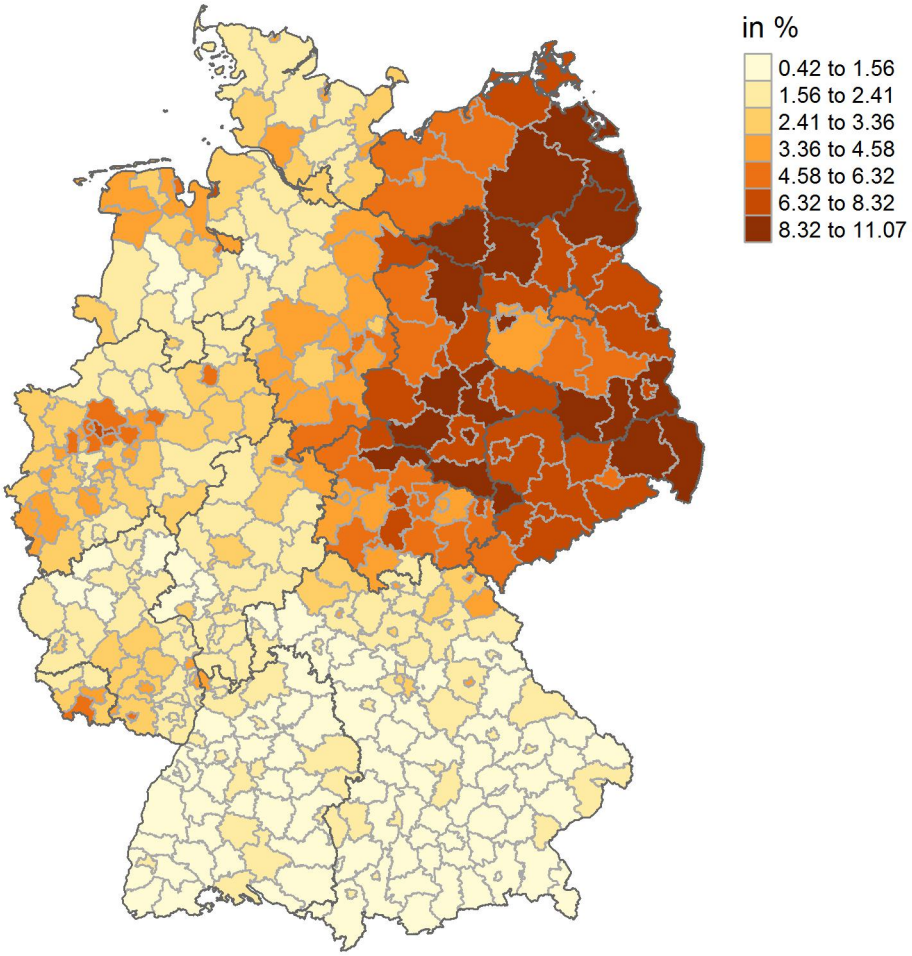


Figure A.1: Long-term Unemployment Rate at County Level (© GeoBasis-DE / BKG 2020)

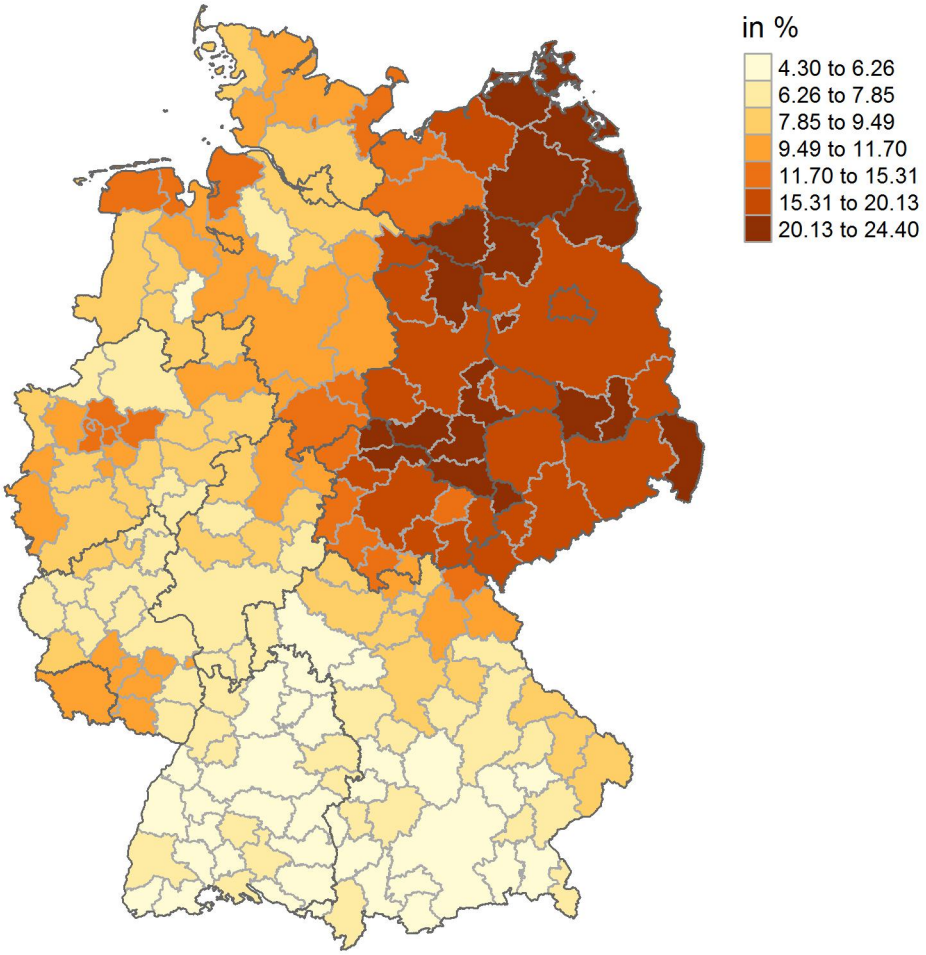


Figure A.2: Unemployment Rate at Local Labor Market Level (© GeoBasis-DE / BKG 2020)

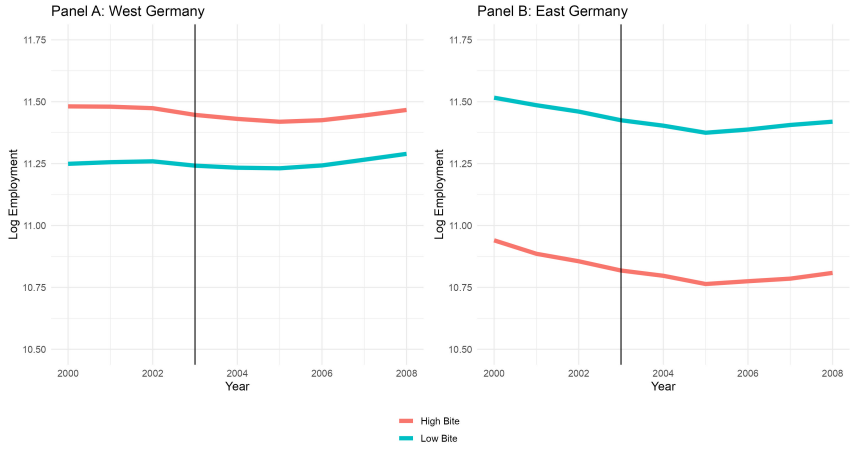


Figure A.3: Average Log Employment by Bite below/above Median by Region at LLM level for balanced Sample

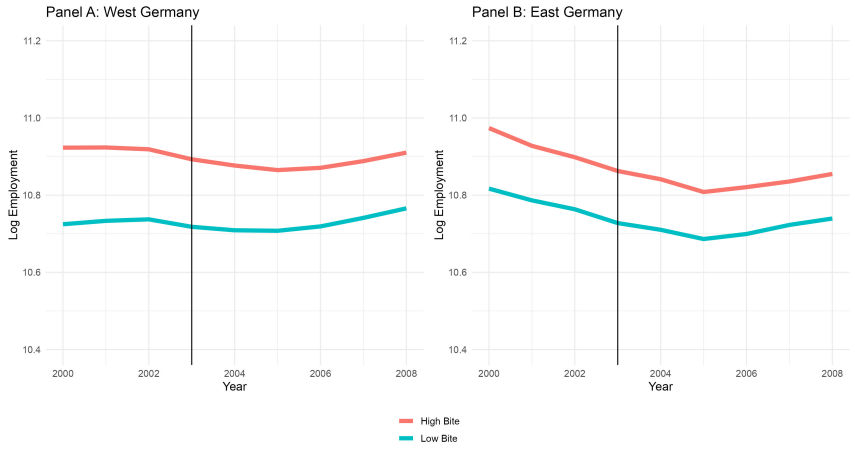
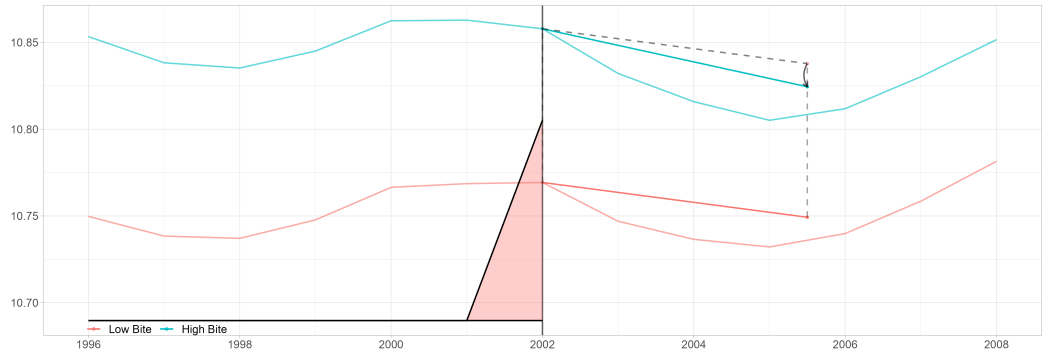
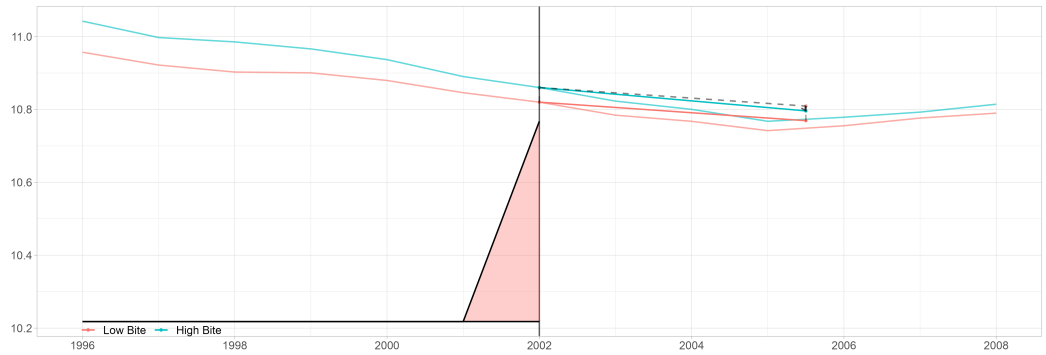


Figure A.4: Average Log Employment by Bite (long-term) below/above Median by Region for balanced Sample



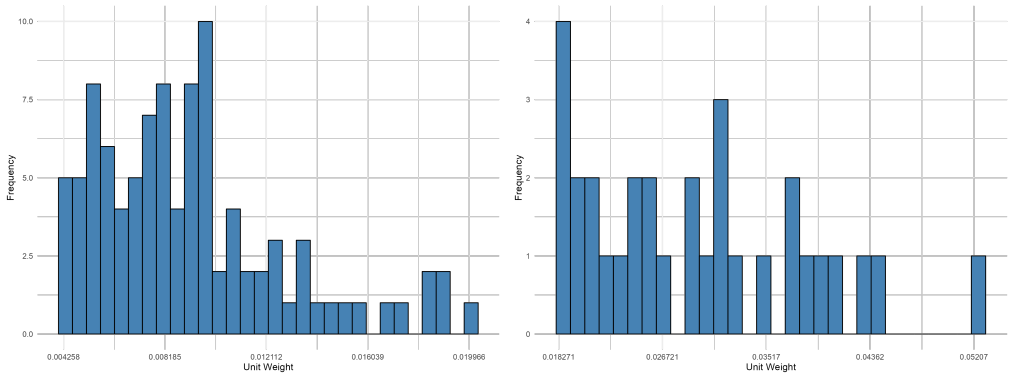
(a) West Germany $\beta = -0.013^{***}$ (0.003)



(b) East Germany $\beta = -0.013^{*}$ (0.007)

Notes: The figure plots the results of applying the synthetic diff-in-diff estimator on the log number of employed person for West and East Germany in Panel (a) and (b), respectively. The vertical line indicates the last pre-treatment year 2002. The two panels show the trend of the log number employed by high- and low-bite counties. The arrows indicate the estimated effects β, which are the differences between counties in low-bite counties and the (projected) counterfactuals. The synthetic diff-in-diff estimator compares the change in the log number employed in low-bite schools with the unit- and time-weighted change in the log number employed in high-bite schools. The time weights are shown in the lower parts of each panel, where only 2002 receives a nonzero weight. Dependent variable is the log number of employed. Unit of analysis: counties. Estimation methods: Synthetic Diff-in-Diff on multiple treated units. Sample period: 1996–2008 (treatment from 2003). The bottom graph in red shows the time weights. Unit weights are illustrated in Figure A.6. Standard errors: bootstrapped. *p<0.1; **p<0.05; ***p<0.01

Figure A.5: SDiD Estimates on Log Number Employed



Panel A: West Germany Panel B: East Germany

Notes: Unit weights correspond to weights of counties with a non-zero weight. Weights correspond to the SDiD estimation in the corresponding panel of Figure A.5.

Figure A.6: Unit Weights of Synthetic DID Design

	West Germany				East Germany			
	2000 - 2005		2006 - 2008		2000 - 2005		2006 - 2008	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Dependent Variables:								
Log Employment	10.83	0.71	10.81	0.71	10.86	0.64	10.78	0.65
Log Unemployment*	8.61	0.72	8.71	0.76	9.59	0.68	9.5	0.7
Employment Rate (in %)	33.08	11.74	32.36	11.84	32.36	7.07	30.84	6.77
Variable of Interest:								
Bite	8.37	2.65	8.37	2.65	18.92	3.12	18.92	3.12
Bite (long-term)	2.39	1.26	2.39	1.26	6.96	1.93	6.96	1.93
Covariates:								
GDP p.c.	26.1	11.15	28.48	12.26	17.31	4.47	19.66	4.84
Age (∅)	40.64	1.33	41.81	1.33	41.98	1.15	43.99	1.39
Population	201521	172616	202607	175204	222333	376195	216747	377963
Sector Shares (in %)								
Agriculture	2.65	2.36	2.44	2.2	2.91	1.88	2.67	1.78
Production	23.95	8.5	22.6	8.45	17.44	6.4	17.65	6.92
Manufacturing	22.6	8.67	21.3	8.6	15.31	6.39	15.66	6.87
Construction	6.87	2.15	6.17	2.05	11.49	2.97	8.97	2.54
Trade	25.66	4.16	25.75	4.15	23.86	2.74	23.99	2.85
Finance	11.57	3.96	12.84	4.24	11.4	3.5	13.21	4.16
N	972		1944		231		462	

Notes: The table shows summary statistics for the period before the reform started and for the period since the start. All variables are measured yearly. Sector shares are the employment share of each sector in county-level employment. To avoid multicollinearity, the share of the public sector is omitted in regressions. * Time series starts in 2001.

Table A.1: Summary Statistics

	Dependent variable: Log Population									
	West Germany					East Germany				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.058***				0.016***	0.086*				0.012
	(0.006)				(0.003)	(0.048)				(0.011)
Bite	−0.003					0.047***				
	(0.017)					(0.018)				
After x Bite	−0.006***	−0.006***	−0.003***	−0.002***	−0.002***	−0.007***	−0.007***	−0.005***	−0.001	−0.001
	(0.001)	(0.001)	(0.0005)	(0.0004)	(0.0004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
County FE	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	no	no	yes	yes	yes	no
Trend	no	no	no	no	yes	no	no	no	no	yes
Controls	no	no	no	yes	yes	no	no	no	yes	yes
Time	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008
Average Bite [S.D.]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]
Observations	4,212	4,212	2,916	2,916	2,916	1,001	1,001	693	693	693

Notes: The table reports estimates from the DiD in continuous treatment regressions on the county level separately for the West and East German subsamples in columns (1) to (5) and (6) to (10), respectively. The dependent variable in all columns is the log population. Covariates are added stepwise by column as indicated. In columns (5) and (10) year fixed effects are replaced with a linear time trend. *Bite* is the unemployment share in 2002. *post* is a dummy that takes the value 1 after 2003, when the reform starts, and 0 otherwise. Covariates include GDP, employment shares of sectors (agriculture, production, manufacturing, construction, trade, finance), mean age (linearly and squared), and population. Standard errors in brackets are clustered at the local labor market region. *p<0.1; **p<0.05; ***p<0.01.

Table A.2: Effect of the Reform on Log Population at County Level

	Dependent variable: Employment Rate (in %)									
	West Germany					East Germany				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.536*				−0.455**	−0.715				−2.092***
	(0.280)				(0.193)	(1.385)				(0.681)
Bite	0.935***					−0.550***				
	(0.234)					(0.154)				
After x Bite	−0.099***	−0.099***	−0.089***	−0.060***	−0.065***	−0.093	−0.093	0.005	−0.012	0.043
	(0.034)	(0.036)	(0.028)	(0.023)	(0.022)	(0.065)	(0.068)	(0.038)	(0.039)	(0.036)
County FE	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	no	no	yes	yes	yes	no
Trend	no	no	no	no	yes	no	no	no	no	yes
Controls	no	no	no	yes	yes	no	no	no	yes	yes
Time	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008
Average Bite [S.D.]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	8.37 [2.65]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]	18.92 [3.14]
Observations	4,212	4,212	2,916	2,916	2,916	1,001	1,001	693	693	693

Notes: The table reports estimates from the DiD in continuous treatment regressions on the county level separately for the West and East German subsamples in columns (1) to (5) and (6) to (10), respectively. The dependent variable in all columns is the employment rate. Covariates are added stepwise by column as indicated. In columns (5) and (10) year fixed effects are replaced with a linear time trend. *Bite* is the unemployment share in 2002. *post* is a dummy that takes the value 1 after 2003, when the reform starts, and 0 otherwise. Covariates include GDP, employment shares of sectors (agriculture, production, manufacturing, construction, trade, finance), mean age (linearly and squared), and population. Standard errors in brackets are clustered at the local labor market region. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.3: Effect of the Reform on the Employment Rate at County Level

	Dependent variable: Log Number Employed									
	West Germany					East Germany				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.041***				−0.019***	−0.013				−0.041***
	(0.008)				(0.004)	(0.024)				(0.015)
Bite	0.085**					0.057				
	(0.038)					(0.038)				
After x Bite	−0.019***	−0.019***	−0.013***	−0.006***	−0.006***	−0.017***	−0.017***	−0.007***	−0.002	0.0001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
County FE	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	no	no	yes	yes	yes	no
Trend	no	no	no	no	yes	no	no	no	no	yes
Controls	no	no	no	yes	yes	no	no	no	yes	yes
Time	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008
Average Bite [S.D.]	2.39 [1.26]	2.39 [1.26]	2.39 [1.26]	2.39 [1.26]	2.39 [1.26]	6.96 [1.94]	6.96 [1.94]	6.96 [1.94]	6.96 [1.94]	6.96 [1.94]
Observations	4,212	4,212	2,916	2,916	2,916	1,001	1,001	693	693	693

Notes: The table reports estimates from the DiD in continuous treatment regressions on the county level separately for the West and East German subsamples in columns (1) to (5) and (6) to (10), respectively. The dependent variable in all columns is the log number employed. Covariates are added stepwise by column as indicated. In columns (5) and (10) year fixed effects are replaced with a linear time trend. *Bite* is the long-term unemployment share in 2002. *post* is a dummy that takes the value 1 after 2003, when the reform starts, and 0 otherwise. Covariates include GDP, employment shares of sectors (agriculture, production, manufacturing, construction, trade, finance), mean age (linearly and squared), and population. Standard errors in brackets are clustered at the local labor market region. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.4: Effect of the Reform on the Log Number Employed at County Level with Long-Term Unemployment Bite

	Dependent variable: Log Number Employed									
	West Germany					East Germany				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.096***				−0.012**	0.056*				−0.061**
	(0.011)				(0.006)	(0.034)				(0.026)
Bite	0.043					−0.046				
	(0.040)					(0.040)				
After x Bite	−0.012***	−0.012***	−0.009***	−0.002***	−0.003***	−0.010***	−0.010***	−0.004**	−0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
County FE	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Year FE	no	yes	yes	yes	no	no	yes	yes	yes	no
Trend	no	no	no	no	yes	no	no	no	no	yes
Controls	no	no	no	yes	yes	no	no	no	yes	yes
Time	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008	1996-2008	1996-2008	2000-2008	2000-2008	2000-2008
Average Bite [S.D.]	8.29 [2.42]	8.29 [2.42]	8.29 [2.42]	8.29 [2.42]	8.29 [2.42]	20.02 [3.14]	20.02 [3.14]	20.02 [3.14]	20.02 [3.14]	20.02 [3.14]
Observations	1,833	1,833	1,269	1,269	1,269	533	533	369	369	369

Notes: The table reports estimates from the DiD in continuous treatment regressions on the LLM level separately for the West and East German subsamples in columns (1) to (5) and (6) to (10), respectively. The dependent variable in all columns is the log number employed. Covariates are added stepwise by column as indicated. In columns (5) and (10) year fixed effects are replaced with a linear time trend. *Bite* is the unemployment share in 2002. *post* is a dummy that takes the value 1 after 2003, when the reform starts, and 0 otherwise. Covariates include GDP, employment shares of sectors (agriculture, production, manufacturing, construction, trade, finance), mean age (linearly and squared), and population. Standard errors in brackets are clustered at the local labor market region. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.5: Effect of the Reform on the Log Number Employed at LLM Level