Essays on
Labor Market Dynamics

Inauguraldissertation zur Erlangung des Doktorgrades
des Fachbereichs Wirtschaftswissenschaften der
Ruprecht-Karls-Universität Heidelberg

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Heidelberg
Januar 2020

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Chapter 1

General Introduction

Labor market dynamics, especially the flexibility of labor markets, are central themes in political debates as means to tackle high unemployment rates and low economic growth in Europe. Understanding the factors that encourage and impede labor force participation and worker mobility is therefore of central concern; to create active labor market policies that fit the needs of future labor markets (European Commission, 2019).

Over the last years, the world of work has transformed considerably through globalization, technological progress, and demographic trends. As a result, the demand for skills in the labor market is changing rapidly, and the pace at which workers need to adapt may even accelerate faster in the future (OECD, 2018). However, the ability to adjust quickly to changing conditions in labor demand, the wage rate, and the product market, may be hindered by barriers to entry and changing economic conditions, which ultimately impede labor market participation and future labor supply.

Over the last decade, researchers have considered regulatory entry barriers for firms as one major impediment to more dynamic labor markets, hindering competition, job creation, and partly accounting for low economic growth rates in Europe (OECD, 2019a). In the economic literature, studies have shown that stricter entry regulation leads to negative effects on employment growth and job creation (see e.g. Bertrand and Kramarz, 2002); while reducing them increases firm entry and output growth (see e.g. Aghion et al., 2008).

Yet, entry barriers need not solely be related to rules for opening a business. In the literature on occupational licensing, entry into a profession is tied to a certain level of training or skill requirement (human capital investment) of the worker (see e.g. Rottenberg, 1962; Shapiro, 1986). Since the seminal work by Friedman and Kuznets (1945), a small literature has analyzed the consequences of occupational entry barriers on prices, quantity and service quality (e.g. Kleiner and Kudrle, 2000, Kleiner, 2000

and Angrist and Guryan, 2004). Still, existing studies focusing on the wage effects of occupational licensing reach no common consensus (e.g. Kleiner and Krueger, 2013; Angrist and Guryan, 2004). It is therefore of interest, for economists and politicians alike, to understand the labor market consequences of deregulating occupational licensing requirements for workers and young labor market entrants.

At the same time, we observe a declining trend in fertility rates across Europe and an aging working population, which may have long-lasting consequences on future labor supply and economic growth. A large economic literature has emphasized the importance of fertility behavior and childcare to understand the labor supply decisions of women (see, for example, Killingsworth and Heckman, 1986; Montgomery and Trussell, 1986 and Blundell and Macurdy, 1999 for a comprehensive overview of the theoretical and empirical literature).

In many industrialized countries, female labor force participation has increased over recent decades, while fertility rates have declined (see, for example, Blau and Kahn, 2007 and Goldin, 2006 for the US case). Between 2000 and 2018 the participation rate of 15-64-year-old women has increased from 62 to 70 percent in the traditional European countries. In particular, German female labor force participation has increased from 63 to 74 percent in 2018 (OECD, 2019b). If the number of older workers moving into retirement increases through an aging working population and the number of young labor market entrants declines, pension costs would increase for a shrinking workforce. Against this background, policies to secure future labor markets, counteracting skill shortages and sustaining the social security system, are of central concern for today's policy makers. Consequently, it is an important task for economic research to understand the drivers of low fertility rates.

This dissertation consists of three essays, covering labor market dynamics along three different dimensions: for one, the labor market implications of liberalizing a highly regulated sector in terms of wage dynamics, employment changes and worker mobility of incumbents, as well as entry and self-employment. The second dimension studies the impact of deregulating entry barriers on career paths and labor market outcomes of apprentices, answering the question whether deregulation encourages or inhibits entry into vocational training. And the third dimension focuses on the link between permanent changes in wages and employment and the timing of fertility decisions for women.

Chapter 2, which is joint work with Christina Gathmann, analyzes the labor market consequences of abolishing occupational licensing requirements in favor of (voluntary) certification in the German crafts and trades sector. Most previous studies on occupational licensing have covered the US, where labor markets have been traditionally

less regulated and licensing requirements have increased over time. Yet, the effects of adopting or abolishing licensing or certification requirements might not be symmetric. Therefore, we analyze the effect of lower entry barriers in a highly regulated market on labor market effects for incumbent workers and certified workers in terms of employment and wage dynamics in the German crafts and trades. Further, we study how entry, exit and self-employment have changed after liberalization. Therefore, the chapter focuses on the impact of a reduction in licensing requirements on labor market outcomes, such as wage dynamics, employment, and mobility of incumbents, as well as entry and self-employment.

In Chapter 3, I examine the effect of deregulating entry requirements on the apprenticeship market and post-apprenticeship labor market outcomes in Germany. Specifically, this chapter addresses the research question how deregulation affects apprenticeship entry and training levels in the German crafts and trades and studies the consequences for individual labor market earnings after vocational training. The focus of this chapter lies on the labor market effects of deregulation for career paths of apprentices and earnings dynamics of young labor market entrants.

Chapter 4 is joint work with Melanie Arntz and Christina Gathmann and proposes a new explanation for the East German fertility crisis: permanent shifts in the East German wage structure that are plausibly exogenous from the individual's perspective. While a large literature has documented the fertility crisis in East Germany, little is known about the causes of the dramatic decline for East German women after unification. In light of demographic change, it has become a central task for economists to understand the determinants of low fertility rates; and to identify policies, through which childbearing can be encouraged, to sustain the social security system or the future economic competitiveness. Therefore, this chapter addresses how employment and wage dynamics affect fertility behavior and labor force participation of women during the rapid transition from a socialist to a market economy.

Overall, this thesis underlines the importance of labor market dynamics, and to understand the determinants of low fertility behavior, when designing optimal policies to stimulate labor force participation, economic performance, and fertility behavior. A common aspect in all three chapters is changing labor markets. Moving from an occupational licensing regime to one of certification in Chapter 1, the removal of occupational entry restrictions and consequently the opening of new career paths for trained apprentices in Chapter 2; and the transition from a socialist to a market economy with substantial changes in the wage and employment structure in Chapter 3. In the following, I now present Chapters 2 to 4 in more detail.

1.1 From Occupational Licensing to Certification: Labor Market Effects of Liberalization

Over the last three decades, many sectors in Europe have undergone a shift toward lowering entry barriers, since too restrictive (firm) entry regulation can have negative effects on competition and better economic performance. Since Friedman and Kuznets (1945)'s classic work, a small and relatively recent literature has analyzed the economic consequences of occupational entry barriers, where entry is tied to certain skill or training requirements (often demonstrated by a formal degree or other credentials). Existing studies typically analyze occupational licensing in the US and for few selected services, where labor markets have been less regulated, and licensing requirements have increased over time (see e.g. Kleiner and Kudrle, 2000 for a survey; Gittleman and Kleiner, 2016; Kleiner and Krueger, 2013 and Kleiner and Park, 2010). While these studies try to carefully control for observable differences and simultaneity, they are unable to fully resolve them.

To fill this gap in the literature, my co-author and I analyze the labor market consequences of occupational licensing and certification in the German crafts and trades, a large and highly regulated sector. Using social security data on workers and firms, we exploit a reform in the German crafts and trades that eliminated strict occupational licensing requirements for some occupations but not for others for identification. After the introduction of the reform, a Master craftsman degree was no longer necessary prerequisite to open up and run a business. Our main analysis combines matching with a difference-in-differences approach to compare the development of careers and wages of incumbents in the liberalized crafts to the regulated crafts.

The labor market consequences of switching from licensing to certification for craftsmen − with or without a Master craftsman title − are from a theoretical perspective a-priori not so clear-cut. Deregulation offers new job opportunities for individuals without the necessary qualifications to open and run a business in the crafts and trades. Consequently, we expect more entry and employment opportunities in the liberalized trades after the reform. Increased competition puts downward pressure on wages in the deregulated trades. Whether the reform affects the wages of incumbent craftsmen or new entrants into the liberalized occupations is an empirical question. Deregulation could also raise the demand for the services among consumers who might have been priced out of the market under the licensing regime. If demand increases, employment growth could be stronger and wage reductions smaller or even absent in the liberalized occupations. Finally, deregulation might also increase wage differentials through two channels. First, the deregulation is likely to reduce the number of Master craftsmen

because fewer craftsmen are likely to get certified, while more craftsmen enter the sector without a certification. Second, tougher competition after deregulation could raise the wage premium for Master craftsmen if skills reduce the marginal cost of production (Guadalupe, 2007; Boone, 2008), or the marginal cost of producing quality in a vertically differentiated market with imperfectly observable quality (Shapiro, 1983, 1986).

The empirical analysis shows that despite more entry into the liberalized occupations, average wages and employment for incumbent workers do not decline. As such, incumbent craftsmen in the deregulated occupations do not lose out after liberalization contrary to widespread concern. Neither do they lose their job or change the liberalized occupations or industries after the reform. We observe, however, a sizable wage increase for certified employed individuals (Master craftsmen) after the reform. Actually, Master craftsmen benefit from the liberalization since they earn a higher skill premium after the reform. This result is consistent with the above-mentioned theoretical channels. Suggestive evidence from plant level data and the German Microcensus indicates more entry and fewer exits of plants after the reform and a rise in self-employment, especially among immigrants. As such, the deregulation might have facilitated the transition of individuals from the EU-10 Central and Eastern Europe accession countries to find employment in Germany.

1.2 Deregulation and Training: Evidence from a Crafts and Trades Reform

In Chapter 3, I analyze the labor market effects of deregulating entry requirements on the career paths of apprentices, entry into apprenticeship, exits and post-apprenticeship labor market outcomes. For this purpose, I exploit variation from the crafts and trades reform used in Chapter 2, through which some occupations no longer required a Master craftsman degree to open up a business and train apprentices after 2004.

Deregulating entry restrictions may affect the vocational training market through the effect of competition in the product market on the distribution of profits (see e.g. Blanchard and Giavazzi, 2003). As such, lower entry barriers should increase competition through the reduction in entry costs. From a demand-side perspective, this discourages training investments if training is costly due to a reduction in incumbent firms' profits per unit of output (Bassanini and Brunello, 2011). As a result, the demand for apprentices is likely to decrease after deregulation. From a supply-side perspective the reform provides incentives for school-leavers to start an apprenticeship. Yet, this effect depends crucially on future post-apprenticeship earnings after deregulation. If companions open up a business after training completion, they might face stronger

competition and earn lower potential profits. If, on the other hand, they stay employed, they may receive higher wages due to increased outside options (Becker, 1964) or lower wages as profits to the firm decrease (Blanchard and Giavazzi, 2003). As such, how the apprenticeship market reacts in equilibrium is ambiguous.

Using annual occupational data from the Confederation of Skilled Crafts and social security records of workers and firms in Germany, I employ a flexible difference-in-differences approach to compare in a first step entry and exits at the apprenticeship market, and in a second step post-apprenticeship wages at the individual level in deregulated occupations compared to still regulated occupations. Results show that less companions pursue the career of a Master craftsman after deregulation. The evidence confirms the theoretical channel of a dominating demand effect. Specifically, I find a negative net effect in the apprenticeship market, driven by a reduction in new apprenticeship contracts, and a reduction in demand for apprentices in the deregulated trades after the reform. These effects translate into even stronger long-run responses in the total number of apprentices, which are partly driven by higher rates of prematurely canceled training contracts. As such, the results suggest that a higher rate of apprentices is leaving the training market without a degree due to the changing career opportunities. Companions who completed apprenticeship training do not earn higher wages on average if they stay employed. Yet, the average effect masks heterogeneity across firm size. Companions in small firms earn higher wages if being employed in small craft firms. These are the ones which have higher incentives to keep their best apprentices after training completion as skilled workers.

The results indicate that the deregulation sizably reduced the number of Master craftsmen relative to potential Master craftsmen after 2004. Although the number of apprentices declined in all trades, the reform seems to have accelerated the decline in the deregulated trades. Consequently, less apprentices seem to pursue the traditional career path in the deregulated crafts and trades.

1.3 Permanent Changes in the Wage Structure and the East German Fertility Crisis

While Chapters 2 and 3 of my dissertation focus on labor market dynamics as a consequence of reduced entry requirements, contributing to the literature on occupational licensing, entry barriers and deregulation, as well as competition and training; Chapter 4 contributes to the literature on economic incentives and fertility, by analyzing the consequences of permanent changes in wages and employment on fertility behavior.

Although the existing literature has well documented the drastic drop in fertility rates of East German women, the underlying causes of the fertility crisis remain still unclear. Some studies argue that the post-unification drop in the East German fertility rate is a result of the crisis phenomenon and related to economic uncertainty (e.g. Witte and Wagner, 1995; Kreyenfeld, 2010a, and Chevalier and Marie, 2017 on the educational outcomes of children) or an adjustment process to the lower West German fertility levels (e.g. Conrad et al., 1996; Lechner, 2001). Yet, the economic uncertainty argument doesn't explain why the fertility behavior did not readjust in the 1990s when the uncertainty related to unification was resolved. The argument of the adjustment process hypothesis relies on the fact that after 1990 East German women faced similar institutional and economic incentives. But still persistent differences between East and West Germany remain, even two decades after unification (see Kreyenfeld and Konietzka, 2004 and Goldstein and Kreyenfeld, 2011 on differences in socio-economic characteristics). As such, it remains an open question what the driving factors of the East German fertility crisis are.

In this essay, we want to fill this gap in the literature by studying the determinants of low fertility rates in East Germany after the fall of the Berlin Wall. Specifically, we analyze the link between permanent changes in the labor market on fertility behavior, thereby providing a novel explanation for the East German fertility crisis. For this purpose, we combine administrative data from the German Federal Pension Fund on the timing of births for East and West German women, born between 1940 and 1977; information on earnings and household income from the German Socio-Economic Panel (GSOEP), for the pre- and post-unification period; and data on East and West German employment and unemployment rates from administrative social security records. Our empirical strategy identifies the role of permanent wage and employment changes for the demand for children, and the timing of birth in East Germany after unification. Thus, exploiting variation at the age $\times$ time $\times$ region level across education groups.

The results suggest that the East German fertility crisis was the consequence of two combined effects: a postponement of first births and a decline in higher-order births among East German women. Especially, wages and penalties associated with career interruptions became significant determinants of the demand for children and the timing of births after unification. The evidence, therefore, highlights that work incentives play an important role for childbearing decisions.

Chapter 2

From Occupational Licensing to Certification: Labor Market Effects of Liberalization

2.1 Introduction

Entry restrictions are considered one of the main barriers to competition and better economic performance, especially in the service sector. Starting with Friedman and Kuznets (1945), several scholars analyzed the economic consequences of occupational licensing, which ties entry into a profession to specific skill or training requirements. Most studies use cross-sectional variation to compare licensed to unlicensed occupations.[1] The existing evidence on the wage effects of occupational licensing are inconclusive and report a wide range of estimates. While Angrist and Guryan (2004) find that state licensing requirements raise the salaries of teachers with a bachelors degree by 3-5%, Kleiner and Krueger (2013) report a wage premium of 18% in licensed occupations.

We take a fresh look at the debate by analyzing the consequences of occupational licensing and certification in a large and traditionally highly regulated market. The crafts and trades sector in Germany has been one of the most heavily regulated within the

[1]Outcomes include prices (Kleiner and Kudrle, 2000), quantities (Gittleman and Kleiner, 2016; Kleiner and Kudrle, 2000; Kleiner and Park, 2010), service quality (Kane et al., 2008; Law and Kim, 2005) or wages (Kleiner and Kudrle, 2000; Kleiner and Park, 2010).

European Union (Monopolkommission, 1998). The main entry restriction in Germany's crafts and trades has been that only individuals with a degree as a Master craftsman (*Meister*) were allowed to open and run a business; and only a Master craftsman could train young apprentices. Obtaining a Master craftsman degree is associated with substantial costs including long periods of training and monetary expenses (see Figure 2.1). The sector is also economically important employing about 13% of all employees and about one-third of all apprentices in 2012.

Fig. 2.1 Career Path and Market Structure
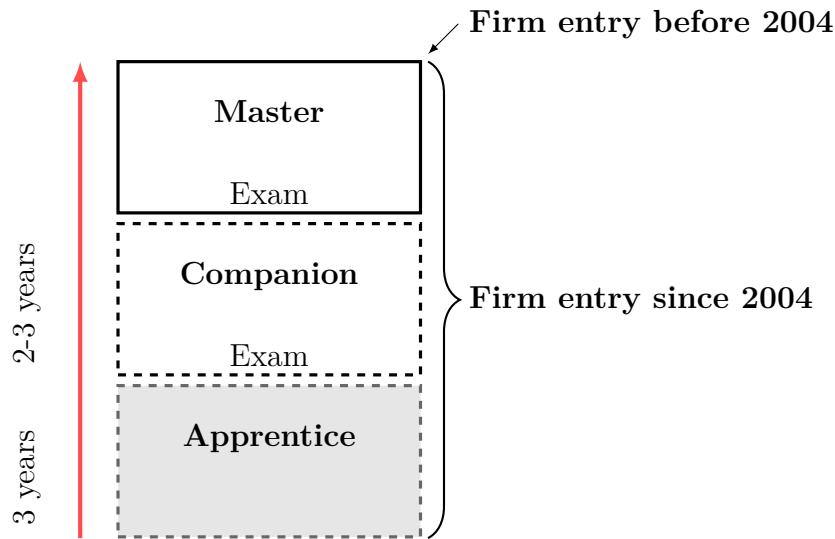
We exploit a large-scale reform that reduced licensing requirements in favor of voluntary certification in many service occupations within the crafts and trades sector. In 2000, the European Court ruled that strict entry barriers were not compatible with the single European market (Monopolkommission, 2001). In response, Germany liberalized entry regulations in the crafts and trades. After 2004, the degree of a Master craftsman was no longer a prerequisite to operate a business in 53 out of the 94 trades ('liberalized trades'). In the remaining 41 trades, the Master craftsman diploma remained the main entry requirement ('regulated trades'). The reform raises important questions about the functioning of regulated labor markets and the consequences of liberalization in the service sector: who benefits or loses from the deregulation? What are the consequences of deregulation for the labor market careers of Masters and other employees? Our analysis will provide answers to these important questions.

The consequences of switching from licensing to certification for the labor market careers of craftsmen, without or with a Master diploma, are a-priori not clear-cut. The deregulation offers new opportunities for individuals without the necessary qualifications to open a business and create new jobs in the crafts and trades. As such, we would

expect more entry and possibly employment opportunities in the liberalized trades after the reform.

Increased competition puts a downward pressure on wages in the deregulated trades. Whether the reform affects the wages of incumbent craftsmen or new entrants into the liberalized occupations is an empirical question. Deregulation could also raise the demand for the services among consumers who might have been priced out of the market under the licensing regime. If demand increases, employment growth could be stronger and wage reductions smaller or even absent in the liberalized occupations. Finally, the deregulation might also increase wage differentials through two channels. First, the deregulation is likely to reduce the number of Master craftsmen because fewer craftsmen are likely to get certified, while more craftsmen enter the sector without a certification. Second, tougher competition after deregulation could raise the skill premium for Master craftsmen if skills reduce the marginal cost of production (Boone, 2008; Guadalupe, 2007) or the marginal cost of producing quality in a vertically differentiated market with imperfectly observable quality (Shapiro, 1983, 1986).

To quantify the reform effects empirically, we require rich panel data with detailed annual information on employment, wages and job mobility in the crafts and trades. Our administrative data from Germany, which are drawn from the population of workers and firms covered by the social security system and span several years prior to and after the reform, are uniquely suited for our purpose. Based on detailed information on each employee's occupation, we can identify the occupations in the crafts and trades sector, and whether the occupation is liberalized in 2004 or not. Furthermore, we can follow the labor market career and wages of each individual even if they switch occupations or industries, become unemployed or move to another region.

Our main empirical strategy combines matching with an event study approach to flexibly compare the careers of craftsmen who worked in a liberalized occupation prior to the reform to those of suitable control individuals. Employees in one of the liberalized occupations may experience very different career trajectories or wage growth than employees of other occupations. We therefore use in the first step a matching procedure to match to each individual employed in a treated occupation just prior to the reform a set of suitable control individuals working in an occupation that remains regulated even after 2004. Our matching procedure combines nearest neighbor matching on the propensity score and works well in removing all observable differences between treated and control groups. In a second step, we use an event study approach to flexibly compare employment, wages and worker mobility in the matched sample.

We have four main results. First, the reform has few effects on average employment and wages of incumbent craftsmen in the liberalized occupations. The absence of

an average effect on wages suggest that any competitive pressure on wages is offset by an increase in demand by consumers who were priced out of the market before the reform. Second, we find a substantial increase in the skill premium for certified individuals (Master craftsmen) in the liberalized occupations who earn up to 8% more after deregulation. Third, incumbent craftsmen in the liberalized occupations are less likely to switch jobs or leave their occupation after the reform, which supports the previous findings that incumbent craftsmen are not negatively affected by the reform.

Our analysis of incumbent craftsmen does not shed light on the importance of entry into the liberalized occupations after the reform. We therefore turn to plant level data to study entry and exit; and survey data to analyze self-employment. We find small effects on plant entry and a decline in plant exits. The latter could imply that the reform made ownership changes or takeovers easier or more likely for craftsmen without the licensing requirements. Yet, we find a sizable rise in self-employment after the reform, esp. among immigrants from the EU accession countries of 2004. As most of the new self-employed craftsmen do not hire additional staff, more self-employment does imply only modest effects on job creation however.

Most analyses of occupational licensing cover the US where labor markets have been less regulated though certification and licensing requirements have increased over time. The effects of adopting or abolishing licensing or certification need not be symmetric. When introducing certification requirements, consumers might not be willing to pay a premium for a certified professional, at least initially. In contrast, abolishing a certification requirement might have little effect on prices or wages as consumers are used to hire certified professionals. Another difference is that the craft and trades sector we analyze consists of a large number of medium-skilled occupations; in contrast, most of the empirical literature has studied teaching or a few selected services in the medical or dental professions (Kleiner and Kudrle, 2000).[2]

In addition to the literature on occupational licensing, our study also contributes to studies on entry barriers. A sizable literature has used changes in the legal procedures for opening up a new business to study firm-level outcomes (see Aghion et al., 2008; Bertrand and Kramarz, 2002; Branstetter et al., 2013; Ciccone and Papaioannou, 2007; Djankov et al., 2002; Klapper et al., 2006; Schaumans and Verboven, 2008; Schivardi and Viviano, 2011).[3] Our analysis investigates a different type of entry barrier: a minimum skill requirement in order to open a new business and train apprentices. Furthermore,

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[2]One study also analyzes self-employment after Germany's 2004 reform using cross-sectional variation (Rostam-Afschar, 2014) though the results have been disputed (Runst et al., 2019).

[3]Bertrand and Kramarz (2002), for instance, evaluate commercial zoning regulations in French retailing. The authors use the introduction of regional zoning boards, which had to approve the establishment of larger businesses in their area, to show that entry barriers reduces employment growth in the retail sector and increases retail prices.

while most of the literature studies the effects on prices, productivity and innovation, we focus instead on the consequences in the labor market.

The paper is organized as follows. The next section discusses the career and market structure in the crafts and trades and the liberalization of 2004. Section 2.3 describes the data and empirical strategy for the analysis of incumbent craftsmen, while Section 2.4 reports how wages, employment and job mobility respond to the reform. Section 2.5 describes the data, empirical strategies and results for entry and exit at the plant level, as well as self-employment in the liberalized crafts and trades. Finally, Section 2.6 discusses the policy implications of our analysis and concludes.

2.2 Institutional Background

2.2.1 The Crafts and Trades Sector

The crafts and trades have been heavily regulated since the Middle Ages when powerful guilds defined who could learn a trade and who could become a Master craftsman (Ogilvie, 2019). Until the 2004 reform, the crafts and trades imposed strict entry requirements that were tied to an occupational license. Only individuals who had obtained a degree as a Master craftsman (*Meister*) could open and run a firm, but only in the specific craft covered by the craftsman degree. In addition, only those with the occupational license could train apprentices, which assist the more experienced craftsmen and are often retained in the firm after completing their training.[4]

Obtaining the occupational license requires several years of training as well as work experience in the specific trade (see Figure 2.1). Individuals first spend 2-3 years as an apprentice under the guidance of a Master craftsman. After passing the trade test (*Gesellenprüfung*), they work as a companion for at least two years followed by a comprehensive exam at the local Chamber of Crafts in order to become a Master craftsman.[5] The entry requirements were closely monitored by the local Chamber of Crafts (*Handwerkskammer*). Every Master craftsman was required to be a member in the local Chamber of Crafts; and every business has to be registered and pay membership fees (*Handwerksrolle*). A craftsman who opened up a business without registering or

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[4]The regulations are set forward in the "Law for the Organization of the Crafts and Trades" (*Gesetz zur Ordnung des Handwerks*) in 1953. Minor reforms were passed in 1994 and 1998. In 1994, EU citizens were offered easier access to the trades in Germany; the 1998 reform reduced the number of trades from 127 to 94, thus allowing firms to offer services in combined trades. Neither reform touched the strict entry regulations, which are the focus of this paper.

[5]The exam contains both practical and theoretical parts, questions on business and law, and a test of pedagogical skills.

fulfilling the prerequisites was fined up to $10,000$ Euros and prosecuted for illegal employment.

Under the European single market, craftsmen from EU countries could apply for an exemption to the strict entry requirements. Obtaining such an exemption imposed substantial time and monetary costs on foreign craftsmen, however (Monopolkommission, 2001). The applicant required written proof (typically in German) of the necessary qualifications and a minimum of six years of uninterrupted work experience as a business owner or facility manager in one of the member countries. After initial screening of the application by the local Chamber of Crafts, the final decision was made by the district president. If the exemption was granted, a craftsman could then register her business in the local Chamber of Crafts.

2.2.2 The Deregulation of 2004

The 2004 reform marked a major liberalization of the entry regulations in Germany's crafts and trades. Most importantly, the degree of a Master craftsman was no longer an entry requirement in 53 out of the 94 crafts and trades.[6] After 2004, anybody could open up a firm and offer services in the liberalized trades. The 2004 reform also eliminated the restriction that only Master craftsmen could train apprentices in the liberalized occupations. Since 2009, individuals in the liberalized occupations have to pass a test to demonstrate their pedagogical and vocational skills before they can train apprentices (BMBF, 2004).

In the other 41 trades, the Master craftsman diploma remained the main entry requirement to open a business and train apprentices. Yet, the 2004 reform expanded access to an occupational license for three groups: the first group were individuals with a professional degree in a closely related field, like engineering or degrees from technical professional schools. The second group were companions with substantial work experience but no Master diploma; or companions with little work experience who passed the Master examination.[7] Finally, a license could also be obtained if the facility manager, rather than the company's owner, had a Master craftsman diploma. Unlike the liberalized occupations, members of all three groups still had to undertake sizable investments that were now considered equivalent to a Master craftsmen diploma.

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[6]While bookbinding occupations were liberalized in 2004, for instance, carpenters remained regulated. A list of liberalized and regulated crafts and trades that can be identified in our data is contained in Appendix Table A.1.

[7]This last rule did not apply to trade services with a high risk potential for the customer like chimney sweepers, opticians, hearing aid or orthopedic technicians.

The reform was mandated by the European court of law, which ruled in October of 2000 that Germany's entry restrictions were inconsistent with the freedom of services granted by Art. 49 of the EU-Treaty.[8] In response, the federal government was required to reform market access to the crafts and trades in Germany. The law proposal passed the Lower House on November 27, 2003, a revised version the Upper House on December 19, 2003, and came into effect almost immediately on January 1, 2004.[9]

2.3 Effects on Employment and Wages

We first analyze how the reform affected incumbent craftsmen who are attached to the labor market and worked in a liberalized (or regulated) occupation in the year prior to the reform.

2.3.1 Data Sources

Individual Social Security Records

To study the effects of the liberalization on employment and wages of craftsmen who were employed in one of the liberalized trades, we use German social security records from 2000 to 2010. The records cover the population of workers and plants covered by the social security system; they do not include the self-employed, civil servants and military personnel. We use a two percent random sample of the administrative records for our analysis.

The data are well suited for investigating the labor market consequences of liberalization. Most importantly, we observe the occupation of each individual at a detailed enough level (3-digit) to identify job spells in the crafts and trades sector. Specifically, we can identify 77 of the 94 occupations in the crafts and trades in our dataset: 44 of

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[8]Even before the ruling of the EU court (see Rs.C-58/98-Josef Corsten, EuZW 2000, 763), the monopoly commission, pushed for a reduction in entry barriers (see e.g. Monopolkommission, 1998).

[9]The proposed law planned to liberalize 64 of the 93 trades leaving only 29 regulated trades offering services that could endanger the health and life of customers. The proposed law passed the Lower House on November 27, 2003 with the support of the governing coalition of Social Democrats and Green Party but faced stiff opposition in the Upper House, which was dominated by the Conservatives. A compromise worked out by a mediation committee adjusted the proposed law along two margins: first, companions could register their own business after 6 years rather than 10 years of working in a trade. The more important change was that 12 trades (bakers, confectioners, butchers, thermal insulation and noise control laggers, well builders, masons and stone cutters, plasterers, painters and lacquers, surgical instrument makers, IT technician, gunsmiths, and hair stylists) were not liberalized as, so the argument, they had invested a lot into training apprentices in the past.

the 53 liberalized occupations and 39 of the 41 still regulated occupations.[10] In addition, we can follow the labor market career of individuals even if they leave their occupation or the crafts and trades sector altogether. We restrict the sample to individuals with at least one job spell in one of the crafts and trades. We further restrict the sample to individuals aged between 16 and 65 who are not employed as an apprentice. We also exclude irregular, marginal and seasonal employment. Finally, we focus on workers that are attached to the labor market, i.e. gainfully employed in the three years prior to the reform, i.e. between 2001 and 2003.

We observe for each individual whether she is employed within the social security system or whether she collects unemployment benefits as of June 30th each year. The wage variable, in contrast, records the average daily wage for the employment spell that contains this reference date.[11] Like most social security data, our wage variable is right-censored at the social security limit. We impute censored wages under the assumption that the error term in the wage regression is normally distributed, allowing for separate variances by district, year and gender. We deflate wages to 2010 prices using the consumer price index. We drop spells with missing or very low wages, i.e. where the daily real wage is below 20 euros. We reweigh wages of part-time spells to convert them into full-time equivalents.[12]

We distinguish three broad skill groups: low-skilled workers enter the labor market without completing high school or an apprenticeship; medium-skilled workers completed either an apprenticeship or graduated from high school. Workers are high-skilled if they graduated from a university or college. Master craftsmen are considered medium-skilled, but can be identified in our data via their occupational status. We use the panel structure to impute missing information on skill levels (Fitzenberger et al., 2006). We also have information about the location of the plant at the state level and the sector of its main activity.

Linked Employer-Employee Data

The individual social security records do not contain information on union coverage, which is an important determinant of employment and wages (Dustmann and Schönberg, 2009). As union coverage might influence how firms in an industry respond to deregulation in the crafts and trades sector, we draw information on union density at

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[10]We cannot classify a few highly specialized crafts and trades (like cutting tool mechanics), which are identifiable only at the 4-digit level.

[11]Because employers are required to update records only at the end of each year, this variable may capture wage changes that occurred from January to December of the same year.

[12]A person is considered full-time employed if she works at least 30 hours per week. Wages of spells with 18-30 working hours per week are divided by 0.6, while wages for those working less than 18 hours are adjusted with a factor of 0.3.

the industry level from a linked employer-employee data set. The matched data combine the IAB Establishment Panel, a large-scale survey of plants, with social security records of all workers who were employed in the surveyed firms as of June 30 each year (see Klosterhuber et al., 2016, for a detailed description). As the IAB establishment panel oversamples large establishments and small states, we employ cross-sectional weights for adjustment.

An establishment can recognize a trade union either by joining an employers' association or through direct negotiations between the firm and the union. Therefore, the union variable in the matched data distinguishes between industry-level agreements, which are negotiated at a regional and industry level, firm-level agreements through direct negotiations between the plant and a union, or no agreement at all. The information on union coverage is available by industry, separately for East and West Germany from 2000 to 2010.

2.3.2 Empirical Strategy

Our empirical strategy combines matching with an event study approach to flexibly compare the evolution of wages and employment in deregulated occupations to those in the regulated occupations. We first describe our matching procedure and then explain our baseline model to track how the liberalization affected wages and employment in the crafts and trades.

Matching Individuals in the Crafts and Trades

An important empirical challenge in analyzing the effects of the 2004 reform is that individuals and their characteristics are likely to differ between the liberalized and the regulated occupations. Table 2.1 shows that, prior to the reform, liberalized occupations are more female, older and less skilled than regulated occupations. Moreover, craftsmen in liberalized occupations work in larger firms and are more prevalent in the producer and consumer good industries than craftsmen in regulated occupations, for instance.

As these characteristics are likely to influence the dynamics of wages and employment, a simple comparison of employees in regulated and liberalized crafts and trades is unlikely to reveal the causal effects of the reform. To identify a valid counterfactual, we match each individual working in a later liberalized occupation in 2003 to several individuals employed in a regulated occupation in 2003. Specifically, we use propensity score matching to predict the probability of working in one of the liberalized occupations in 2003, using the following pre-reform characteristics: age, gender, whether a person is low-, medium- or high-skilled, the number of jobs held, plant size and the broad

Table 2.1 Summary Statistics

	Liberalized Occupations	Regulated Occupations	Control Occupations	Diff. btw. Liberalized and Regulated Occ. Coeff.	S.E.	Diff. btw. Liberalized and Control Occ. Coeff.	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Characteristics Not Matched							
Employment growth (past 3 years)	-0.014	0.003	-0.022	-0.017	0.042	0.008	0.046
Daily real wage	88.217	88.992	88.527	-0.775	7.034	-0.310	7.148
FTE daily real wage	90.508	89.672	89.773	0.836	6.684	0.735	6.839
Log FTE daily real wage	4.442	4.434	4.436	0.007	0.079	0.005	0.079
FTE daily wage growth	0.015	0.013	0.013	0.002	0.005	0.001	0.005
Firm employment	1433.154	1096.099	1164.269	337.055	819.098	268.885	814.767
Part-time	0.068	0.020	0.034	0.048**	0.024	0.034	0.024
East German	0.189	0.223	0.192	-0.034	0.037	-0.003	0.037
Training duration	350.130	500.197	369.791	-150.066***	40.323	-19.660	38.701
Foreign	0.103	0.062	0.097	0.040***	0.012	0.005	0.019
Panel B: Matched Characteristics							
Employees:							
1-4	0.058	0.101	0.064	-0.043	0.026	-0.006	0.024
5-50	0.275	0.499	0.286	-0.224***	0.078	-0.011	0.077
51-250	0.295	0.217	0.312	0.078**	0.030	-0.017	0.033
251-750	0.195	0.078	0.174	0.117***	0.034	0.021	0.034
751-1500	0.077	0.032	0.073	0.045**	0.021	0.004	0.024
>1500	0.100	0.073	0.091	0.028	0.050	0.010	0.056
Age	41.767	39.659	42.050	2.109***	0.495	-0.282	0.457
Female	0.260	0.091	0.229	0.169***	0.062	0.031	0.063
Number of jobs	3.196	3.203	3.126	-0.007	0.162	0.070	0.148
Education:							
Low-skilled	0.191	0.060	0.208	0.131***	0.022	-0.017	0.034
Medium-skilled	0.794	0.920	0.776	-0.126***	0.022	0.018	0.031
High-skilled	0.016	0.020	0.016	-0.005	0.011	-0.001	0.010
Industry:							
Agriculture and Fishing	0.001	0.005	0.001	-0.004*	0.002	0.000	0.001
Energy and Mining	0.006	0.012	0.003	-0.006	0.004	0.003	0.002
Food	0.018	0.045	0.023	-0.027	0.029	-0.005	0.020
Consumer Goods	0.206	0.029	0.232	0.177**	0.081	-0.026	0.117
Producer Goods	0.370	0.052	0.374	0.319**	0.147	-0.004	0.150
Investment Goods	0.105	0.224	0.104	-0.119*	0.061	0.001	0.045
Construction	0.096	0.349	0.111	-0.253**	0.105	-0.016	0.068
Retail Trade	0.058	0.154	0.050	-0.096	0.067	0.009	0.027
Transport and Communications	0.003	0.012	0.003	-0.009**	0.004	0.000	0.001
Finance and Insurance	0.001	0.001	0.001	0.000	0.000	0.000	0.000
Hotel and Restaurant Industry	0.002	0.003	0.002	-0.001	0.001	0.000	0.001
Educational Services	0.004	0.006	0.002	-0.002	0.002	0.002	0.002
Health, Veterinary abd Social Services	0.008	0.011	0.005	-0.004	0.006	0.003	0.004
Corporate Services	0.101	0.056	0.077	0.045	0.070	0.023	0.072
Other Services	0.022	0.043	0.012	-0.021	0.037	0.010	0.015

Notes: Columns (1)-(3) compare means in 2003 for liberalized occupations, regulated occupations and matched control occupations in terms of (Panel A) unmatched characteristics and (Panel B) characteristics we explicitly match on. Columns (4)-(5) report differences between liberalized and regulated occupations and columns (6)-(7) differences between liberalized and matched control occupations. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Individual social security records (2000-2010).

industry structure. Appendix Figure A.1 shows that we get substantial overlap in the propensity score between treated and control group.

Restricting the sample to the common support, we then match each individual working in the liberalized occupations to five individuals in the regulated occupations based on nearest neighbor matching (NN) with replacement.[13] A comparison of characteristics between individuals in the liberalized occupations (shown in column (1) of Table 2.1) and the matched sample of individuals in the regulated occupations (shown in column (3) of Table 2.1) reveals that the matched sample has very similar characteristics. After matching, no observable differences between matched individuals remain (see Columns (6)-(7) of Table 2.1). It is important to stress that the matching procedure does not only eliminate observable differences in the variables we match on (see Panel B of Table 2.1), but also in the characteristics we do not match on (see Panel A of Table 2.1). To assess the quality of our matching, Figure 2.2 shows the standardized bias before matching (shown as dots) and after matching (shown as x).[14] For all variables, the standardized bias declines from 23% before matching to 3% after matching (see also Appendix Table A.2). For 20 out of the 24 variables, the standardized bias is below the benchmark for good covariance balance of 5% (Imbens and Rubin, 2015).

Econometric Model

We then compare labor market outcomes of individuals employed in occupations that got deregulated in 2004 to their matched controls whose occupation remain regulated. In particular, we estimate variants of the following model:

$$
\begin{aligned}
Y_{iost} = \delta \ Lib_{io(in\ 2003)} &+ \sum_{t=2004}^{2010} \beta_t \left(Lib_{io(in\ 2003)} * dYear_t \right) \\
&+ \sum_{t=2000}^{2002} \gamma_t \left(Lib_{io(in\ 2003)} * dYear_t \right) + \alpha_t + \mu_o + \theta_s + \varepsilon_{iost},
\end{aligned}
\tag{2.1}
$$

where the dependent variable Y_{iost} is a labor market outcome for individual i working in occupation o, in state s at time t. $Lib_{i(o\ in\ 2003)}$ is an indicator whether an individual worked in an occupation in 2003, which got liberalized in 2004. The variable is zero for individuals working in an occupation in 2003 that remains regulated after the reform. The variable $dYear_t$ are indicators for calendar years. We focus on three years before

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[13]51% of control cases are used only once to form a match, another 20% are matched twice and 8% three times. Less than 20% of the control cases are used more than three times to form a match. As each match consists of five different control observations, the concern that matches are influenced by a few control observations is unwarranted.

[14]The standardized difference equation before and after matching for all treated t and control occupations c is given by $\hat{\Delta}_{ct}^{before} = \frac{\bar{X}_t - \bar{X}_c}{\sqrt{(s_c^2 + s_t^2)/2}}+$ and $\hat{\Delta}_{ct}^{after} = \frac{\bar{X}_{tM} - \bar{X}_{cM}}{\sqrt{(s_c^2 + s_t^2)/2}}$, where $\bar{X}_c$ and $\bar{X}_t$ denote the sample averages of the covariate values for the control and treated occupations, and s_c^2 and s_t^2 are the within-group sample variances of the covariate (see Imbens and Rubin, 2015, for details).

Fig. 2.2 Balancing Plot before and after Matching
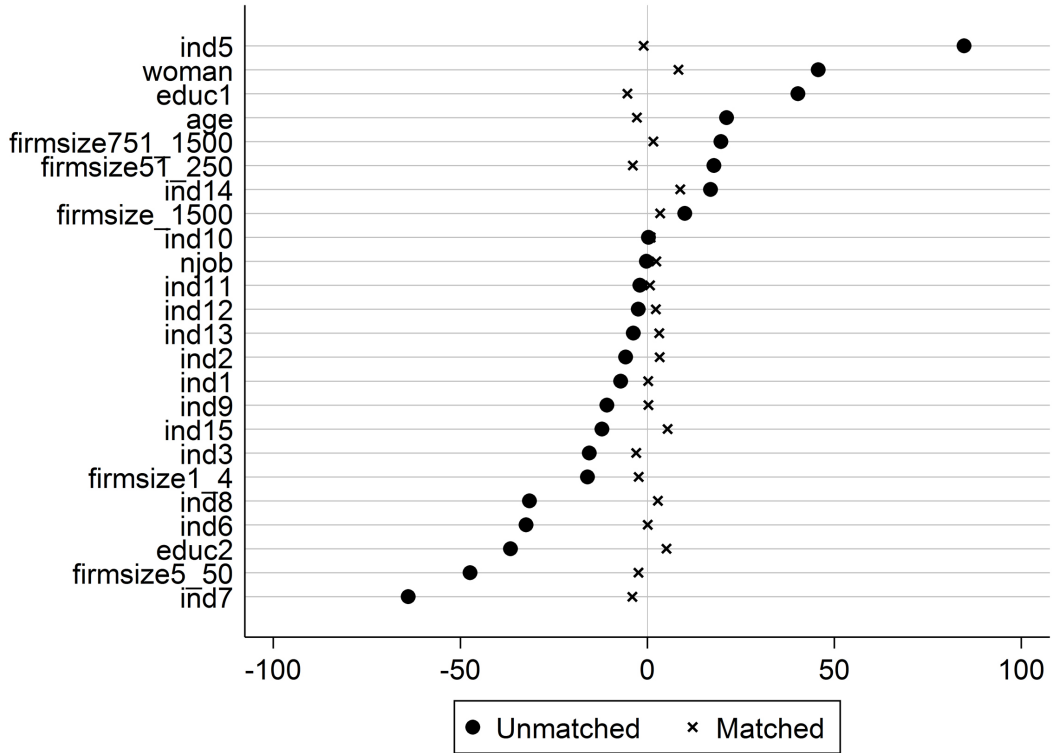
Notes: The figure plots the standardized bias before (shown as dots) and after matching (shown as **x**).

the reform (i.e. 2000 to 2002) and up to seven years after the reform (i.e. from 2004 to 2010) with 2003 as the reference year.

Equation (2.1) controls for year fixed effects (α_t) to account for aggregate shocks and occupation fixed effects (μ_o) to absorb any time-invariant differences in occupations; we further include state fixed effects (θ_s) to adjust for state-level differences in employment and wages. To account for heterogeneity in industry premiums, we also include broad industry fixed effects. In some specifications, we further add industry-specific trends and occupation-specific trends to allow for differential employment or wage dynamics induced by technological change, trade or regulation that affect specific industries or occupations. We cluster standard errors at the occupation level to allow for correlated error terms within occupations over time.

Our key identifying assumption is that outcomes of matched individuals working in the regulated occupations in 2003 form a valid counterfactual for outcomes of individuals working in the occupations that are deregulated after 2004. Since we do not match control occupations based on any outcome variables, a comparison between the treatment and control occupations in the years prior to the deregulation allows us to assess the plausibility of this assumption. If the identification assumption is valid, treatment and control occupations should experience similar trends in Y_{iost} prior to the

deregulation. We show in Section 2.4 that the coefficients γ_{2000} to γ_{2002} in equation (2.1) are indeed close to zero and statistically insignificant. While this condition is neither necessary nor sufficient for the absence of differential trends in the post-reform period, it is commonly used to corroborate the absence of differential pre-trends (Kahn-Lang and Lang, 2019).[15]

The parameters of interest are β_{2004} to β_{2010}, which measure the cumulative effect of the reform on labor market outcomes on incumbent craftsmen in the deregulated occupations relative to the matched control individuals. Recall that we trace the outcomes of incumbent craftsmen irrespective of whether they remain in the deregulated or regulated occupation, leave the labor market, switch occupations or leave the crafts and trades sector altogether.

2.4 Results for Incumbent Craftsmen

2.4.1 Average Wages and Employment

By reducing entry barriers, the reform should open up new business opportunities in the liberalized crafts and trades after 2004 compared to the control occupations. If there is entry into the liberalized occupations after the reform, competition in the liberalized crafts and trades should increase, which in turn puts a downward pressure on wages. Yet, more competition and lower prices could also draw additional consumers into the liberalized market.

We first turn to the effect of deregulation on wages of incumbent craftsmen in liberalized occupations relative to regulated occupations. Figure 2.3 shows the evolution of wages and the 95% confidence interval for three pre-reform and seven post-reform years conditional on year, occupation, industry and state fixed effects. Over the post-reform period, we see little change in average wages of incumbent craftsmen relative to 2003. Table 2.2 traces the effect over alternative specifications: in column (1), we control for year fixed effects, occupation fixed effects, state fixed effects and industry fixed effects. Column (2) adds industry-specific time trends and column (3) occupation-specific trends. Column (3) is the most demanding specification as it accounts for both uptrend or downtrend in occupational wages, but also for differential industry premiums due to the business cycle or secular changes in technology, for instance. Across all specifications, the pre-reform estimates are zero indicating no differential pre-trends in

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[15]Another concern of event study designs like ours is that treatment effects might differ across cohorts of individuals who get treated at different times (Abraham and Sun, 2019). This issue is absent in our setting as we do not stack treatments over time, but control for level differences in outcomes through year fixed effects (α_t).

Fig. 2.3 Effect of Liberalization on Average Wages

our matched sample. The coefficients are not only zero in every single year, they are also not jointly significantly different from zero (see the p-value of the F-test at the bottom of the table).

All post-reform coefficients are small and not statistically significant different from zero in each post-reform year. The p-value of the joint F-test indicates that the post-reform coefficients on wages are also jointly not significant from zero. Hence, wages of the average craftsmen in the deregulated occupations did not decline after the reform relative to wages of craftsmen in the regulated occupations.

One reason we do not find wage effects is that strong unions may keep up wages in the liberalized occupations even as competition increases after the reform. To assess the role played by unions, we merge a measure of union coverage to our matched sample. The strength of unions is measured as the share of employees covered by collective bargaining agreements at the firm or at the industry level and varies by industry, region (East/West) and year. Splitting the sample at the median into high and low union coverage in 2003, we find that employees in regulated occupations are actually more likely to be covered by collective bargaining agreements than employees in liberalized

Table 2.2 Liberalization, Wages and Employment

	Wages			Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized (t=2000)	-0.006	-0.005	-0.003	0.003*	0.003*	0.001
	(0.009)	(0.007)	(0.006)	(0.002)	(0.002)	(0.002)
Liberalized (t=2001)	-0.005	-0.005	-0.003	0.002*	0.002**	0.001
	(0.008)	(0.006)	(0.006)	(0.001)	(0.001)	(0.002)
Liberalized (t=2002)	-0.002	-0.001	-0.001	0.001	0.001	0.000
	(0.005)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)
Liberalized (t=2003)						
Liberalized (Reform year)	-0.003	-0.003	-0.003	0.001	0.001	0.002
	(0.006)	(0.006)	(0.005)	(0.001)	(0.001)	(0.001)
Liberalized (t=2005)	0.011	0.010	0.009	-0.002	-0.002	-0.000
	(0.011)	(0.010)	(0.008)	(0.002)	(0.002)	(0.002)
Liberalized (t=2006)	0.005	0.004	0.002	-0.003	-0.003	-0.000
	(0.012)	(0.009)	(0.007)	(0.002)	(0.001)	(0.003)
Liberalized (t=2007)	0.007	0.005	0.002	-0.001	-0.001	0.001
	(0.016)	(0.012)	(0.008)	(0.003)	(0.002)	(0.004)
Liberalized (t=2008)	0.012	0.01	0.006	-0.001	-0.001	0.002
	(0.018)	(0.013)	(0.009)	(0.003)	(0.003)	(0.005)
Liberalized (t=2009)	0.011	0.008	0.004	0.002	0.002	0.006
	(0.019)	(0.014)	(0.009)	(0.004)	(0.003)	(0.006)
Liberalized (t=2010)	0.020	0.017	0.011	0.003	0.003	0.006
	(0.022)	(0.015)	(0.011)	(0.003)	(0.003)	(0.006)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	-	Yes	Yes	-	Yes	Yes
Occupation-specific trend	-	-	Yes	-	-	Yes
Observations	207,465	207,465	207,465	188,371	188,371	188,371
R-squared	0.428	0.429	0.430	0.087	0.087	0.087
F-test pre-trends	0.851	0.858	0.916	0.174	0.220	0.986
F-test joint reform effect	0.178	0.188	0.174	0.000	0.000	0.000

Notes: The table reports estimates of deregulation on workers in the crafts and trades based on equation (2.1). The dependent variable in columns (1)-(3) is the log daily fte-adjusted daily real wage (in 2010 prices). In columns (4)-(6), the dependent variable is employment, which is equal to one if the person is employed full-time and equal to 0.6 or 0.3 if the person is employed part-time (18-30 hours or under 18 hours per week respectively). Employment is zero if the person is unemployed, out of the labor force or employed outside the social security system. The reference year is 2003. We include only workers who are attached to the labor market, i.e. those who are employed for three years prior to the reform. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5%** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

occupations. Among liberalized occupations, 54 percent of employees work in sectors with high union coverage in 2003; the share is with 63 percent even higher for employees in regulated occupations. The same pattern holds if we focus on collective bargaining agreements at the industry level only, which are often considered more powerful than firm-level agreements. Here, only 7 percent of employees in liberalized occupations, but 18 percent of employees in regulated occupations work in industries with high union coverage prior to the reform.

Hence, unions are actually more prevalent in the occupations that remained regulated − contrary to our expectation. Yet, wages could then decline after the reform in those sectors with low union coverage. To test this, we rerun our model in equation (2.1) but restrict the sample to employees working in industries with low union coverage. Appendix Table A.3 shows that wages in the liberalized occupations with low union coverage at the industry or firm level (see columns (3) and (4)) exhibit a slight downward trend after the reform. Yet, the coefficients do not reach statistical significance. The same pattern holds if we restrict the sample to employees with low-industry level coverage. Overall then, differences in union coverage or wage-setting cannot explain why average wages remain unchanged.

We next turn to the question whether individuals who were employed in 2001 to 2003 and worked in one of the liberalized occupations in 2003 face worse employment opportunities after the reform than individuals in the control occupations. The dependent variable is now employment, which is equal to one if a person is employed full-time, 0.3 or 0.6 if she is employed part-time (below 18 hours or 18 to 30 hours respectively); and 0 if she is unemployed, outside the labor force or works outside the social security system. Using the model from equation (2.1) and the same specifications as before, columns (4)-(6) of Table 2.2 shows the results.

We see few changes in employment in the liberalized occupations after the reform. Incumbent craftsmen in the liberalized trades do not leave employment, nor do they move to part-time employment at higher rates than craftsmen in the regulated occupations after the reform. Conditional on occupation-specific and industry-specific trends, there is, if anything, a slight positive effect on employment (see column (6)). While none of the coefficients reach statistical significance individually, they are jointly significant (see the p-value of the F-test in the last row). Yet, the effect is economically very small. Taken together, the reform did not harm incumbent craftsmen by lowering their employment opportunities or reducing earnings in the liberalized occupations.

Fig. 2.4 Liberalization and Wages of Master Craftsmen
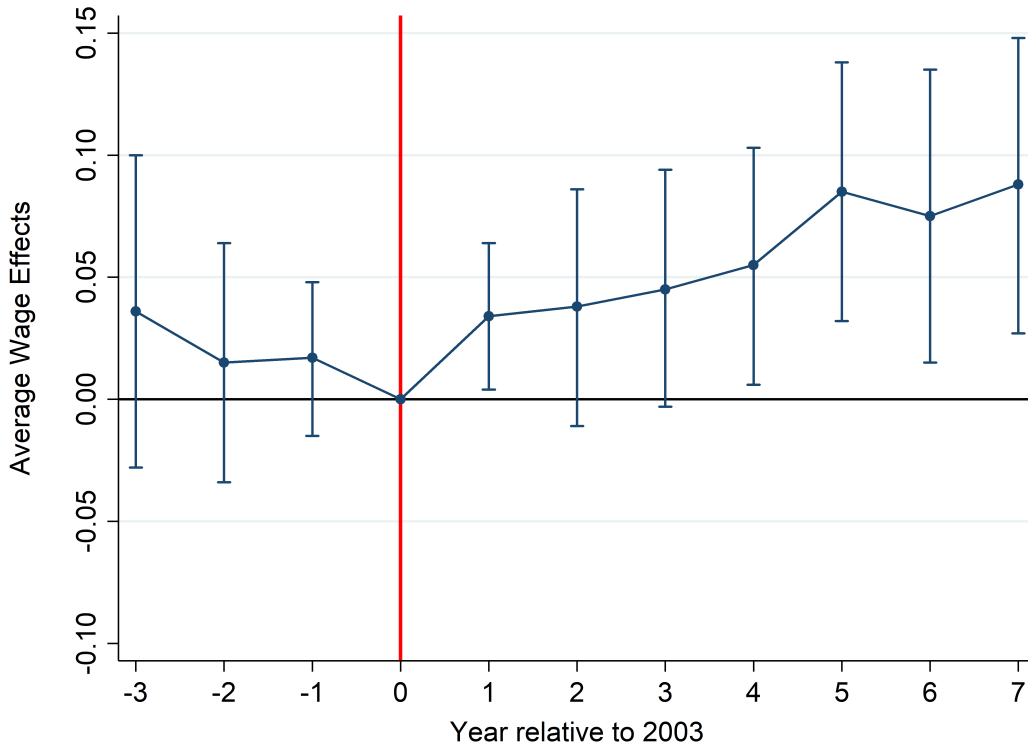
Notes: The figure plots estimates and the 95% confidence interval of the effect of deregulation on the wages of Master craftsmen in the liberalized trades based on equation (2.1) after controlling for year, occupation, industry and state fixed effects. The dependent variable is the log fte-adjusted daily real wage (in 2010 prices). The reference year is 2003. The sample is restricted to Master craftsmen who are attached to the labor market, i.e. employed for three years prior to the reform. Standard errors are clustered at the occupation level.
Source: Matched sample, individual social security records (2000-2010).

2.4.2   Wages of Master Craftsmen

Even in the absence of an average effect, the reform could have changed the wage structure in the liberalized trades for two reasons. First, the deregulation is likely to increase competition by low-skilled craftsmen (without a completed apprenticeship, for instance). These new entrants might be more of a substitute for incumbent craftsmen with less than a Master craftsmen diploma than the average employee in our sample. A second channel how the reform could have shifted the wage structure is that tougher competition, together with a declining supply of Master craftsmen, could push up the skill premium for craftsmen with a Master diploma in the liberalized occupations.

To investigate how wages change after the reform, we rerun our basic model in equation (2.1) but restrict the sample to Master craftsmen. Figure 2.4 shows that wages of Master craftsmen in the regulated and deregulated occupations evolve similarly prior to the reform. After the reform, the wages of Master craftsmen in the deregulated occupations increase continuously. The estimates (in column (1) of Table 2.3 show that

Table 2.3 Liberalization and Wages of Master and Low-skilled Craftsmen

| | Wages of Master Craftsmen | | | Wages of Low-skilled Workers | | |
| | (with a Master Craftsman Diploma) | | | (without completed Apprenticeship) | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized (t=2000)	0.036	0.037	0.046	-0.025*	-0.024*	-0.020
	(0.032)	(0.030)	(0.030)	(0.014)	(0.014)	(0.015)
Liberalized (t=2001)	0.015	0.016	0.024	-0.015	-0.014	-0.011
	(0.025)	(0.023)	(0.023)	(0.009)	(0.009)	(0.010)
Liberalized (t=2002)	0.017	0.016	0.021	0.000	0.001	0.001
	(0.016)	(0.015)	(0.015)	(0.006)	(0.006)	(0.006)
Liberalized (t=2003)						
Liberalized (Reform year)	0.034**	0.035**	0.030*	-0.011	-0.011	-0.011
	(0.015)	(0.016)	(0.016)	(0.012)	(0.012)	(0.012)
Liberalized (t=2005)	0.038	0.038*	0.029	0.003	0.002	0.001
	(0.024)	(0.023)	(0.022)	(0.016)	(0.016)	(0.016)
Liberalized (t=2006)	0.045*	0.045**	0.033	-0.017	-0.019	-0.020
	(0.025)	(0.022)	(0.020)	(0.017)	(0.017)	(0.017)
Liberalized (t=2007)	0.055**	0.054**	0.038	-0.011	-0.013	-0.014
	(0.024)	(0.023)	(0.023)	(0.017)	(0.017)	(0.019)
Liberalized (t=2008)	0.085***	0.085***	0.071***	-0.013	-0.016	-0.018
	(0.027)	(0.024)	(0.026)	(0.020)	(0.020)	(0.021)
Liberalized (t=2009)	0.075**	0.072**	0.053	-0.012	-0.015	-0.017
	(0.031)	(0.028)	(0.034)	(0.022)	(0.022)	(0.024)
Liberalized (t=2010)	0.088***	0.082***	0.059	-0.005	-0.008	-0.010
	(0.031)	(0.031)	(0.039)	(0.021)	(0.022)	(0.025)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	-	Yes	Yes	-	Yes	Yes
Occupation-specific trend	-	-	Yes	-	-	Yes
Observations	8,224	8,224	8,224	26,053	26,053	26,053
R-squared	0.507	0.509	0.516	0.391	0.391	0.396

Notes: The table reports estimates of deregulation on Master craftsmen (in columns (1)-(3)) and low-skilled workers (in columns (4)-(6)) in the crafts and trades based on equation (2.1). The dependent variable in all specifications is the log daily fte-adjusted daily real wage (in 2010 prices). The reference year is 2003. All specifications include year, occupation, state and broad industry fixed effects. Columns (2) and (5) further add industry-specific trends; columns (3) and (6) further control for occupation-specific trends. We include only workers who are attached to the labor market, i.e. those who are employed for three years prior to the reform. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

in the reform year, wages are 3.4% higher relative to Master craftsmen in the matched control sample. In 2010 or six years later, wages for Master craftsmen have increased by 8.8% compared to wages of Master craftsmen in the control trades since 2003. These

results are robust to including industry-specific trends (see column (2) of Table 2.3) and even occupation-specific trends (in column (3) of Table 2.3).

Do we see the opposite pattern of declining wages for low-skilled craftsmen in the liberalized occupations? The right-hand side of Table 2.3 reruns the same specifications for the sample of employees without apprenticeship training or Master diploma. While there is a slight negative trend in wages, the coefficients do not reach statistical significance at conventional levels. Do all incumbent Master craftsmen benefit from the removal of entry requirements through higher wages? Appendix Table A.4 shows that the rise in the skill premium is concentrated among prime-aged Master craftsmen (see columns (1) and (2)) and are very similar if we restrict our sample to men (see column (3)).

The rise in the Master premium is highly robust. Restricting the sample to individuals working as Master craftsmen in their current job might selectively drop craftsmen with a Master diploma, but who work in a job that does not require (and hence, not report) a Master diploma. If these job switchers are less productive than other Master craftsmen, our estimates of the skill premium would be biased upward. To address this concern, we include all individuals in our sample of Master craftsmen who ever worked or will work in a job requiring a Master craftsmen. This definition also includes companions who would pass the Master examination in a later year. Column (4) of Table A.4 in the Appendix shows that the estimates for this broader Master craftsmen sample are very similar to the baseline estimates in Table 2.3. As such, selective attrition of low-productive Master craftsmen from the liberalized trades cannot explain the rising skill premium. Our sample of Master craftsmen also includes foremen in the construction sector, which are not Master craftsmen as defined by the Crafts and Trades Code. Dropping the construction sector, column (5) shows that the results remain unchanged. Hence, the higher skill premium for Master craftsmen is not an artifact of the broader coding category in the construction sector.

The rising skill premiums cannot be explained by Master craftsmen in the deregulated occupations moving from smaller to larger firms after the reform. Column (6) of Appendix Table A.4 shows that the skill premium increases even conditional on firm size fixed effects and controlling for differential firm size distributions through industry fixed effects and industry-specific trends. The skill premium might also emerge because of skill-biased shocks to industry demand or technology. To rule out this concern, we include in column (7) of Table A.4 detailed industry fixed effects and industry-specific trends at the 2-digit level. This demanding specification reduces the precision

of our estimates, but the broad pattern of a continuously rising skill premium in the post-reform period remains the same as in the baseline.[16]

Another concern is that our estimation strategy does not account for the estimation error in the matching step (Abadie and Spiess, 2019).[17] To test the robustness of our matching estimator and the inference based on clustered standard errors, we implement a kernel matching estimator. The kernel matching estimator has two advantages: first, we can use more control observations to construct a match (based on an Epanechnikov kernel); in addition, we can obtain valid standard errors by implementing a bootstrap procedure. Holding the set of matching variables constant, we implement a wild bootstrap procedure to account for the estimation error in the matching step (Cameron et al., 2008). Appendix Figure A.2 shows that the kernel matching estimator, like the nearest neighbor matching step, reduces the standardized bias substantially.[18] Appendix Table A.9 reports the estimates based on a kernel matching estimator for wages of Master craftsmen (in column (1)), average wages (in column (3)) and employment (in column (5)). We find the same pattern as with the nearest neighbor matching estimator: no effect on average wages and employment, but an increase in the wages of Master craftsmen. The second specification reports the bootstrapped confidence intervals for the three outcome variables (in columns (2), (4) and (6)). The bootstrapped standard errors confirm our main conclusions: there is no statistically significant effect on average wages and employment, but a pronounced and statistically highly significant rise in the skill premium for Master craftsmen.

Overall then, incumbent Master craftsmen actually benefit from the removal of entry requirements. Their higher wages can be rationalized by two channels, one on the supply and one on the demand side. After removing the entry requirement of a Master craftsman diploma, fewer individuals now obtain a Master craftsman degree in the deregulated occupations (Lembcke, 2019). If consumers are willing to pay a premium for services by a Master craftsman, the skill premium increases. A second explanation comes from the demand side through an increase in competition in the product market.

---

[16]Further, the skill premium for Master craftsmen is not driven by the introduction of minimum wages in 1997 (see columns (1)-(4) in Table A.5 in the Appendix) and in 2007 (see column (5)). Column (6) shows that the results of the wage premium for Master craftsmen remain unchanged when jointly excluding all minimum wage occupations.

[17]Using a bootstrap procedure often results in invalid standard errors (Abadie and Imbens, 2008). Ignoring the matching step results in consistent standard errors only if matching without replacement is used in the first step and the regression model in the second step is correctly specified (Abadie and Spiess, 2019).

[18]Appendix Table A.8 further demonstrates that observable characteristics are well balanced (see columns (6)-(7)).

Tougher competition in the deregulated occupations makes skills more valuable if these reduce the marginal costs of production (Boone, 2008; Guadalupe, 2007).[19]

2.4.3   Job and Occupational Mobility of Incumbents

In response to the liberalization, incumbent craftsmen might be more likely to take advantages of new job opportunities in the liberalized crafts and trades. As a result, we might see more job mobility within the liberalized occupations. Alternatively, we might see more occupational mobility as craftsmen switch from the regulated to the liberalized occupations to take advantage of the lowered entry barriers, for example.

Fig. 2.5 Liberalization and Mobility of Incumbent Craftsmen
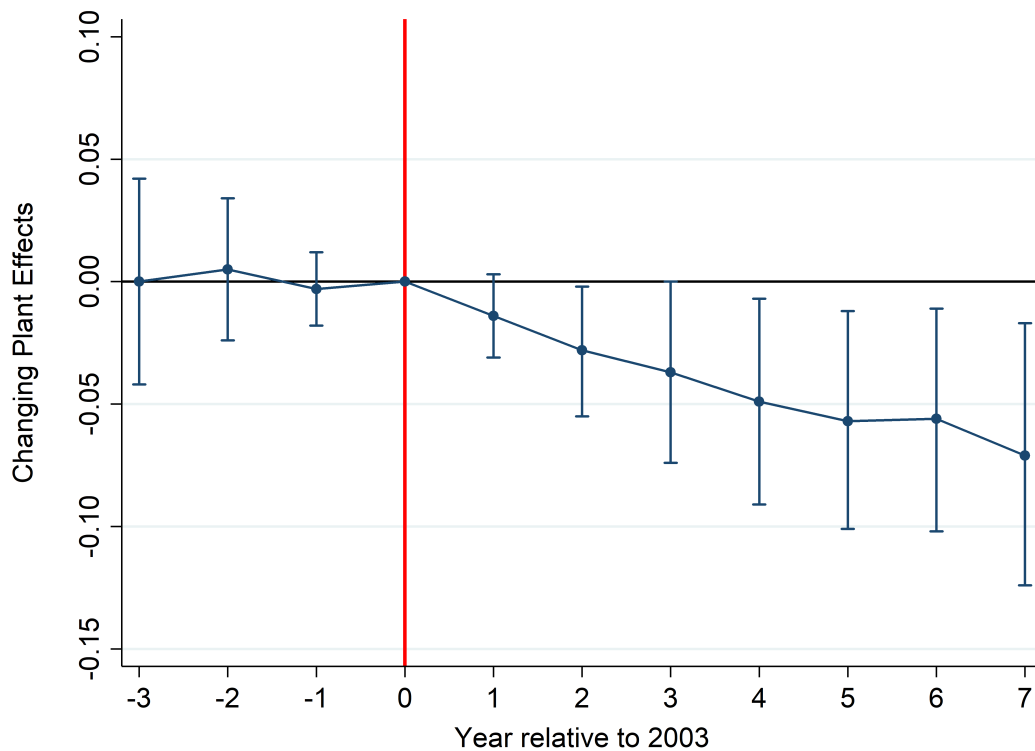
Notes: The figure plots estimates and 95% confidence interval of the effect of deregulation on the probability of a plant change (relative to 2003) in the liberalized trades based on equation (2.1), after controlling for occupation, year, state and industry fixed effects as well as occupations-specific and industry-specific trends. The sample is restricted to incumbent craftsmen who are attached to the labor market, i.e. employed in the three years prior to the reform. Standard errors are clustered at the occupation level.
Source: Matched sample, individual social security records (2000-2010).

To study how liberalization affects the mobility of incumbent craftsmen, we estimate variants of regression (2.1), where Y_{ijost} is now the cumulative probability of switching

---

[19]In a market with licensing (the Master craftsman diploma is an entry requirement), the wage differential between skilled and less skilled workers is compressed. In a market with certification (where Master craftsman diplomas act as a signal), the wage differences between high- and less-skilled workers increases (Shapiro, 1986). The 2004 reform, which abolished licensing and adopted certification in the deregulated crafts and trades, may thus lead to an increase in the price of Master craftsmen.

Fig. 2.6 Liberalization and Occupational Mobility of Incumbent Craftsmen
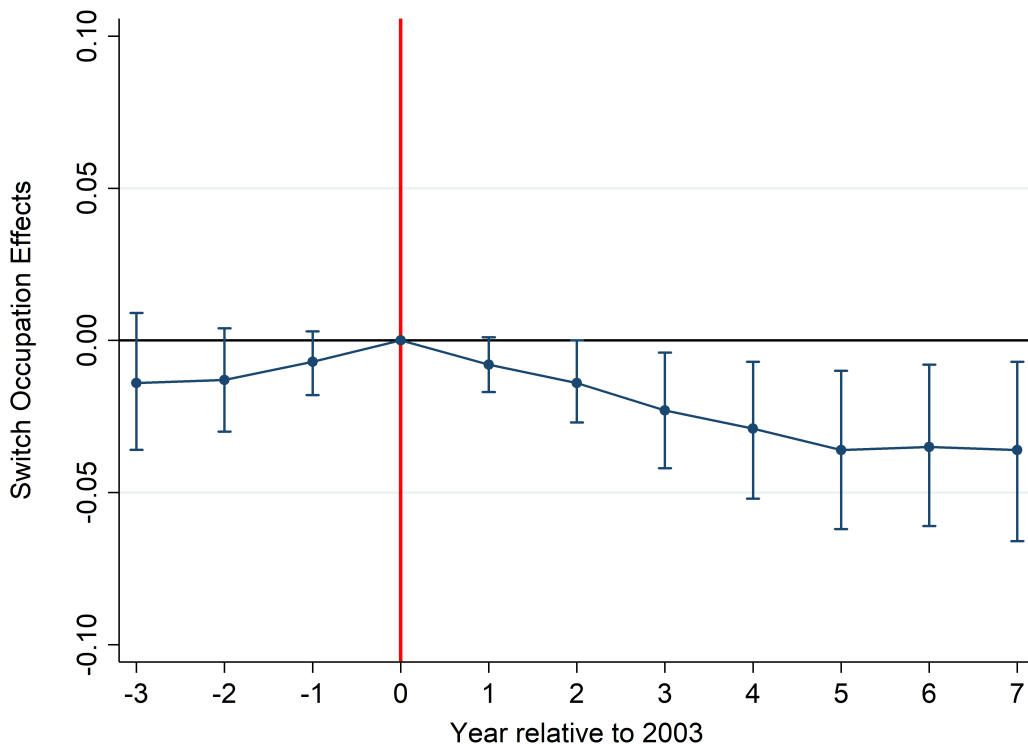
Notes: The figure plots estimates and 95% confidence interval of the effect of deregulation on the probability of switching the occupation (relative to 2003) in the liberalized trades based on equation (2.1) after controlling for occupation, year, state and industry fixed effects. The sample is restricted to incumbent craftsmen who are attached to the labor market, i.e. employed in the three years prior to the reform. Standard errors are clustered at the occupation level.
Source: Matched sample, individual social security records (2000-2010).

employers or switching occupations between the pre-reform year (2003) and the post-reform years. Figure 2.5 plots how the reform affects job mobility and Figure 2.6 shows the impact on the likelihood of switching occupations. Craftsmen in the liberalized occupations are consistently less likely to switch jobs or occupations after the reform than craftsmen in the regulated occupations. The estimates in Table 2.4 indicate that craftsmen in deregulated occupations are 7.1 percentage points less likely to switch employers seven years after the reform (see column (2) of Table 2.4). Furthermore, we also find that they are 6.1 percentage points less likely to switch occupations (see column (4) of Table 2.4) and 9.8 percentage points less likely to switch their 2-digit industry (see column (6) of Table 2.4) than their peers in the regulated occupations. The decline in occupational and industry mobility does not suggest that incumbent craftsmen get displaced from their jobs after the liberalization. On the contrary, the evidence suggests that jobs in the liberalized occupations might have become more attractive as they are more accessible to craftsmen from the regulated occupations, but also because of the higher wages for Master craftsmen.

Table 2.4 Liberalization and Mobility of Incumbent Craftsmen

	Job Mobility		Occupational Mobility		Industry Mobility	
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized (t=2000)	-0.013	0.000	-0.008	-0.004	-0.018	-0.004
	(0.018)	(0.021)	(0.012)	(0.013)	(0.012)	(0.016)
Liberalized (t=2001)	-0.004	0.005	-0.009	-0.006	-0.014	-0.005
	(0.013)	(0.015)	(0.009)	(0.010)	(0.009)	(0.011)
Liberalized (t=2002)	-0.008	-0.003	-0.005	-0.004	-0.013***	-0.008
	(0.007)	(0.008)	(0.006)	(0.006)	(0.005)	(0.006)
Liberalized (t=2003)						
Liberalized (Reform year)	-0.008	-0.014	-0.010**	-0.012***	-0.014***	-0.020***
	(0.009)	(0.009)	(0.005)	(0.004)	(0.004)	(0.004)
Liberalized (t=2005)	-0.017	-0.028**	-0.018**	-0.022***	-0.023***	-0.035***
	(0.014)	(0.013)	(0.007)	(0.007)	(0.006)	(0.008)
Liberalized (t=2006)	-0.020	-0.037*	-0.030***	-0.035***	-0.036***	-0.052***
	(0.020)	(0.019)	(0.011)	(0.010)	(0.008)	(0.011)
Liberalized (t=2007)	-0.028	-0.049**	-0.038***	-0.045***	-0.045***	-0.065***
	(0.022)	(0.021)	(0.013)	(0.013)	(0.011)	(0.014)
Liberalized (t=2008)	-0.031	-0.057**	-0.047***	-0.055***	-0.052***	-0.076***
	(0.022)	(0.023)	(0.016)	(0.016)	(0.012)	(0.016)
Liberalized (t=2009)	-0.026	-0.056**	-0.048***	-0.057***	-0.057***	-0.086***
	(0.022)	(0.023)	(0.017)	(0.017)	(0.013)	(0.018)
Liberalized (t=2010)	-0.037	-0.071***	-0.051***	-0.061***	-0.066***	-0.098***
	(0.026)	(0.027)	(0.020)	(0.020)	(0.014)	(0.020)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	-	Yes	-	Yes	-	Yes
Occupation-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	207,518	207,518	207,518	207,518	207,518	207,518
R-squared	0.262	0.265	0.796	0.797	0.370	0.377
Mean (prior Reform)	0.137	0.137	0.079	0.079	0.071	0.071

Notes: The table reports estimates of deregulation on the mobility of incumbent workers in the crafts and trades based on equation (2.1). The dependent variable in columns (1)-(2) is the probability of switching plants; in columns (3)-(4) the probability of switching the occupation and in columns (5)-(6) the probability of switching industries at the 2-digit level. The reference year is 2003. All specifications include year, occupation, state and broad industry fixed effects as well as occupation-specific trends. The second specification (in columns (2) and (4)) further add industry-specific trends. The sample is restricted to workers who are attached to the labor market, i.e. those who are employed for three years prior to the reform. Standard errors are clustered at the occupation level. Weighted means for the pre Reform period are given below. Significance levels: 1% ***, 5% ** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

2.5 Liberalization, Entry and Self-Employment

Our analysis so far has followed craftsmen who worked in the liberalized or regulated crafts and trades prior to the reform. As such, our results could not shed light on the question whether the liberalization encouraged entry and resulted in more competition in

the liberalized crafts and trades. Furthermore, the absence of a wage effect for incumbent craftsmen does not necessarily imply that wages in the deregulated occupations remain unchanged as wages of entrants into the liberalized occupations might have declined after the reform.

We first turn to the stock of craftsmen (incumbents and new entrants) to study the overall effect of liberalization on wages, employment and mobility in the crafts and trades. Then, we use plant-level information to investigate entry and exit in the crafts and trades. In a third step, we use survey data to analyze self-employment, which is not covered by the social security data.

2.5.1 Overall Average Wages, Employment and Mobility

To study the effect of liberalization on average wages, employment and the mobility of entrants, we restrict our sample to all craftsmen who work in a liberalized or regulated occupation in a given year. Thereby, we include incumbent workers, who were employed and worked in a crafts and trades occupation before 2004, and new workers who entered a liberalized or regulated occupation in 2004 or later. In the absence of average labor market effects for incumbent craftsmen, restricting the sample to all craftsmen should reveal how labor market outcomes of entrants are affected by the reform. If entry in the liberalized trades increases after the reform, wages for new entrants could potentially decline. At the same time, entrants may be more likely to make use of new employment opportunities in the liberalized trades.

Empirical Approach

To analyze how overall wages, employment and mobility change after the reform, we run versions of the following model:

$$
\begin{aligned}
Y_{i(o)jst} = {} & \delta\ Lib_{i(o)t} + \sum_{t=2004}^{2010} \beta_t \left(Lib_{i(o)t} * dYear_t\right) + \sum_{t=2000}^{2002} \gamma_t \left(Lib_{i(o)t} * dYear_t\right) \\
& + \alpha_t + \mu_o + \nu_j + \theta_s + \varepsilon_{i(o)jst},
\end{aligned}
\tag{2.2}
$$

where $Y_{i(o)jst}$ is an outcome of interest for individual i, working in occupation o, industry j, state s and at time t. Labor market outcomes include the employment probability and average wages at the individual level. $Lib_{i(o)t}$ is an indicator whether a person is working in an occupation in year t that got liberalized in 2004. All specifications include year (α_t), occupation (μ_o), state (θ_s), broad industry (ν_j) fixed effects and industry-specific trends. We cluster standard errors at the occupation level.

Results on Overall Average Wages, Employment and Mobility

To study the effect of deregulation on wages and employment for entrants in the liberalized occupations relative to the regulated occupations, we estimate regression (2.2) on the overall sample. Lower entry barriers should encourage entry into the deregulated crafts, and consequently, put downward pressure on wages for entrants in the liberalized trades. As such, we might expect a decline in average wages for all craftsmen in the absence of average wage effects for incumbents.

The results in column (1) of Table 2.5 suggest few changes in terms of average wages in the liberalized trades. Incumbents and new entrants in the liberalized crafts and trades do not earn lower wages after the reform, if anything, there is a slight positive effect after the reform. Including further broad industry-specific trends and occupation-specific trends, column (3) shows a slight negative reform effect. Yet, the effect is economically very small. Given that lower entry barriers should induce entry, one explanation for the absence of average wage effects for entrants and incumbents is that the demand for craftsmen has shifted outward after the reform.

We next turn to the question whether individuals face new employment opportunities in the liberalized crafts, or fare worse compared to craftsmen in the regulated trades. Column (4) to (6) of Table 2.5 show that overall employment opportunities have worsened shortly after the reform for craftsmen in the liberalized trades compared to the regulated trades; the reform estimates are jointly significant. Yet, evaluated at the pre-reform mean the employment effect of -0.003 amounts to a 0.4% lower likelihood of employment, which is economically very small.

In terms of mobility, entrants might be more flexible with regard to job, occupation or industry choice given the lower entry barriers in the deregulated trades. To investigate this, Appendix Table A.6 shows the mobility results when running a difference-in-differences analysis. Column (1) and (2) suggest that the incumbent effect of job mobility is dominating (see Table 2.4). We further observe no effect on the likelihood that more workers change the occupation (see columns (3) and (4) in Table A.6), nor that the likelihood of changing industries increases (see columns (5) to (6)). Overall, we observe less craftsmen to leave their industry and observe no effect on the likelihood of changing the occupation.

These results do not suggest that entrants have faced worse wage opportunities in the deregulated trades compared to the regulated trades. On the contrary, the evidence on the absence of average wage effects suggests that the demand for craftsman has increased. In terms of employment and mobility, our individual-level analysis on the overall sample could not fully answer what happened to entry in the liberalized trades.

Table 2.5 Liberalization, overall Wages and Employment

	Wages			Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized (t=2000)	-0.003	0.000	0.007	0.003*	0.003	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Liberalized (t=2001)	-0.006	-0.003	0.001	0.003**	0.002	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Liberalized (t=2002)	-0.004	-0.003	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Liberalized (t=2003)						
Liberalized (Reform year)	-0.002	-0.003	-0.005*	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Liberalized (t=2005)	0.012	0.010	0.005	-0.003*	-0.003*	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Liberalized (t=2006)	0.014	0.011*	0.004	-0.003**	-0.003**	-0.003**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Liberalized (t=2007)	0.021	0.017*	0.007	-0.003	-0.002	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Liberalized (t=2008)	0.019	0.014	0.001	-0.003	-0.002	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Liberalized (t=2009)	0.012	0.006	-0.010	-0.001	0.000	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Liberalized (t=2010)	0.025	0.017	-0.002	0.000	0.000	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	-	Yes	Yes	-	Yes	Yes
Occupation-specific trend	-	-	Yes	-	-	Yes
Observations	418,642	418,642	418,642	418,936	418,936	418,936
R-squared	0.476	0.477	0.477	0.079	0.079	0.079
F-test pre-trends	0.469	0.331	0.100	0.165	0.395	0.283
F-test joint reform effect	0.000	0.000	0.000	0.000	0.000	0.000

Notes: The table reports estimates of deregulation on wages and employment of incumbent workers and new entrants in the crafts and trades based on equation (1). The dependent variable in columns (1)-(3) is the natural log of fte-adjusted daily real wages (in 2010 prices). The dependent variable in columns (4)-(6) is the probability of fte-adjusted employment. The treatment indicator is time-varying. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Individual social security records, overall sample (2000-2010).

To analyze entry and exit after liberalization, we next turn to entry and exit at the plant level.

2.5.2 Entry and Exit at the Plant Level

Data and Empirical Approach

To study entry and exit, we use administrative data on the universe of plants with at least one employee subject to social security contributions over the 2000-2010 period.[20] Our data is a 50 percent sample of all plants in Germany. We observe the industry of each plant at the 4-digit level, which we use to identify plants in the crafts and trades. Overall, we can identify 66 of the 94 occupations in the crafts and trades sector: 34 of the 53 liberalized occupations and 32 of the 41 occupations that remain regulated. Our data contain $80,269$ plants in the liberalized crafts and trades and $161,377$ plants in the still regulated trades.[21]

As dependent variables, we study total employment in the plant as well as the total number of inflows and outflows of workers at the plant level. Specifically, inflows count the number of employees in a plant as of June 30 in year t, who were not employed in that plant as of June 30 in year $t - 1$; outflows count all employees who have been working in a plant as of June 30 of year $t - 1$, but have left the plant by June 30 of year t. Finally, we study the opening and closing of plants. Plant openings are defined as a new plant id combined with worker inflows that cannot be accounted for by spin-offs, mergers or takeovers (following the methodology in Hethey and Schmieder, 2010). Plant closings are defined correspondingly as a vanishing plant id with corresponding worker outflows not accounted for by spin-offs, mergers or takeovers.

To analyze entry and exit in the crafts and trades sector, we run versions of the following model:

$$
Y_{fjst} = \delta \ Lib_j + \sum_{t=2004}^{2010} \beta_t \left(Lib_j * dYear_t \right) + \sum_{t=2000}^{2002} \gamma_t \left(Lib_j * dYear_t \right)
$$
$$
+ \ \alpha_t + \nu_j + \theta_s + \varepsilon_{fjst}, \tag{2.3}
$$

where Y_{fjst} are worker flows into and out of the plant. A plant is affected by the reform $(Lib_j = 1)$ if the plant operates in an industry belonging to the crafts and trades sector.

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[20]Our data does not distinguish between plant and firm; as such, we use the two terms interchangeably.

[21]The treatment assignment is based on a walkover "*Wirtschaftszweigklassifikation - Handwerk*" (ZDH, 2016). We cannot identify the following liberalized occupations at the industry level: tile and mosaic setter (layer), flooring plasterer, metal and bell founder, cutting tool mechanic, parquet layer, cooper, textile artisan, milliner, weaver, sail maker, cellar-man, chandler, building cleaner, precision optician, glass and ceramic painter, media designer for flexography, hand-drawn instrument maker, bow maker and gilder. From the still regulated occupations we cannot identify heat, low-temperature and noise insulation worker, stonemason and sculptor, chimney sweep, surgical instrument maker, installer and heating fitter, electro technician, rope maker, optometrist (optician) and dental technician at the industry level.

All specifications include year (α_t), state (θ_s) and broad industry (ν_j) fixed effects. In some specifications, we also include industry-specific trends. As the treatment variable is now defined at the industry level, we cluster standard errors at the 4-digit industry level.

Results on Plant Entry and Exit

The deregulation should encourage entry into the liberalized crafts and trades. Column (1) of Table A.7 in the Appendix shows the results for plant openings. The dependent variables is a binary indicator equal to one if a plant opening occurs in a given year; and zero otherwise. There is little response immediately after the reform; in the longer run, we see more plant openings in the liberalized crafts and trades, however.

Column (2) studies plant closings. On the one hand, lower entry barriers could encourage ownership changes by craftsmen without a Master craftsman diploma. As a result, plant closings might actually decline after the reform. On the other hand, tougher competition through entry might actually spur more plant closings after the reform. The results suggest that fewer plants exit the market in the liberalized crafts and trades after the reform, supporting the first argument. The effect points to a decline in plant closings of 0.5 percentage points in the post-reform period or about 12% ($-0.005/0.042$) relative to the pre-reform mean.

The new business opportunities could also be met by employment growth in existing plants. To check this channel, we use plant employment (in logs) as dependent variable. Note that employment in a plant (e.g. a bakery), includes both employed craftsmen (e.g. the baker) and employees in other occupations (e.g. the service personnel or accountant). The results in column (3) of Table A.7 in the Appendix suggest no change in plant size in the liberalized crafts and trades after the reform.

Finally, we analyze worker inflows and outflows at the plant-level. Column (4) in Table A.7 in the Appendix shows that inflows into plants operating in the liberalized occupations do not seem to change much after the reform. Yet, worker outflows decline in plants operating in the liberalized trades shortly after the reform (see column (5) of Table A.7 in the Appendix), but outflows of workers from plants in the liberalized trades go up during the economic crisis of 2008-2009 relative to the regulated trades. How can the higher outflows at the plant level be reconciled with the consistently lower job mobility by the individual craftsmen in the liberalized occupations (see columns (1) and (2) of Table 2.4)? The main difference is that Table 2.4 covers craftsmen only, while Appendix Table A.7 covers all employees of a plant operating in the crafts and trades. A higher outflow among all employees is consistent with a lower mobility among craftsmen if employees other than craftsmen are more likely to leave a plant in the crafts and

trades after the reform than in the regulated trades. In addition, the evidence in Table 2.4 covers incumbents who were employed in the crafts and trades in 2003; the evidence in Table A.7, in turn, covers all employees (craftsmen and others) independently of whether they have worked in the crafts and trades before or after the reform.

2.5.3 Liberalization and Self-Employment

Entry into the liberalized occupations might not only occur from other social security jobs or un- or non-employment. The reform might have encouraged self-employment among craftsmen who, after 2004, could open their own business without having a Master craftsmen degree.

Data and Empirical Approach

To study self-employment, we turn to the Microcensus, a large population survey with information on both self-employed and employed individuals. The Microcensus is an annual, cross-sectional survey of 1% of the population and contains detailed information on occupations and self-employment. We use the information on occupations to identify individuals working in either the liberalized or regulated occupations. Of the 94 occupations, we can identify 38 liberalized occupations and 40 regulated occupations. We restrict the sample to individuals between the ages of 16 and 65 who work in the crafts and trades sector. We drop apprentices, military or community service personnel and marginally employed persons who work less than 15 hours per week and earn less than 400 Euros per month.

Our main outcome variable of interest is whether an individual is self-employed. We also distinguish between self-employment with and without employees, as well as the skill level of the self-employed person. As in the social security data, we distinguish between three skill groups: the low-skilled have not finished high school or a vocational degree. The medium-skilled have completed high school or a vocational degree through an apprenticeship, which includes companions and Master craftsmen. Finally, the high-skilled are those with a degree from a university or technical college. We use the highest occupational degree to identify Master craftsmen; the category also contains some technical college degrees and other professional degrees.[22]

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[22]These additional degrees include degrees from a health care school, a professional academy (*Fachakademie*) and schools of cooperative education (*Berufsakademie*).

To investigate the effect of liberalization on self-employment more systematically, we use a difference-in-differences model of the following form:

$$Y_{iojst} = \delta Lib_o + \gamma After_t + \beta(Lib_o * After_t) + \alpha_t + \mu_o + \nu_j + \theta_s + \varepsilon_{iojst} \qquad (2.4)$$

where Y_{iojst} is an indicator equal to one if individual i in occupation o and industry j is self-employed in state s and year t; and zero otherwise. The treatment variable is the interaction effect $Lib_o * After_t$ which is equal to one if the occupation is liberalized and the individual is observed in a post-reform year. All specifications also include occupation (μ_o) and industry (ν_j) fixed effects, state (θ_s) and year (α_t) fixed effects. In some specifications, we also control for industry-specific or occupation-specific linear trends. As before, standard errors are clustered at the occupation level.

Fig. 2.7 Liberalization and Self-Employment
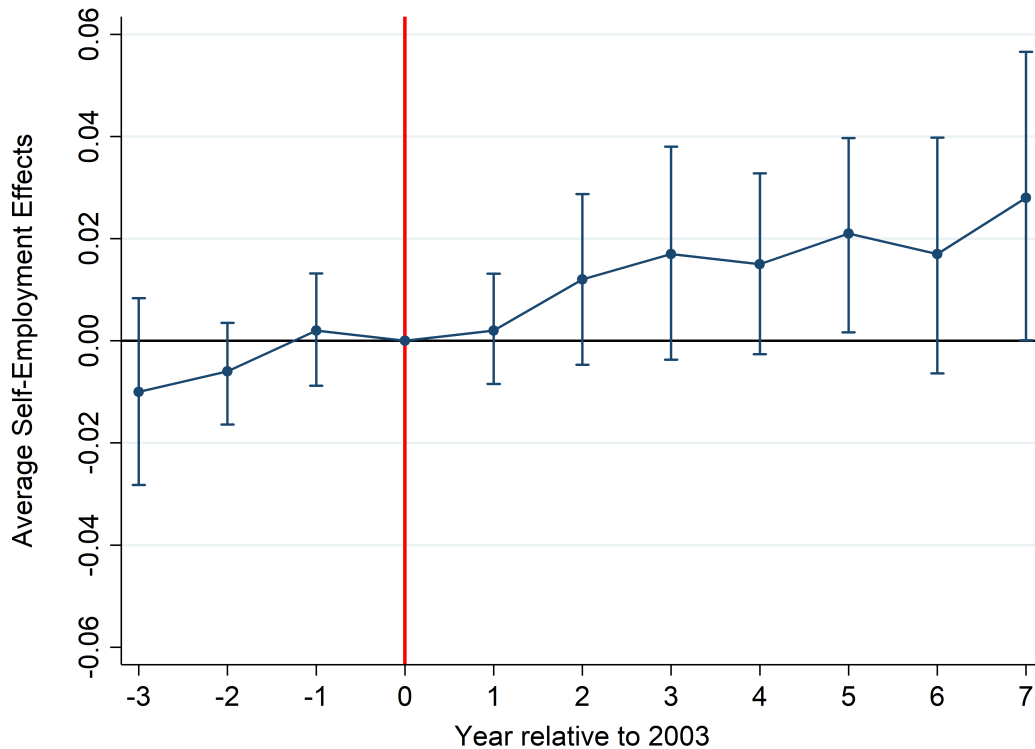
Notes: The figure plots estimates and the 95% confidence interval of the effect of deregulation on the probability of self-employment (relative to 2003) in the liberalized trades based on equation (2.4) after controlling for occupation, year, state and industry fixed effects. Standard errors are clustered at the occupation level.
Source: Microcensus (2000-2010).

Liberalization and Self-Employment

Figure 2.7 plots the coefficients from estimating equation (2.4) with individual year indicators to trace the dynamics of the reform. The figure shows that self-employment

increases after the reform relative to self-employment in the regulated occupations. By 2010, self-employment has increased by 2.8 percentage points in the liberalized trades. Given a baseline of 9.4 percent self-employment in the liberalized occupations, the reform raised self-employment by a sizable 30 percent.

The difference-in-differences estimates of equation (2.4) shown in Table 2.6 confirm the basic pattern of Figure 2.7. Over the post-reform period, overall self-employment increased by 1.8 percentage points in the liberalized crafts and trades (see column (1)). Who are the workers that enter the liberalized occupations? The growth in self-employment tends to be stronger among those without a Master craftsman degree (see column (3) of Table 2.6). Among those without a Master craftsman, entry into self-employment tends to be stronger for those without a completed apprenticeship (see column (4)) than for companions with a completed apprenticeship (see column (2)). These two groups indeed benefit from the reform as they could now open their own business in a liberalized trade after the reform, but not before. Table 2.6 further demonstrates (in columns (5) and (6)) that most craftsmen entering the liberalized occupations work as solo self-employed, i.e. do not hire additional employees.

As we do not have panel data about self-employment, we cannot use matching to adjust for pre-reform differences in observable characteristics between the liberalized and regulated occupations. The standard difference-in-differences approach in equation (2.4) might be biased if these differences affect the adjustment to the liberalization through differential sorting of workers into the liberalized occupations or through shifts in the industry, for instances. To account for such development, we use the semiparametric difference-in-differences approach (Abadie, 2005) for repeated cross-sections. Specifically, we adjust for observable characteristics like gender, age, firm size, medium- or high-skilled and for industry fixed effects before running a difference-in-differences regression. The results in Appendix Table A.11 show that we still find a positive effect of liberalization on self-employment even after accounting for differences across workers, heterogeneity in firm size and industry mix between liberalized and regulated occupations.

Where do the entrants into self-employment come from and do the new businesses survive in the liberalized market? To answer this question, we study year-to-year changes in employment states. Columns (1) and (2) of Appendix Table A.12 analyze new entrants into self-employment using the same model as in equation (2.4). The estimates show that entrants tend to switch from dependent employment as craftsmen or apprentice into self-employment (see column (1)); there is little entry from un- or non-employment into self-employment in the liberalized occupations. The absence of increased entry from outside the labor market also indicates that the Hartz reforms, which encouraged take-up of jobs or self-employment did not affect the liberalized and

Table 2.6 Liberalization and Self-Employment

| | Self-Employment | | | | | | Wages for Self-Employed | |
| | (overall) | (with Completed Apprenticeship) | (w/o Master Craftsmen) | (w/o Completed Apprenticeship) | (with Additional Employees) | (w/o Employees) | (with Additional Employees) | (w/o Employees) |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Liberalized	0.018*	0.016*	0.023**	0.024**	0.000	0.018*	0.044	0.008
	(0.010)	(0.008)	(0.011)	(0.011)	(0.003)	(0.010)	(0.032)	(0.041)
Treated	-0.322***	-0.315***	-1.073	-0.028	-0.220***	-0.101***	-0.438***	1.319***
	(0.014)	(0.013)	(1.026)	(0.018)	(0.011)	(0.007)	(0.131)	(0.076)
After	0.013*	0.092***	0.009*	-0.020	0.001	0.012*	-1.276**	-0.206***
	(0.007)	(0.030)	(0.006)	(0.036)	(0.003)	(0.007)	(0.520)	(0.057)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	255,270	214,820	24,626	156,406	255,270	255,270	13,459	10,334
R-squared	0.114	0.110	0.103	0.127	0.064	0.087	0.172	0.195

Notes: The table reports estimates of the effect of liberalizing the crafts and trades sector on the probability of self-employment and estimates on average wages for self-employed persons using a difference-in-differences approach. The dependent variable in columns (1)-(6) is whether a person is self-employed or not and in columns (7)-(8) the natural log of wages. Column (1) reports results for the full sample of craftsmen; column (2) restricts the sample to companions with completed apprenticeship. Columns (3) and (4) further split the sample of craftsmen into those without a Master craftsman degree and those without completed apprenticeship. Columns (5) and (6) report results for entrepreneurs with additional employees and solo self-employment. Column (7) restricts wage estimates to self-employed workers with additional employees and column (8) to solo self-employed workers. All specifications include year, occupation, state and industry fixed effects, as well as industry-specific trends. Standard errors are clustered at the occupation level. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Microcensus (2000-2010).

regulated occupations differentially.[23] Turning to exit from self-employment, columns (3) and (4) show no effects. These results are robust to including individual controls and industry- or occupation-specific trends.

2.5.4 Liberalization and Self-Employment among Immigrants

As mentioned in the introduction, 2004 saw not only the deregulation of the crafts and trades sector but also the accession of Central and Eastern European countries (plus Cyprus and Malta) to the European Union. Germany, like most other EU countries, restricted full labor market access to the new members for seven years until January 1, 2011, the so-called transition period. In order to work in Germany as an employee, a person from one of the accession countries had to apply for work permit at the federal employment agency in Germany.[24] In principle, citizens from EU member states can offer their services across the border. Yet, businesses registered in one of the EU-10 countries were restricted to offer their services in many liberalized trades (e.g. construction, building, inventory and transport cleaning sectors and interior decoration) during the transition period out of fear of unfettered competition.[25]

The restrictions to freedom of movement just discussed did not apply to self-employment, however. Hence, citizens from the new accession countries could immediately open up a business in Germany and offer their services if they could demonstrate sufficient monetary funds and health care coverage. Aggregate statistics suggest that the EU enlargement in 2004 indeed increased the presence of foreign craftsmen in the crafts and trades, esp. in the liberalized occupations. In 2005, 1.6% of all businesses in the crafts and trades were owned by citizens from the EU-10; one year later, the share had increased to 2.4%. In the liberalized occupations, almost 12% of all new businesses in 2007 were owned by citizens of the EU-10 countries, while the share was negligible (0.1%) in the regulated occupations.

Did the deregulation of 2004 increase entry of immigrants from the EU-10 countries into the liberalized trades? To answer that question, we re-estimate our model from equation (2.4) but split the sample by nationality. Table 2.7 shows the results on self-employment for German nationals (columns (1)-(3)) and then three groups of immigrants: immigrants from one of the traditional EU countries who have free access

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[23]We also see little effect on hours worked among the self-employed, which indicates that tougher competition does not lead to longer working hours.

[24]The federal employment agency granted between 60,000 and 100,000 work permits but also rejected around 20,000 each year.

[25]Other services, such as care services or IT consulting, did not face any restriction. In these cases, companies based in one of the accession countries could indeed send their employees, including third-country nationals as long they had a permanent employment contract, to provide services in Germany (see *Gesetz über den Arbeitsmarktzugang im Rahmen der EU-Erweiterung* (April, 23 2004)).

Table 2.7 Liberalization and Self-Employment among Natives and Immigrants

	German			from Traditional EU Countries (EU15)			from 2004 Accession Countries			from Non-European Countries		
	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Liberalized	0.020**	-0.001	0.021**	-0.011	-0.024*	0.014	0.045	-0.013	0.058*	-0.012	-0.011	-0.001
	(0.009)	(0.005)	(0.009)	(0.022)	(0.013)	(0.018)	(0.038)	(0.018)	(0.034)	(0.014)	(0.009)	(0.014)
Treated	-0.234***	-0.149***	-0.085***	-0.449***	-0.473***	0.024	-0.171	-0.106*	-0.065	1.105***	1.060***	0.045
	(0.022)	(0.020)	(0.011)	(0.043)	(0.025)	(0.029)	(0.142)	(0.055)	(0.139)	(0.040)	(0.027)	(0.034)
After	0.057***	0.024***	0.033**	0.004	0.024	-0.020	0.040	-0.007	0.047	0.153***	0.025	0.128**
	(0.015)	(0.008)	(0.013)	(0.055)	(0.049)	(0.050)	(0.122)	(0.040)	(0.118)	(0.049)	(0.020)	(0.049)
Year fixed rffects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	148,590	148,590	148,590	24,814	24,814	24,814	2,123	2,123	2,123	9,082	9,082	9,082
R-squared	0.165	0.109	0.097	0.161	0.107	0.091	0.356	0.134	0.346	0.189	0.074	0.167

Notes: The table reports estimates of the effect of liberalizing the crafts and trades sector on the probability of self-employment, self-employment with employees and solo self-employment, for sub groups based on a before and after difference-in-differences estimation strategy. Columns (1)-(3) show results for the probability of self-employment for German nationals, columns (4)-(6) for immigrants from traditional EU countries (i.e. Austria, Belgium, Denmark, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden and the United Kingdom), columns (7)-(9) for immigrants from 2004 EU accession countries (Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia and Slovenia) and columns (10)-(12) for immigrants from non-European countries. Additional covariates in columns (1)-(3) include a constant, a treatment dummy and an after dummy for the time period before and after the reform took place, a female dummy, medium- and high-skill dummies and age and age squared. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Microcensus (2000-2010).

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to the occupations (in columns (4)-(6)); immigrants from one of the EU-10 accession countries who could be self-employed in one of the liberalized trades (in columns (7)-(9)); and finally, immigrants from outside the EU who have to apply for a work permit and are not directly affected by the EU accession (in columns (10)-(12)). The results show an interesting pattern: self-employment increases among German nationals as well as the immigrants from the new accession countries; there is no effect for immigrants from the traditional EU countries or from immigrants coming from outside the EU. It is also interesting to note that the effect of the reform on solo self-employment is actually stronger for immigrants from the EU accession countries than for German nationals (compare column (9) to column (3)). Hence, the liberalization of market access helped immigrants from the new accession countries to start their business in Germany before full freedom of movement was granted in 2011.

Could the EU accession of the EU-10 in 2004 alone explain the rise in self-employment in the liberalized occupations after the reform? In that case, it might not have been the reform that raised entry but rather some outside event unrelated to the reform creating a spurious treatment effect. We can use the employment choices of EU-10 immigrants relative to German natives to address this question. EU-10 immigrants are affected by the accession (including the ensuing restrictions on freedom of movement) and the deregulation in the crafts and trades in 2004. German nationals, in contrast, are not directly affected by the accession, but respond to the deregulation. Immigrants and natives might still differ in the reasons they enter self-employment or in which occupation they work. Yet, as long as these differences are fixed over time, the difference in how the choices between EU-10 immigrants and natives change with the reform tells us something about the influence of the EU accession.

We first check whether EU-10 immigrants are more likely to be self-employed in the crafts and trades rather than other occupations compared to German natives. Appendix Table A.13 shows estimates from a triple differences estimator where the dependent variable is again self-employment (overall, with and without additional employees). In columns (1) to (3), the treatment effect is the interaction of three indicators: whether a person is self-employed in the crafts and trades or not ('Crafts and Trades'); whether the year is 2004 or later ('After'); and whether the person is a citizen of one of the EU-10 countries ('EU10'). The results show that EU-10 immigrants are much more likely to be self-employed in the crafts and trades after 2004 than German natives relative to self-employment outside the crafts and trades and before 2004. The second set of results in columns (4) to (6) of Appendix Table A.13 uses the same setup but restricts the sample to individuals working in the crafts and trades. Here, the treatment effect is the triple interaction between an indicator whether the occupation got liberalized ('Treated'); an indicator for the post-reform period ('After'); and an indicator whether

the person is a citizen of the EU-10 countries ('EU10'). Here, the results indicate that self-employment increases in the liberalized occupations after the reform for both EU-10 and German citizens, which supports our earlier results that the reform triggered additional entry. The coefficient on the triple interaction effect is positive, suggesting that EU-10 citizens tend to enter self-employment in the liberalized occupations at higher rates than Germans after 2004. Yet, the coefficient does not reach statistical significance. Moreover, the higher entry rates of EU-10 immigrants alone cannot explain the higher self-employment rates in the liberalized occupations in the post-reform period. As such, the reform effect for self-employment documented in the last section is not an artifact of the EU accession but a direct consequence of the deregulation in the crafts and trades.

2.6 Discussion and Conclusion

The crafts and trades sector in Germany has been one of the most heavily regulated since the Middle Ages. Eventually, the strict entry restrictions were bond to be incompatible with the idea of the European Single Market. As a consequence, entry and skill requirements to open up a business became less strict for many service occupations.

In this paper, we show that, contrary to widespread concerns, incumbent craftsmen in the deregulated occupations did not lose out after the liberalization. Despite more entry into the liberalized occupations, average wages and employment did not decline. Neither do incumbent craftsmen lose their jobs nor do they leave the market nor the liberalized occupations after the reform. This result indicates that liberalization might have increased demand among price-sensitive consumers who were priced out of the market before the reform.

We further document that Master craftsmen actually benefit from the liberalization as their wages increase after the reform. The higher skill premium arises if consumers might be willing to pay a premium for services provided or overseen by a Master craftsman if they infer those services to be of higher quality. If, after the reform, Master craftsmen become more scarce in the liberalized occupations, the skill premium will rise. After the deregulation, the number of examinations for the Master craftsmen degree indeed declined reducing the flow of new Master craftsmen into the deregulated occupations (Lembcke, 2019). In combination with increased entry from low-skilled craftsmen into self-employment, the stock of Master craftsmen decreases pushing up the skill premium.

Entry into the liberalized occupations appeared both from new plants opening, but also fewer plants closing. In addition, many craftsmen started their own business as solo

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self-employed, many of which are immigrants from Central and Eastern Europe and did not have the official qualifications for the occupational license prior to the reform. As such, the deregulation might have facilitated the transition of individuals from the EU-10 accession countries to find employment in Germany.

Overall, our evidence does not suggest that the reform induced any major market distortions or losses for craftsmen; on the contrary, the reform might have made it easier for EU immigrants to take advantage of the freedom of movement and find jobs in Germany. As such, it is unfortunate that the current debate calls for a (partial) reinstatement of the licensing requirements in several of the liberalized occupations.

Appendix A

Fig. A.1 Common Support
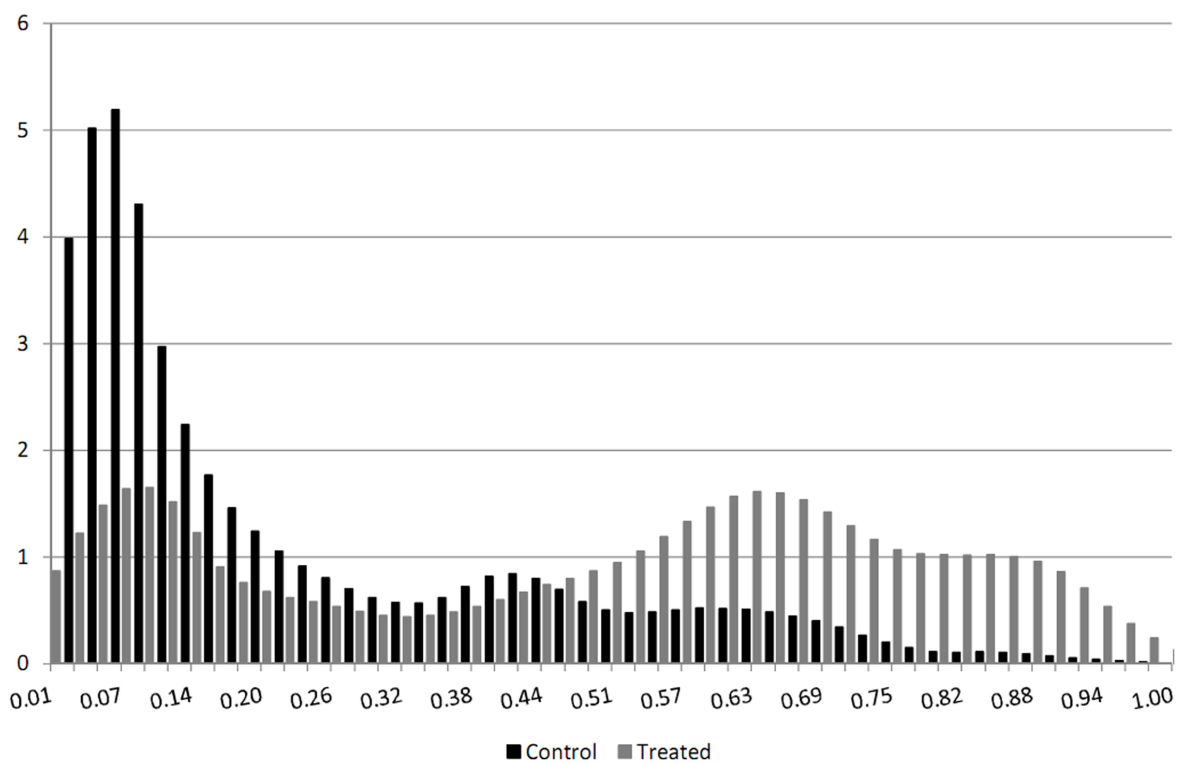
Notes: The figure plots the propensity score distribution for the treatment and the control group.

Fig. A.2 Balancing Plot of Sample before and after (Kernel-)Matching
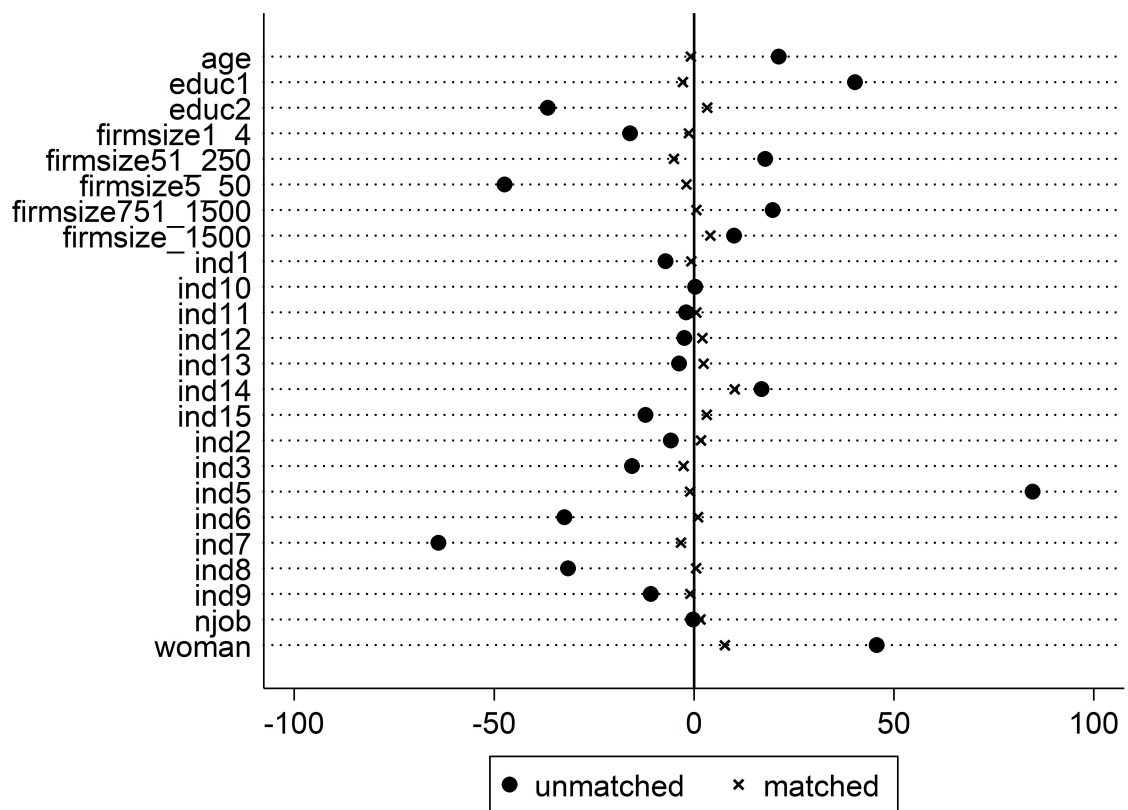
Notes: The figure plots the standardized bias before (shown as dots) and after kernel matching (shown as **x**).

Table A.1 Liberalized and Regulated Occupations in the Crafts and Trades Sector (2004 Reform)

Liberalized Occupations	Regulated Occupations
1. Tile, Slab and Mosaic Layer	1. Bricklayer and Concretor
2. Cast Stone and Terrazzo Maker	2. Stove and Air Heating Mechanic
3. Screed Layer	3. Carpenter
4. Vessel and Equipment	4. Roofer
5. Constructor	5. Road Contruction Worker
6. Clockmaker	6. Thermal and Acoustic Insulation Fitter
7. Engraver	7. Well Sinker
8. Metal Former	8. Stonemason
9. Galvaniser	9. Plasterer
10. Metal and Bell Founder	10. Painter and Lacquerer
11. Cutting Tool Mechanic	11. Scaffolder
12. Goldsmith and Silversmith	12. Chimney Sweep
13. Parquet Layer	13. Metal Worker
14. Shutter and Sunshade Mechatronics	14. Surgical Instrument Maker
15. Model Builder	15. Coachbuilder
16. Turner (Ivory Carver) and Wooden Toy Maker	16. Precision Engineer
17. Wood Carver	17. Motorbike and Bicycle Mechanic
18. Cooper	18. Refrigeration Mechanic
19. Basket Maker	19. Communication Technician
20. Costume Tailor	20. Automotive Mechatronics Technician
21. Embroiderer	21. Mechanic for Agricultural and Construction Machinery
22. Milliner	22. Gunsmith
23. Weaver	23. Plumber
24. Sailmaker	24. Installer and Heating Fitter
25. Furrier	25. Electrics Technician
26. Shoemaker	26. Electrical Machine Engineer
27. Saddler	27. Joiner
28. Interior Decorator	28. Boat Builder
29. Miller	29. Rope Maker
30. Brewer and Maltster	30. Baker
31. Wine Cellarperson	31. Pastry-cook
32. Textile Cleaner	32. Butcher
33. Building Cleaner	33. Dispensing Optician
34. Glass Finisher	34. Hearing Aid Acoustician
35. Precision Optician	35. Orthotic Technician
36. Glass and China Painter	36. Orthopaedic Shoemaker
37. Precious Stone Engraver and Cutter	37. Dental Technician
38. Photographer	38. Hairdresser
39. Bookbinder	39. Glazier
40. Typesetter and Printer	40. Glass Blower and Glass Apparatus Maker
41. Screen Printer	41. Mechanic for Tyres and Vulcanization
42. Flexographer	
43. Ceramist	
44. Organ and Harmonium Maker	
45. Piano and Harpsichord Maker	
46. Reed and Organ Musical Instrument Maker	
47. Violin Maker	
48. Bow Maker	
49. Metal Wind Instrument Maker	
50. Wooden Wind Instrument Maker	
51. Plucked Instrument Maker	
52. Gilder	
53. Sign and Illuminated Advertisement Maker	

Notes: The table lists the occupations in the crafts and trades sector that were liberalized in the 2004 Reform; and those that remained regulated.

Table A.2 Standardized Bias before and after Matching

	Sample	Treated	Control	% Bias	% Reduction	T-stat	p > |t|
1-4 employees	Unmatched	0.058	0.101	-16		-12.83	0.000
	Matched	0.058	0.064	-2.3	85.4	-1.78	0.074
5-50 employees	Unmatched	0.275	0.499	-47.4		-39.38	0.000
	Matched	0.275	0.286	-2.4	95	-1.71	0.087
51-250 employees	Unmatched	0.295	0.217	17.8		15.81	0.000
	Matched	0.295	0.312	-3.9	78.4	-2.47	0.013
751-1500 employees	Unmatched	0.077	0.032	19.7		19.37	0.000
	Matched	0.077	0.073	1.6	91.7	0.95	0.342
> 1500 employees	Unmatched	0.101	0.073	10		8.97	0.000
	Matched	0.101	0.091	3.4	65.9	2.2	0.028
Age	Unmatched	41.767	39.659	21.2		18.15	0.000
	Matched	41.767	42.050	-2.8	86.6	-1.93	0.053
Female	Unmatched	0.260	0.091	45.7		45.11	0.000
	Matched	0.260	0.229	8.3	81.9	4.84	0.000
Number of jobs	Unmatched	3.196	3.203	-0.2		-0.19	0.847
	Matched	3.196	3.126	2.4	-956.7	1.69	0.091
Low-skilled	Unmatched	0.191	0.060	40.3		40.9	0.000
	Matched	0.191	0.208	-5.3	86.8	-2.92	0.003
Medium-skilled	Unmatched	0.794	0.920	-36.6		-35.78	0.000
	Matched	0.794	0.776	5.1	85.9	2.93	0.003
Agriculture and Fishing	Unmatched	0.001	0.005	-7.1		-5.12	0.000
	Matched	0.001	0.001	0.2	97	0.26	0.796
Energy and Mining	Unmatched	0.006	0.012	-5.8		-4.61	0.000
	Matched	0.006	0.003	3.3	43	3.12	0.002
Food	Unmatched	0.018	0.045	-15.5		-11.89	0.000
	Matched	0.018	0.023	-3	80.3	-2.54	0.011
Producer Goods	Unmatched	0.370	0.052	84.7		93.58	0.000
	Matched	0.370	0.374	-1	98.8	-0.52	0.602
Investment Goods	Unmatched	0.105	0.224	-32.4		-25.65	0.000
	Matched	0.105	0.104	0.1	99.6	0.12	0.908
Construction	Unmatched	0.096	0.349	-63.9		-48.72	0.000
	Matched	0.096	0.111	-4	93.7	-3.54	0.000
Retail Trade	Unmatched	0.058	0.154	-31.5		-24.24	0.000
	Matched	0.058	0.050	2.8	91.1	2.55	0.011
Transport and Communications	Unmatched	0.003	0.012	-10.8		-7.94	0.000
	Matched	0.003	0.003	0.3	97.7	0.28	0.777
Finance and Insurance	Unmatched	0.001	0.001	0.3		0.3	0.761
	Matched	0.001	0.001	0.9	-157.9	0.64	0.522
Hotel and Restaurant Industry	Unmatched	0.002	0.003	-2		-1.66	0.097
	Matched	0.002	0.002	0.7	65.1	0.56	0.574
Educational Services	Unmatched	0.004	0.006	-2.4		-1.99	0.047
	Matched	0.004	0.002	2.3	3.8	1.96	0.050
Health, Veterinary and Social Services	Unmatched	0.008	0.011	-3.7		-3.01	0.003
	Matched	0.008	0.005	3.2	13.3	2.69	0.007
Corporate Services	Unmatched	0.101	0.056	16.9		15.84	0.000
	Matched	0.101	0.077	8.8	48.1	5.59	0.000
Other Services	Unmatched	0.022	0.043	-12.1		-9.51	0.000
	Matched	0.022	0.012	5.4	55	5.05	0.000

Notes: The table reports the standardized bias in % before and after matching for treated and control groups. The table also shows the reduction in the standardized bias after matching, the t-statistic and the corresponding p-value for a difference in means test.
Source: Individual social security records (2000-2010).

Table A.3 Liberalization and Average Wages by Union Coverage

	Baseline		Low Union Coverage		Low Industry Union Coverage	
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized (t=2000)	-0.006	-0.005	-0.005	-0.005	-0.003	-0.003
	(0.009)	(0.007)	(0.008)	(0.009)	(0.008)	(0.008)
Liberalized (t=2001)	-0.005	-0.005	0.000	0.001	0.000	0.000
	(0.008)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)
Liberalized (t=2002)	-0.002	-0.001	0.002	0.002	0.002	0.002
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Liberalized (t=2003)						
Liberalized (Reform year)	-0.003	-0.003	-0.010	-0.010	-0.008	-0.008
	(0.006)	(0.006)	(0.010)	(0.009)	(0.009)	(0.008)
Liberalized (t=2005)	0.011	0.010	-0.003	-0.003	-0.001	-0.001
	(0.011)	(0.010)	(0.014)	(0.013)	(0.013)	(0.011)
Liberalized (t=2006)	0.005	0.004	-0.011	-0.010	-0.007	-0.007
	(0.012)	(0.009)	(0.015)	(0.013)	(0.013)	(0.012)
Liberalized (t=2007)	0.007	0.005	-0.007	-0.008	-0.006	-0.006
	(0.016)	(0.012)	(0.017)	(0.014)	(0.016)	(0.013)
Liberalized (t=2008)	0.012	0.010	-0.010	-0.010	-0.008	-0.008
	(0.018)	(0.013)	(0.019)	(0.014)	(0.017)	(0.013)
Liberalized (t=2009)	0.011	0.008	-0.011	-0.012	-0.010	-0.010
	(0.019)	(0.014)	(0.020)	(0.016)	(0.017)	(0.014)
Liberalized (t=2010)	0.020	0.017	0.003	0.004	0.003	0.004
	(0.022)	(0.015)	(0.022)	(0.018)	(0.019)	(0.016)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	-	Yes	-	Yes	-	Yes
Observations	207,465	207,465	84,751	84,751	119,000	119,000
R-squared	0.428	0.429	0.401	0.402	0.424	0.425

Notes: The table reports estimates of deregulation on average wages in columns (1)-(2). Columns (3)-(4) show average wages for industries with a low share of workers covered by a firm- or industry-level union agreement and columns (5)-(6) by an industry-level union agreement in the crafts and trades, based on equation (2.1). The dependent variable in all specifications is the log daily fte-adjusted real wage (in 2010 prices). The reference year is 2003. We include only workers who are attached to the labor market, i.e. those who are employed for three years prior to the Reform. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5%** and 10% *.
Source: Matched social security records and establishment panel (2000-2010).

Table A.4 Effect of Liberalization on Wages of Master Craftsmen (Heterogeneity and Robustness)

	Wages of Master Craftsmen						
	Ages 26-50	Ages 51-65	Men	Ever Master Spell	Drop Construction	Firm Size Controls	2-digit Industry Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Liberalized (t=2000)	0.062**	0.009	0.015	0.028	0.036	0.037	0.047
	(0.029)	(0.047)	(0.025)	(0.021)	(0.035)	(0.032)	(0.031)
Liberalized (t=2001)	0.017	0.038	0.004	0.006	0.023	0.023	0.025
	(0.024)	(0.044)	(0.019)	(0.015)	(0.024)	(0.023)	(0.023)
Liberalized (t=2002)	0.027	0.02	0.005	0.015	0.021	0.016	0.017
	(0.019)	(0.035)	(0.014)	(0.014)	(0.017)	(0.015)	(0.016)
Liberalized (t=2003)							
Liberalized (Reform year)	0.055**	-0.012	0.023	0.022*	0.044**	0.033**	0.030*
	(0.022)	(0.030)	(0.014)	(0.013)	(0.017)	(0.015)	(0.016)
Liberalized (t=2005)	0.058**	-0.003	0.029	0.041**	0.056**	0.043**	0.030
	(0.028)	(0.027)	(0.022)	(0.018)	(0.022)	(0.021)	(0.021)
Liberalized (t=2006)	0.071***	-0.02	0.027	0.045**	0.070***	0.042**	0.034
	(0.026)	(0.047)	(0.021)	(0.019)	(0.021)	(0.020)	(0.022)
Liberalized (t=2007)	0.066**	0.02	0.028	0.051***	0.072***	0.057**	0.039
	(0.028)	(0.047)	(0.021)	(0.018)	(0.026)	(0.022)	(0.025)
Liberalized (t=2008)	0.079**	0.041	0.057**	0.062***	0.104***	0.092***	0.067**
	(0.033)	(0.041)	(0.023)	(0.021)	(0.025)	(0.022)	(0.028)
Liberalized (t=2009)	0.085**	0.031	0.049*	0.064***	0.088***	0.079***	0.052
	(0.034)	(0.050)	(0.025)	(0.022)	(0.031)	(0.027)	(0.034)
Liberalized (t=2010)	0.108***	0.008	0.063**	0.063***	0.092***	0.084***	0.061*
	(0.038)	(0.041)	(0.028)	(0.024)	(0.033)	(0.029)	(0.037)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm size fixed effects	-	-	-	-	-	Yes	-
Observations	5,924	2,212	7,526	15,325	6,600	8,224	8,246
R-squared	0.542	0.616	0.508	0.423	0.515	0.569	0.55

Notes: The table reports estimates of deregulation on wages of Master craftsmen based on equation (2.1). The dependent variable in all specifications is the log daily fte adjusted real wage (at 2010 prices) of Master craftsmen. The reference year is 2003. Columns (1)-(2) show the effects by age groups and column (3) for men only. Column (4) reruns the model for craftsmen who ever worked as Master craftsmen, column (5) restricts the sample to Master craftsmen outside the construction sector. Column (6) adds firm size controls and column (7) 2-digit industry fixed effects as well as detailed industry-specific trends. All specifications include year, occupation, state and broad industry fixed effects. The sample is restricted to workers who are attached to the labor market, i.e. those who are employed for three years prior to the Reform. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5%** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

Table A.5 Effect of Liberalization on Wages of Master Craftsmen (Robustness)

	Wages of Master Craftsmen					
	Drop Roofing (1)	Drop Painting (2)	Drop Construction (3)	Drop Electrics (4)	Drop Cleaning (5)	Drop all Sectors (6)
Liberalized (t=2000)	0.037	0.037	0.036	0.037	0.032	0.028
	(0.030)	(0.031)	(0.035)	(0.031)	(0.031)	(0.037)
Liberalized (t=2001)	0.015	0.018	0.023	0.014	0.012	0.015
	(0.023)	(0.023)	(0.024)	(0.023)	(0.023)	(0.025)
Liberalized (t=2002)	0.015	0.013	0.021	0.016	0.011	0.011
	(0.015)	(0.016)	(0.017)	(0.016)	(0.014)	(0.016)
Liberalized (t=2003)						
Liberalized (Reform year)	0.034**	0.034**	0.044**	0.035**	0.036**	0.045**
	(0.016)	(0.016)	(0.017)	(0.016)	(0.016)	(0.018)
Liberalized (t=2005)	0.039*	0.038	0.056**	0.039	0.039*	0.056**
	(0.023)	(0.024)	(0.022)	(0.024)	(0.023)	(0.024)
Liberalized (t=2006)	0.047**	0.048**	0.070***	0.049**	0.046**	0.076***
	(0.021)	(0.022)	(0.021)	(0.022)	(0.022)	(0.022)
Liberalized (t=2007)	0.053**	0.056**	0.072***	0.058**	0.054**	0.078***
	(0.023)	(0.024)	(0.026)	(0.023)	(0.023)	(0.026)
Liberalized (t=2008)	0.086***	0.086***	0.104***	0.091***	0.082***	0.108***
	(0.024)	(0.025)	(0.025)	(0.024)	(0.024)	(0.024)
Liberalized (t=2009)	0.073**	0.071**	0.088***	0.076***	0.068**	0.089***
	(0.028)	(0.030)	(0.031)	(0.029)	(0.027)	(0.031)
Liberalized (t=2010)	0.081**	0.083**	0.092***	0.086***	0.080***	0.097***
	(0.031)	(0.033)	(0.033)	(0.031)	(0.030)	(0.033)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,122	8,018	6,600	8,014	8,155	6,280
R-squared	0.513	0.510	0.515	0.505	0.511	0.510

Notes: The table reports estimates of deregulation on wages of Master craftsmen based on equation (2.1). The dependent variable in all specifications is the log daily fte-adjusted real wage (at 2010 prices) of Master craftsmen. The reference year is 2003. Column (1) restricts the sample to Master craftsmen outside the roofing occupation. Column (2) restricts the sample to Master craftsmen outside the painting and varnishing occupation. Column (3) restricts the sample to Master craftsmen outside the construction sector and column (4) restricts the sample to Master craftsmen outside the electrician profession. Column (5) restricts the sample to Master craftsmen outside the cleaning profession and column (6) to Master craftsmen outside all sectors specified in columns (1)-(5). All specifications include year, occupation, state and broad industry fixed effects and industry-specific trends. The sample is restricted to workers who are attached to the labor market, i.e. those who are employed for three years prior to the Reform. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5%** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

Table A.6 Liberalization and Overall Mobility

	Job Mobility		Occupational Mobility		Industry Mobility	
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized	0.014	0.005	-0.005	-0.008	-0.003	-0.013
	(0.009)	(0.011)	(0.004)	(0.005)	(0.004)	(0.012)
Treated	-37.516***	-16.543***	-21.051***	-21.156***	-0.076**	-0.061**
	(3.186)	(3.808)	(1.197)	(1.637)	(0.029)	(0.027)
After	0.247***	0.247***	0.074***	0.074***	0.108***	0.199***
	(0.012)	(0.035)	(0.010)	(0.019)	(0.013)	(0.051)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	-	Yes	-	Yes	-	Yes
Occupation-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	418,286	418,286	418,936	418,936	418,936	418,936
R-squared	0.083	0.085	0.038	0.040	0.060	0.066
Mean (prior Reform)	0.247	0.247	0.208	0.208	0.189	0.189

Notes: The table reports estimates of deregulation on the mobility of incumbent workers and new entrants in the crafts and trades using a difference-in-differences approach. The dependent variable in columns (1)-(2) is the probability of switching plants; in columns (3)-(4) the probability of switching the occupation, and in columns (5)-(6) the probability of switching industries at the 2-digit level. The reference year is 2003. All specifications include a constant, a time-varying treatment dummy and an after dummy for the time period before and after the reform took place, as well as year, occupation, state and broad industry fixed effects and occupation-specific trends. The second specification (in columns (2) and (4) and (6)) further adds industry-specific trends. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Establishment history panel (2000-2010).

Table A.7 Liberalization, Plant Openings, Closings and Growth

	Plant Openings (1)	Plant Closings (2)	Employment (3)	Inflows (4)	Outflows (5)
Liberalized (t=2000)	0.002	-0.005*	-0.005	0.065***	-0.024
	(0.003)	(0.003)	(0.009)	(0.022)	(0.021)
Liberalized (t=2001)	0.003	-0.002	0.003	0.067	-0.008
	(0.002)	(0.003)	(0.005)	(0.038)	(0.017)
Liberalized (t=2002)	0.000	-0.003	0.011**	0.031	0.012
	(0.001)	(0.003)	(0.004)	(0.019)	(0.012)
Liberalized (t=2003)					
Liberalized (Reform year)	-0.001	-0.005***	-0.004	0.031	-0.054***
	(0.003)	(0.002)	(0.004)	(0.020)	(0.017)
Liberalized (t=2005)	0.001	-0.003	0.001	0.040	-0.036
	(0.003)	(0.002)	(0.003)	(0.028)	(0.022)
Liberalized (t=2006)	0.000	-0.005**	-0.003	0.025	-0.030*
	(0.002)	(0.002)	(0.005)	(0.036)	(0.017)
Liberalized (t=2007)	0.002	-0.006*	-0.003	0.038	-0.04
	(0.002)	(0.003)	(0.009)	(0.041)	(0.027)
Liberalized (t=2008)	0.007**	-0.003	0.004	0.093*	0.011
	(0.003)	(0.003)	(0.013)	(0.048)	(0.015)
Liberalized (t=2009)	0.009**	-0.005	-0.002	0.011	0.075***
	(0.004)	(0.006)	(0.015)	(0.071)	(0.020)
Liberalized (t=2010)	0.007**	-0.004	0.010	0.062	-0.006
	(0.003)	(0.005)	(0.016)	(0.057)	(0.021)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Firm size fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes
Observations	943,702	943,702	943,702	784,621	729,656
R-squared	0.027	0.027	0.809	0.496	0.482
Mean (prior Reform)	0.042	0.042	2.76	1.67	1.62

Notes: The table shows the effect of the deregulation on plant openings and closings, as well as employment growth at the firm and in- and outflows based on equation (2.3). Columns (1)-(2) analyze the effect on the probability of a plant opening and the probability of a plant closure. The dependent variable in column (3) is the natural log of the total number of employees at the firm. The dependent variables in column (4) and (5) are the natural logarithm of the activity shown in the top row. Standard errors are clustered at the industry level. Means for the pre Reform period are given below. Significance levels: 1% ***, 5% ** and 10% *.
Source: Establishment history panel (2000-2010).

Table A.8 Summary Statistics of Sample using Kernel Matching

	Liberalized Occupations	Regulated Occupations	Control Occupations	Diff. btw. Liberalized and Regulated Occ.		Diff. btw. Liberalized and Control Occ.	
				Coeff.	S.E.	Coeff.	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)

Panel A: Characteristics Not Matched

Employment growth (past 3 years)	-0.014	0.003	-0.020	-0.017	0.042	0.006	0.045
Daily real wage	88.217	88.992	88.852	-0.775	7.034	-0.361	7.110
FTE daily real wage	90.508	89.672	89.852	0.836	6.684	0.656	6.824
Log FTE daily real wage	4.442	4.434	4.437	0.007	0.079	0.005	0.079
FTE daily wage growth	0.015	0.013	0.014	0.002	0.005	0.001	0.005
Firm employment	1433.154	1096.099	1147.026	337.055	819.098	268.128	814.849
Part-time	0.068	0.020	0.034	0.048**	0.024	0.034	0.024
East German	0.189	0.223	0.192	-0.034	0.037	-0.003	0.038
Training duration	350.130	500.197	381.724	-150.066***	40.323	-31.593	39.104
Foreign	0.103	0.062	0.096	0.040***	0.012	0.007	0.018

Panel B: Matched Characteristics

Employees:							
1-4	0.058	0.101	0.061	-0.043	0.026	-0.003	0.024
5-50	0.275	0.499	0.275	-0.224***	0.078	-0.009	0.076
51-250	0.295	0.217	0.316	0 .078**	0.030	-0.022	0.034
251-750	0.195	0.078	0.174	0.117***	0.034	0.021	0.034
751-1500	0.077	0.032	0.076	0 .045**	0.021	0.001	0.023
>1500	0.100	0.073	0.089	0.028	0.050	0.011	0.055
Age	41.767	39.659	41.843	2.109***	0.495	-0.075	0.458
Female	0.260	0.091	0.231	0.169***	0.062	0.029	0.064
Number of jobs	3.196	3.203	3.149	-0.007	0.162	0.047	0.146
Education:							
Low-skilled	0.191	0.060	0.200	0.131***	0.022	-0.009	0.032
Medium-skilled	0.794	0.920	0.782	-0.126***	0.022	0.011	0.030
High-skilled	0.016	0.020	0.018	-0.005	0.011	-0.002	0.011
Industry:							
Agriculture and Fishing	0.001	0.005	0.001	-0.004*	0.002	0.000	0.001
Energy and Mining	0.006	0.012	0.005	-0.006	0.004	0.002	0.002
Food	0.018	0.045	0.022	-0.027	0.029	-0.005	0.020
Consumer Goods	0.206	0.029	0.227	0.177**	0.081	-0.021	0.117
Producer Goods	0.370	0.052	0.374	0.319**	0.147	-0.004	0.150
Investment Goods	0.105	0.224	0.101	-0.119*	0.061	0.004	0.044
Construction	0.096	0.349	0.109	-0.253**	0.105	-0.013	0.068
Retail Trade	0.058	0.154	0.057	-0.096	0.067	0.002	0.030
Transport and Communications	0.003	0.014	0.004	-0.009**	0.004	-0.001	0.002
Finance and Insurance	0.001	0.001	0.001	0.000	0.000	0.000	0.001
Hotel and Restaurant Industry	0.002	0.003	0.002	-0.001	0.001	0.000	0.001
Educational Services	0.004	0.006	0.003	-0.002	0.002	0.001	0.002
Health, Veterinary and Social Services	0.008	0.011	0.005	-0.004	0.006	0.002	0.004
Corporate Services	0.101	0.056	0.073	0.045	0.070	0.027	0.072
Other Services	0.022	0.043	0.016	-0.021	0.037	0.006	0.017

Notes: Columns (1)-(3) compare means in 2003 for liberalized occupations, regulated occupations and matched control occupations in terms of (Panel A) unmatched characteristics and (Panel B) characteristics we explicitly match on. Columns (4)-(5) report differences between liberalized and regulated occupations and columns (6)-(7) differences between liberalized and matched control occupations. As a matching estimator we use Kernel Matching. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

Table A.9 Kernel Matching and Bootstrapped Standard Errors for Wages and Employment

	Wages of Master Craftsmen		Average Wages		Average Employment	
	no Bootstr.	with Bootstr. S.E.	no Bootstr.	with Bootstr. S.E.	no Bootstr.	with Bootstr. S.E.
	(1)	(2)	(3)	(4)	(5)	(6)
Liberalized (t=2000)	0.042	0.042	-0.006	-0.006	0.002	0.002
	(0.027)	[-0.017, 0.011]	(0.007)	[-0.020, 0.013]	(0.002)	[-0.002, 0.006]
Liberalized (t=2001)	0.024	0.024	-0.006	-0.006	0.001	0.001
	(0.022)	[-0.021, 0.080]	(0.006)	[-0.018, 0.010]	(0.001)	[-0.001, 0.004]
Liberalized (t=2002)	0.017	0.017	-0.002	-0.002	0.000	0.000
	(0.014)	[-0.013, 0.051]	(0.004)	[-0.010, 0.008]	(0.001)	[-0.001, 0.002]
Liberalized (t=2003)						
Liberalized (Reform year)	0.036**	0.036**	-0.004	-0.004	0.000	0.000
	(0.016)	[0.001, 0.072]	(0.005)	[-0.016, 0.005]	(0.001)	[-0.002, 0.003]
Liberalized (t=2005)	0.039*	0.039	0.008	0.008	-0.002	-0.002
	(0.023)	[-0.018, 0.087]	(0.010)	[-0.016, 0.029]	(0.002)	[-0.006, 0.002]
Liberalized (t=2006)	0.045**	0.045*	0.004	0.004	-0.003*	-0.003
	(0.019)	[-0.004, 0.084]	(0.010)	[-0.020, 0.025]	(0.002)	[-0.007, 0.002]
Liberalized (t=2007)	0.054***	0.054**	0.003	0.003	-0.002	-0.002
	(0.020)	[0.010, 0.094]	(0.012)	[-0.026, 0.030]	(0.003)	[-0.007, 0.005]
Liberalized (t=2008)	0.077***	0.077**	0.006	0.006	-0.002	-0.002
	(0.023)	[0.026, 0.123]	(0.013)	[-0.027, 0.037]	(0.003)	[-0.008, 0.005]
Liberalized (t=2009)	0.069***	0.069**	0.005	0.005	0.001	0.001
	(0.025)	[0.014, 0.119]	(0.014)	[-0.032, 0.037]	(0.004)	[-0.007, 0.011]
Liberalized (t=2010)	0.080***	0.080**	0.014	0.014	0.000	0.000
	(0.028)	[0.018, 0.139]	(0.016)	[-0.027, 0.051]	(0.004)	[-0.008, 0.009]
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,165	21,165	434,983	434,983	435,062	435,062
R-squared	0.495	0.495	0.431	0.431	0.110	0.110

Notes: The table reports the effects of deregulation for wages and employment of incumbent craftsmen based on equation (2.1). The dependent variable in columns (1)-(2) is the log real daily wage (at 2010 prices) of Master craftsmen (in fte); the log real daily wage (at 2010 prices) of all craftsmen in columns (3)-(4); and employment (measured in fte) of all craftsmen in columns (5)-(6). The first specification in columns (1), (3) and (5) shows the baseline result without accounting for estimation in the matching stage. Specifications (2), (4) and (6) show bootstrapped standard errors using a wild bootstrap. The matching estimator used in the analysis is the kernel matching estimator with common support restriction, kernel type epanechnikov and bandwidth of 0.06. The sample is restricted to craftsmen who are attached to the labor market, i.e. those who are employed for three years prior to the reform. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Matched sample, individual social security records (2000-2010).

Table A.10 Summary Statistics

	Liberalized Occupations in 2003 (1)	Liberalized Occupations in 2010 (2)	Regulated Occupations in 2003 (3)	Regulated Occupations in 2010 (4)
Panel A: Dependent Characteristics				
Self-employment	0.095	0.131	0.142	0.164
Solo self-employment	0.051	0.082	0.052	0.069
Self-employment with employees	0.044	0.049	0.090	0.096
Hours worked	32.814	33.593	39.454	40.744
Monthly real income	1,286.009	1,230.120	1,602.985	1,562.260
Panel B: Independent Characteristics				
Employees:				
1-4	0.064	0.172	0.082	0.203
5-10	0.049	0.092	0.083	0.179
11-19	0.052	0.092	0.070	0.133
20-49	0.066	0.116	0.064	0.130
>50	0.212	0.504	0.152	-0.028
Age:				
16-25	0.089	0.071	0.144	0.123
26-50	0.821	0.796	0.786	0.776
51-65	0.090	0.133	0.070	0.101
Female	0.472	0.494	0.114	0.123
Part-time	0.288	0.345	0.039	0.061
Education:				
Low-skilled	0.320	0.166	0.117	0.067
Medium-skilled	0.659	0.569	0.865	0.744
High-skilled	0.021	0.264	0.019	0.190
Master craftsmen	0.069	0.067	0.188	0.209
Un-/Non-employed	0.014	0.025	0.008	0.007
East German	0.156	0.165	0.196	0.203
German	0.846	0.829	0.922	0.918
Traditional EU countries	0.037	0.034	0.025	0.022
2004 accession countries	0.009	0.024	0.005	0.011
2007 accession countries	0.004	0.005	0.001	0.002
Non-EU countries	0.105	0.108	0.047	0.047
Observations	7,748	7,275	17,315	15,894

Notes: Columns (1) and (3) compare means in 2003 for liberalized occupations and regulated occupations for dependent characteristics (Panel A) and independent characteristics (Panel B). Columns (2) and (4) compare means in 2010 for liberalized and still regulated occupations. Columns (5)-(6) report differences between liberalized and regulated occupations in 2003. The traditional EU countries above comprise all EU member countries except for Germany, these are the following: Denmark, Finland, Sweden, France, Greece, Italy, the Netherlands, Austria, Portugal, Spain, Belgium, Ireland, Luxemburg and the UK. The 2004 accession countries include Poland, Slovenia, the Czech Republic, Hungary, Estonia, Lithuania, Latvia, Slovenia, Cyprus and Malta, whereas in 2007 Bulgaria and Romania joined the EU (2007 Accession Countries). Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Microcensus (2000-2010).

Table A.11 Liberalization and Self-Employment based on a Semiparametric DiD Estimator

	Abadie's Semiparametric Diff-in-Diff Estimator		
	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed
	(1)	(2)	(3)
Liberalized	0.026***	0.015***	0.014***
	(0.004)	(0.003)	(0.003)
Observations	186,749	186,749	186,749

Notes: The table reports estimates of the effect of liberalizing the crafts and trades sector on the probability of self-employment (column (1)), of self-employment with employees (in column (2)) and of solo self-employment (in column (3)). The table implements the semiparametric difference-in-differences estimator for repeated cross-sectional data by Abadie (2005). The propensity score is obtained by a series logit estimator on individual characteristics (age, age squared, female, medium- or high-skilled), firm size and industry fixed effects. The difference-in-differences estimation in the second step is restricted to observations on the common support of the propensity score. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Microcensus (2000-2010).

Table A.12 Worker Flows into and out of Self-Employment

	Entry Self-Employment		Exit Self-Employment to	
	Dependent Employment	Un-/Non- Employment	Dependent Employment	Un-/Non- Employment
	(1)	(2)	(3)	(4)
Liberalized	0.006	0.001	0.001	0.001
	(0.010)	(0.001)	(0.001)	(0.001)
Treated	-0.026***	0.002***	-0.001	0.001
	(0.009)	(0.001)	(0.001)	(0.001)
After	0.035*	0.003	-0.001	-0.001
	(0.018)	(0.003)	(0.002)	(0.001)
Year fixed effects	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes
Observations	186,749	186,749	186,749	186,749
R-squared	0.064	0.005	0.001	0.005
Mean (prior Reform)	0.053	0.001	0.003	0.000

Notes: The table reports estimates of the effect of liberalizing the crafts and trades sector on the probability of entry into and exits from self-employment in liberalized occupations based on equation (2.4). Means of the dependent variable in the pre-reform period are reported in the last row. The first two columns show results for entry into self-employment from dependent employment (column (1)) and from un-/non-employment (column (2)). The next two columns show how liberalization affects exit from self-employment into employment (column (3)) and into un-/non-employment (column (4)). All specifications control for age, age squared, medium- and high-skilled and include year, ocupation, state and industry fixed effects as well as industry-specific trends. Significance levels: 1% ***, 5% ** and 10% *.
Source: Microcensus (2000-2010).

Table A.13 Liberalization and Self-Employment of Immigrants from EU-10 Countries

	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed	Self-Employed	Self-Employed w/ Employees	Solo Self-Employed
	(1)	(2)	(3)	(4)	(5)	(6)
Crafts and Trades x After x EU10	0.145***	0.025**	0.120***			
	(0.042)	(0.012)	(0.039)			
Crafts and Trades x After	0.009**	0.004*	0.005			
	(0.004)	(0.002)	(0.003)			
Crafts and Trades x EU10	-0.059***	-0.042***	-0.018			
	(0.015)	(0.012)	(0.011)			
Crafts and Trades	0.021	-0.002	0.023			
	(0.050)	(0.015)	(0.042)			
Liberalized x EU10				0.095	0.005	0.090
				(0.073)	(0.020)	(0.070)
Liberalized				0.016**	-0.001	0.017**
				(0.008)	(0.003)	(0.008)
Treated x EU10				0.008	0.021	-0.013
				(0.029)	(0.022)	(0.019)
Treated				0.002	-0.049***	0.051***
				(0.024)	(0.017)	(0.014)
After x EU10	0.020**	-0.002	0.022***	0.117**	0.017	0.100**
	(0.009)	(0.006)	(0.008)	(0.050)	(0.014)	(0.047)
After	0.003	-0.008**	0.011**	0.087***	0.030***	0.057***
	(0.007)	(0.004)	(0.005)	(0.018)	(0.010)	(0.015)
EU10	0.003	-0.009	0.011**	-0.063***	-0.060***	-0.003
	(0.008)	(0.007)	(0.006)	(0.014)	(0.012)	(0.012)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,452,069	1,452,069	1,452,069	186,749	186,749	186,749
R-squared	0.346	0.225	0.225	0.162	0.104	0.100

Notes: The table reports estimates of the effect of the EU enlargement in 2004 on self-employment using a triple differences strategy. The dependent variables are the probability of self-employment (in columns (1) and (4)), self-employment with employees (columns (2) and (5)) and solo self-employment (in columns (3) and (6)). In columns (1) to (3), the teatment effect is the triple interaction between an indicator whether the person is employed in the crafts and trades; and an indicator for the years after the reform in 2004. The third term is an indicator whether a person is a citizen of one of the EU-10 accession countries (i.e. Poland, Hungary, the Czech Republic, Slovakia, Slovenia, Estonia, Lithuania, Latvia, Cyprus and Malta); the indicator is zero if the person is a German native. In columns (4) to (6), the interaction term (Liberalized) is further interacted with an indicator equal to one if a person comes from one of the EU-10 countries that joined the European Union in 2004 (i.e. Poland, Hungary, the Czech Republic, Slovakia, Slovenia, Estonia, Lithuania, Latvia, Cyprus and Malta). The indicator is zero if the person is a German native. All specifications include year, occupation, state and industry fixed effects as well as socio-demographic characteristics (gender, age, age squared, medium- or high-skilled). Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: Microcensus (2000-2010).

Chapter 3

Deregulation and Training: Evidence from a Crafts and Trades Reform

3.1 Introduction

In the last few decades deregulation, specifically the removal of entry barriers, has gained considerable interest among policy makers in Europe as entry restrictions are considered to distort competition, prices, employment and better economic performance (OECD, 2019a).

While a large literature has analyzed the effects of entry regulations on firm-level outcomes, innovation and employment growth, the effects of deregulation on vocational training are scarce and hotly debated.[1] Proponents of entry restrictions argue that restricting market access increases incentives to invest in vocational training and maintains the high training quality apprentices receive. Yet, opponents criticize the dampening effects on competition and economic growth through restricting firm entry (Monopolkommission, 2001). My study provides new evidence on this debate by analyzing a major deregulation in the German crafts and trades sector and its consequences for the apprenticeship market and post-apprenticeship labor market outcomes.

[1]Aghion et al. (2008) show that deregulating the License Raj in India has led to increased firm entry and output growth. While analyzing an increase in regulation in French zoning boards, Bertrand and Kramarz (2002) find that stricter regulation in the retail industry leads to a decline in employment growth and job opportunities.

Among European countries, the German crafts and trades sector is one of the most heavily regulated, where firm entry restrictions are tied to the ability of providing vocational training in form of apprenticeships. The German crafts and trades is an important sector, employing around 28% of all apprentices in the dual Vocational Education and Training system (VET) and around 13% of the workforce in 2003. Further, it generates positive externalities as many sectors and industries in the economy hire skilled workers (*Fachkräfte*) which were trained in the crafts and trades (Monopolkommission, 1998).[2]

To shed light on the labor market consequences of deregulation on the apprenticeship market, I exploit the 2004 reform in the German crafts and trades sector. The reform removed entry barriers in 53 ("deregulated trades") out of 94 trades, while the entry restrictions stayed in place in the remaining 41 ("still regulated") trades. The main entry restriction in the German crafts and trades has been that business owners were required to hold a Master craftsman degree (*Meisterbrief*) and that only Master craftsmen were allowed to train apprentices.[3] To obtain a Master craftsman degree, workers had to make large human capital investments, passing three career steps which required substantial time and monetary costs for training and examination. First, workers had to successfully pass a trade test after 2-3 years in apprenticeship, followed by a companionship phase which included further advanced training; until in a third step, workers could take the Master craftsman examination (as shown in Figure 3.1). After 2004, holding a Master craftsman degree as prerequisite to exercise independently a craft business was no longer necessary in the deregulated trades.[4]

The reform raises important questions about deregulation and the consequences for the vocational training market: How did the reform change the careers of apprentices and companions? What are the effects on apprenticeship entry and training levels? And what are the consequences for labor market earnings after apprenticeship training?

A mechanism that provides a direct theoretical link between deregulation and vocational training is based on the effect of competition on the distribution of profits in the product market (see e.g. Blanchard and Giavazzi, 2003). Lower entry barriers should increase competition as more firms enter the market, incentivized by the reduction in entry costs. As a consequence, stronger competition decreases incumbent firms' profits per unit of output, which in turn discourages training investments if training is costly (Bassanini and Brunello, 2011). As a consequence, the demand for apprentices should

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[2]For young school-leavers who do not enter tertiary education, apprenticeship training serves as a way of accumulating high skill levels. For evidence of an increase at the bottom of the German skill distribution, see e.g. Nickell and Bell (1996) and Freeman and Schettkat (2001).

[3]The Master craftsman degree entitled craftsmen to train apprentices (*kleiner Befähigungsnachweis*) and to open up and run their own business (*großer Befähigungsnachweis*).

[4]In addition to the removal of the Master craftsman certificate requirement, it was no longer necessary to complete basic vocational training in the apprenticeship system in order to open and run a business in the deregulated trades after 2004.

Fig. 3.1 Career Path and Market Structure in the Deregulated Crafts and Trades
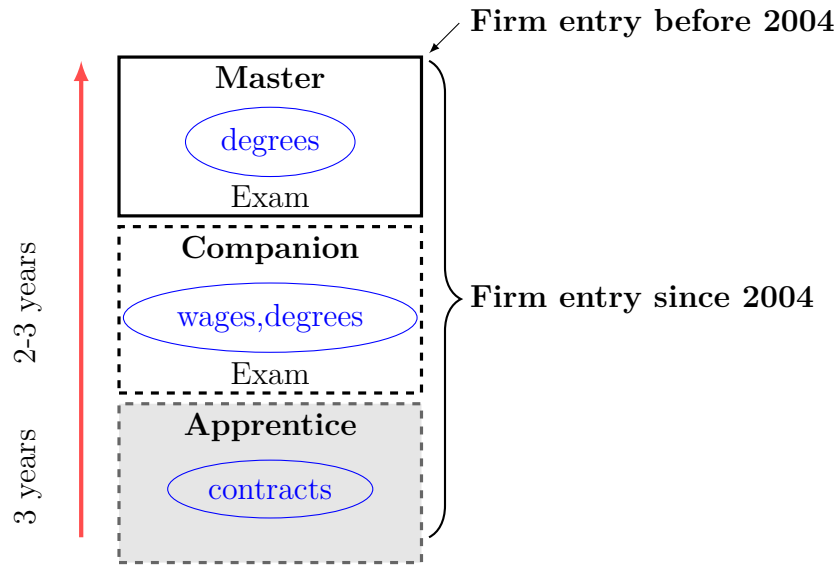
decrease. From a supply-side perspective, the reform reduces the costs of skill investment to become a business owner. This cost reduction provides incentives for school-leavers to start an apprenticeship. At the same time, the supply of apprentices depends on how post-apprenticeship returns change after the reform. If companions open up their own business after training completion, they might earn lower potential profits if competition is strong. If on the other hand companions stay employed, they may receive higher wage offers in the post-training wage bargaining period since outside options have increased (Becker, 1964); or they receive lower wages as profits to the firm decrease (Blanchard and Giavazzi, 2003). As such, it is ambiguous how the apprenticeship market reacts in equilibrium.

To empirically assess the effects of the reform on the vocational training market, I use annual data at the occupation level from the Confederation of Skilled Crafts, spanning five years prior to the reform and up to seven years after the reform (1999-2010). This data set enables me to identify the crafts and trades occupations explicitly. To quantify the reform effects on wages earned after apprenticeship training, I require rich panel data with detailed annual information on the occupation, employment status, wages and job mobility, so that I can track apprentices over time, from the beginning of apprenticeship training up to labor market entry and onwards. Here, I make use of administrative data, which is drawn from social security records of workers and firms in Germany from 2000 up until 2010. This enables me to identify an apprentice's crafts and trades occupation and whether she pursues apprenticeship training in a deregulated or regulated trade.

I have four main results. First, the share of new Master craftsmen relative to potential entrants in the deregulated trades immediately and strongly declines one year after the reform by around 37%. This result is suggestive evidence that less companions follow the career path of a Master craftsman after deregulation. Second, the deregulated trades experience a sizable decline in the number of new apprenticeship contracts by about 19% shortly after the reform and by 25% in 2008 relative to the pre-reform year. Yet, new apprenticeship contracts do not solely convey labor supply decisions since they are tied to firms' labor demand. In particular, I observe a corresponding decline in the demand for apprentices in the deregulated trades of around 15% in 2005 and of around 20% in 2009. These results suggest a negative net effect in the apprenticeship market. One possible explanation is a dominating demand effect since firms facing stronger competition in the product market may receive lower profits, which reduces incentives to invest in training.

Third, I find even stronger long-run responses in the total number of apprentices with a decline of around 16% in the short run and up to 37% six years after the reform. At the same time, the number of passed trade tests plummets in the deregulated trades by around 33% in 2009, suggesting that apprenticeship graduation cannot solely explain the strong negative apprenticeship effects. Particularly, the large negative long-run effect on apprenticeship levels five years after the reform coincides with the drop in training supply. This can be indicative of higher rates of apprentices leaving without a degree due to the changed career opportunities. Interestingly, the growth rate in prematurely cancelled apprenticeship training contracts increases in 2009 by around 3.5% in the crafts and trades. In total, this increase is equivalent to around 1,750 prematurely cancelled training contracts, suggesting that apprentices leaving the market without a degree explain some of the strong negative apprenticeship effects.

Fourth, I find that average wages do not change much for companions who recently completed their apprenticeship training. But I observe substantial heterogeneity in terms of firm size. Craft firms, which are typically smaller firms with less than 50 employees, should experience a stronger reform effect than large firms since they rely primarily on training their own workers compared to hiring graduates from technical colleges or universities. As such, they have higher incentives to keep their best apprentices after training completion as skilled workers. I find that companions in smaller firms experience strong positive wage effects at labor market entry of around 10% two to three years after the reform. These results suggest that small craft firms in the deregulated trades still use apprenticeship training as main recruitment mechanism.[5]

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[5]See ZDH (2019) for a discussion of apprenticeship training investment in small craft firms.

My analysis contributes to several strands of the literature. A large literature in the US analyzes the effects of deregulation on labor market outcomes, such as wages and employment (Card, 1986; Hirsch and Macpherson, 2000), union coverage and wages (Hirsch, 1993; Rose, 1987), worker and executive compensation (Black and Strahan, 2001; Wozniak, 2007; Cunãt and Guadalupe, 2009), unemployment (Ebell and Haefke, 2009) and wage inequality (Fortin and Lemieux, 1997).[6] Most studies find that deregulation has a positive effect on long-run employment growth and increases wage dispersion. However, this paper investigates a different margin by analyzing the effect of deregulation on careers of apprentices.

The literature focusing on entry regulation has analyzed the effect of firm entry state regulations on firm formation and employment growth (Bertrand and Kramarz, 2002; Bruhn, 2011; Branstetter et al., 2013), firm entry (Kaplan et al., 2011), entrepreneurship, the size of entrant firms and growth of incumbent firms (Klapper et al., 2006), firm productivity, profit margins and employment (Schivardi and Viviano, 2011), skill premia (Guadalupe, 2007) and returns to education (Fernandes et al., 2014). Overall, this literature shows that stricter entry regulation reduces employment and increases output prices; while deregulating entry barriers increases firm entry and output growth (Aghion et al., 2008).[7] Yet, little is known about the effect of deregulation on vocational training and labor market outcomes of trained workers. My study analyzes a different margin to the literature by studying how deregulation affects apprenticeship entry and post-training earnings.

My study is closely related to a small but growing literature on the effects of deregulation and competition on training. Theoretical studies predict that increased competition leads to less incentives for firms to invest in general training due to a reduction in the size of economic rents (see e.g. Acemoglu and Pischke, 1999a and Gersbach and Schmutzler, 2012). Most empirical studies have focused on the effect of competition on firms' investments in workplace or on-the-job training. For the US, Autor (2001) uses an aggregate regulation index to determine the effect of competition on induction training in temporary help firms and finds that more competition leads to an increase in firms' training provision. Heywood et al. (2017) also find positive effects of competition on further employer provided training, but only if the competitive pressure does not involve firm solvency.[8]

Closest to my analysis is the study by Bassanini and Brunello (2011) which analyzes the link between deregulation and training across OECD countries. The authors develop

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[6]For a detailed review of the US evidence on deregulation, see Peoples (1998) and for a survey on institutions and earnings inequality in the US, see Katz and Autor (1999).

[7]For cross-country level evidence see Djankov et al. (2002) and Ciccone and Papaioannou (2007).

[8]See Heywood et al. (2017) for a more detailed review of the literature on product market competition and training.

a theoretical model of price competition with endogenous firm entry and test it by exploiting a reduction in entry barriers for European industries in the 1990s across 15 European countries. By using cross-sectional cross-country data, they find that a 10% reduction in product market regulation leads to a 3.2% increase in firm-specific workplace training of full-time employees aged between 25 and 54 years in new and incumbent firms. In contrast, my study provides micro-level evidence on the effects of a major deregulation on apprenticeship training levels in a market where incumbent firms train and its effect on individual post-apprenticeship earnings.

The paper is organized as follows. Section 3.2 discusses the institutional background of the German crafts and trades, the vocational training system and how the reform led to the deregulation of entry restrictions. Section 3.3 presents theoretical considerations, while Section 3.4 describes the data and empirical strategy for the analysis of apprentices and their post-apprenticeship earnings. Section 3.5 reports the results of the analysis and Section 3.6 adds robustness. Finally, Section 3.7 discusses the policy implications of my analysis and concludes.

3.2 Institutional Background

To understand how the deregulation affects the apprenticeship market, this section discusses the main characteristics of the German crafts and trades market and how the reform changed the market access to the deregulated trades.

3.2.1 The Crafts and Trades System in Germany

In Germany strict entry regulations in the crafts and trades sector date back to the Middle Ages, where guilds laid down detailed regulations on who could enter and learn a trade through apprenticeship training and who could become a Master craftsman (*Meister*). In 1953, the Crafts and Trade Code (*Handwerksordnung*) was passed by the German parliament, defining strict entry requirements in the crafts and trades.[9] Specifically, this law defined that only Master craftsmen could start and operate a business in the crafts and trades and train apprentices.

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[9]The Crafts and Trades Code was reformed in 1994 and 1998, enabling Master craftsmen to offer services in related crafts occupations in which they hadn't been formally registered. In 1998, the 127 crafts occupations were combined and reduced to 94 trades allowing craftsmen to offer a broader range of services. In addition, market entry was facilitated for craftsmen from EU countries, who could apply then for an exemption of the entry requirements, which imposed sizeable monetary and opportunity costs to non-German craftsmen. However, the Master craftsman requirement in order to start and run a business remained. Both reforms did not affect the strict entry regulations in the crafts and trades.

To obtain a Master craftsman degree high investments in monetary terms (i.e. examination fees), but also in terms of time costs (i.e. years of training and practical work experience) had to be made. At the beginning of the traditional career path, individuals had to undertake 2-3 years apprenticeship training under the supervision of a Master craftsman. In addition, the employer and the apprentice had to sign a training contract, which pins down the apprentice's pay and the training curriculum. Thereby, the business owner commits herself to provide general and occupation-specific skills at the workplace and guarantees that the apprentice learns the minimum set of skills for completing training in her occupation.[10] In providing apprenticeship training, firms pay largely the costs for training, which amount to around 4,390 euros per apprentice per year (Schönfeld et al., 2016). At the end of the training period, the apprentice would take a trade test (*Gesellenprüfung*) to become a companion. After successful completion she receives a graded skill certificate in her specific trade.[11] Then, companions could continue working at their training firm or work as an employee for another firm within their occupation. To further pursue the career path, the companion had to take advanced training of 2-3 years in part-time in order to take a comprehensive examination for the Master craftsman diploma.[12] The examination of the Master craftsman degree involved trade-specific practical and theoretical parts in which the examinee had to demonstrate her ability to train apprentices in terms of pedagogical skills and the knowledge in business and law that is necessary to independently run a business (§45(2), HWO, 2002).

The trade test and the Master craftsman examination alike were conducted by the local Chamber of Crafts (*Handwerkskammer*). Because of the substantial additional costs in obtaining a Master craftsman degree many qualified companions who had accumulated work experience were deterred from becoming self-employed (Monopolkommission, 1998). These entry requirements and training prerequisites were closely monitored by the local Chamber of Crafts. Every business owner (here Master craftsman) had to enroll in the register for the artisans (*Handwerksrolle*) and pay membership fees to the local Chamber of Crafts. Not registering but operating a business in the crafts and trades is illegal and prosecuted with high fines.

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[10]For a detailed description of the dual structure of the German apprenticeship system, see Soskice (1994), Harhoff and Kane (1997) and Hoeckel and Schwartz (2010).

[11]Also, persons who worked for at least 6 years in their respective trade qualified to take the trade test (§37 HWO, 2002).

[12]Alternatively, advanced training could be taken in full-time lasting around four months up until two years.

3.2.2 The Reform of 2004

The reform of the German Crafts and Trades Code in 2004 changed the entry requirements in the crafts and trades sector. Most importantly, the new law reduced the costs of skill investment to become a business owner by abolishing entry barriers for 53 out of 94 trades. After the reform, workers could start and operate a business without having to invest into a Master craftsman diploma or a trade certificate (*Gesellenbrief*), as shown in Figure 3.1.

Yet, the traditional career path (from apprentice to companion to Master craftsman) was still a possible career trajectory after the reform in the deregulated trades. In addition, training personnel temporarily no longer had to pass a qualifying examination in order to train apprentices in the deregulated trades between 2004 and 2008.[13] The strict entry regulation remained in place in the remaining 41 regulated trades. Only occupations that involved a high risk potential for the craftsman herself and her customers, and which have been more training intensive in terms of apprenticeship training provision, remained regulated.[14]

Further, also the regulated trades experienced a lowering of entry barriers by the 2004 reform. For one, seen as an equivalent to the Master craftsman degree in those professions was a German degree in a related field, such as an engineering title or a degree from technical professional schools. Second, companions who accumulated at least six years of work experience, of which four were in a leading position, could open up a business. Apart from working in trades that involved a high risk potential, companions could also take the Master craftsman examination right after their trade test. Further, business owners who employed Master craftsmen were allowed to open up a business.

In 2000, the European court ruled that the German Crafts and Trade Code was incompatible with the idea of free rendering of services by EU law (Art. 49 of the EU-Treaty).[15] In response, market access was eased in the German crafts and trades sector. The first law proposal passed the Federal Council of Germany on November 27th, 2003; and the second, revised version, passed the German Bundestag on December 19th, 2003, while the reform came almost immediately into force on January 1st, 2004.

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[13]See BMBF (2004, p.132) for reference on the training offensive and Ulmer and Gutschow (2009) for a survey analysis of the temporary suspension and its effects on training firms.

[14]See Table B.1 in the Appendix for a list of the number of occupations that were deregulated and those that remained regulated.

[15]Before the ruling of the EU court (see Rs.C-58/98-Josef Corsten, EuZW 2000, 763), the monopolies commission argued for a reduction in entry barriers in the German crafts and trades (see Monopolkommission, 1998).

Fig. 3.2 Trends in the Ratio of Successful Master Craftsman Examinations to Passed Trade Tests Growth by Deregulated and Regulated Trades
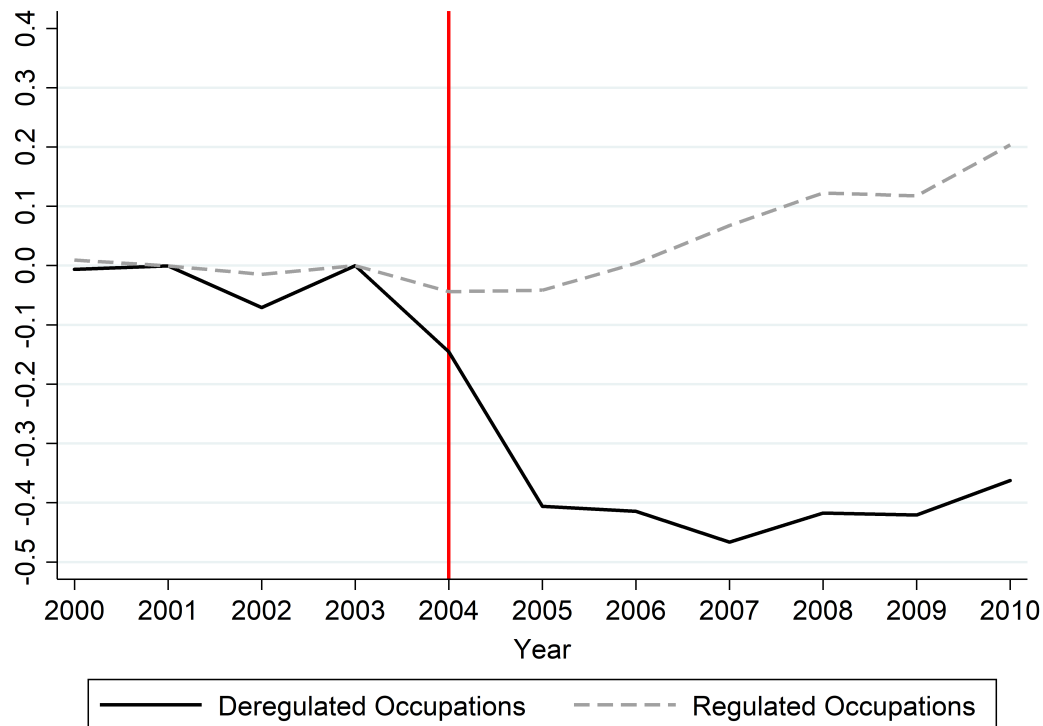
Notes: The figure plots the growth rate of the share of passed Master craftsmen examinations relative to passed trade tests, which is lagged by two years, relative to 2003 (pre-reform period) by deregulated and still regulated occupations. The vertical line indicates the reform year in 2004.
Source: ZDH sample, 2000-2010.

The deregulation of the crafts and trades code in 2004 is reflected in the share of craftsmen who pursue a Master craftsman degree (as shown in Figure 3.2). Before the reform, the trend growth rate in the share of Master craftsman entrants relative to the potential pool of Master craftsmen is constant and parallel in both deregulated and still regulated trades. One year after the reform the pattern changes and the growth rate of Master craftsman entry drops sharply by about 40% in 2005. As a result of the deregulation, the growth rate of Master craftsmen declined from -14.5% in 2004 to around -46.6% in 2007, three years after the reform, suggesting that less companions pursue a Master craftsman degree.

3.3 Theoretical Considerations

To understand how the crafts and trades reform affects the vocational training market and post-apprenticeship outcomes two changes need to be distinguished. The first major change of the reform takes place at the product market, where firms produce goods and services, and offer apprenticeship training. There, lower entry barriers should increase

competition as more firms enter the market, incentivized by the reduction in entry costs.[16] At the same time, stronger product market competition reduces incumbent firm's profits per unit of output which results in a negative effect on the size of economic rents that accrue to the firm (Blanchard and Giavazzi, 2003). Bassanini and Brunello (2011) extend a general equilibrium model of imperfect product and labor markets with endogenous firm entry and transfer it to firm's training investment decisions. They predict that the negative rent effect translates into a decline of training investments of incumbent firms.[17] Yet, firms can still use training as a screening and recruitment tool, i.e. to select the best companion, and avoid hiring or other training costs.[18]

The second change of the reform applies to apprentices in the deregulated crafts and trades and their career paths. For them, the reform reduces the costs of skill investment to become a business owner. Instead of pursuing the traditional career path, which comprises two additional training periods after training completion (companion and Master, see Figure 3.1); they could start and operate their own business without further skill investments. Consequently, the deregulation provides incentives for school-leavers to enter apprenticeship training (Becker, 1962). Thus, the reform should lead to an increase in individual labor supply. However, one has to consider that at this early career stage opening up a business still represents an entry barrier for young companions who do not have the experience and financial funds compared to incumbent craftsmen with more labor market experience, who may have worked already for years in their business.

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[16]In the literature on human capital theory, Becker (1962, 1964) shows that under perfect competition employers do not invest in general human capital, i.e. portable skills, due to the risk of poaching. But recent empirical studies have shown that firms provide general training in markets with labor market frictions and a compressed wage structure (Acemoglu and Pischke, 1998, 1999b and Dustmann and Schönberg, 2009; Katz and Ziderman, 1990), which make general skills de facto firm-specific.

[17]A large part of the literature on industrial organization theory has shown that product market competition increases firm's incentives to lower unit costs through which firms can offer lower product prices (e.g. Aghion et al., 1997; Boone, 2000; and Vives, 2008). Linking this profit-cost relationship to firm's incentives to invest in workplace training, Bassanini and Brunello (2011) predict an additional, yet opposing effect if the number of firms is allowed to vary. Specifically, they expect firms' marginal benefits of training per worker to increase due to the higher sensitivity of output to relative prices, which in turn fosters training investments. Yet, these opposing effects are based on two assumptions which do not apply to the German apprenticeship market. First, workplace training should instantly increase the productivity of the trainee at the firm, which in turn increases the output gains from training that are positively correlated with the elasticity of demand. In contrast, the timing of productivity is a central part of the learning process in apprenticeship training. A worker who invests in human capital by starting an apprenticeship is not productive at the beginning of the training period, but becomes more productive after two to three years in the crafts and trades (see e.g. Becker, 1964 and Acemoglu and Pischke, 1999a). Second, these effects are based on the assumption that through endogenous firm entry, the elasticity of product demand increases and firms face higher output gains from lowering prices and hence from training their employees. Contrasting this, the reform shows that entry deregulation leads to a strong inflow of self-employed workers mostly without employees who do not face the incentives to invest in apprenticeship training (see Gathmann and Lembcke, 2019).

[18]See e.g. Stevens (1994). For a review of the literature on information asymmetries and training, see Wolter and Ryan (2011).

Further, the overall supply effect depends on post-apprenticeship returns. If on the one hand, companions become a business owner right after apprenticeship completion, they may face high upfront costs and less potential profits in the competitive market. If on the other hand, companions stay employed, they may face higher wage offers in the post-apprenticeship bargaining period since outside options have increased (Becker, 1962), or lower returns if firms pass on lower profits to their workers (Blanchard and Giavazzi, 2003).

How apprenticeship entry and post-apprenticeship wages react in equilibrium depends on the net effect of labor supply and demand in the vocational training market. New firm entrants can potentially offer lower product prices than incumbent firms if they are self-employed, likely without employees and do not have to pass on value added taxes (VAT) to the consumer.[19] This should lead to a strong negative effect on incumbent firm's training investment via the competition channel in the product market. Since the negative rent effect in the product market discourages firms to invest in training, I expect a decline in firm's training investment and consequently a decrease in the demand for apprentices in the deregulated trades. Below, I will identify the net effect of the deregulation on apprenticeship training and post-apprenticeship outcomes.

3.4   Data and Empirical Strategy

My empirical analysis is based on two main data sources. The first data source consists of administrative data from the German Confederation of Skilled Crafts (ZDH, *Zentralverband des Deutschen Handwerks*) and the second data source comprises German social security records.

3.4.1   Administrative Crafts and Trades Records

To study the effects of deregulation on the apprenticeship market, I use data on administrative records from the German Confederation of Skilled Crafts from 1999 to 2010, which cover the entire population in the crafts and trades sector. The data are available at the craft occupation-year level and provide information on the total number of new apprenticeship contracts, the total number of apprentices, passed trade tests,

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[19]Self-employed in the crafts and trades sector who indicate revenues below 17,500 euro are not subject to VAT and consequently do not have to charge the customer VAT (see §19 UStG). For evidence on an increase in solo self-employment after deregulation see Gathmann and Lembcke (2019).

passed Master craftsman examinations, and the total number of firms in the crafts and trades as of December 31st each year.[20]

The dataset is well suited to analyze the overall effect of deregulation on vocational training since it enables me to exactly identify the number of new apprenticeship contracts to approximate apprenticeship entry and the number of passed Master craftsman examinations for Master craftsman entry in the crafts and trades sector.

To further infer individual-level information that is trade-related, I match the ZDH data to employment-related information from the "Occupations as Reflected in Statistics, IAB" database according to the three-digit occupational classification.[21] I further distinguish between broad trade groups (*Gewerbegruppen*) which are comparable to industries.[22] Thus, I can identify 72% of the deregulated occupations and 83% of the still regulated occupations.

3.4.2   Social Security Records

To analyze the effect of deregulation on post-apprenticeship wages, I employ a two percent random sample of administrative records which comprises the population of workers and plants covered by the social security system, spanning from 2000 to 2010. Not included are self-employed, civil servants, nor military employees.

This data source contains detailed information at the person-year level which is linked to plant-year level information from the Establishment History Panel. Thus, I can follow individuals from apprenticeship training to labor market entry onwards and over time, even if they leave their occupation or change jobs. Via the three-digit occupation code I can classify each apprentice as working in a job spell of a deregulated occupation or a still regulated trade. Overall, I can classify 83% of the deregulated occupations and 95% of the remaining still regulated trades.

For each individual I observe whether she is employed within the social security system or whether she collects unemployment benefits as of June 30 each year. The wage variable records the average daily wage for the employment spell that contains this reference date. Like most social security data, the wage variable is right-censored at the social security limit. I impute censored wages, assuming that the error term in the wage regression is normally distributed, thereby allowing for separate variances by

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[20]The terms firm and plant are used interchangeably here since the data does not distinguish between the two.

[21]The database consists of aggregate employment statistics which are drawn from individual administrative social security records, specifically employment statistics at the occupation-year level.

[22]The trade groups are: 1 main construction and finishing trades; 2 electrical and metal trades; 3 woodworking trades; 4 clothing, textile and leather trades; 5 food trades; 6 health, hygienic, chemical and cleaning trades and 7 glass, paper, ceramic and other trades.

year and gender for East and West Germany separately. I deflate wages to 2014 prices using the consumer price index and drop post-apprenticeship wages with missing or very low wages, i.e. where the daily real wage is below 20. Since the first wage spell for stayers after apprenticeship training completion is an average wage of apprenticeship training and regular employment, wages only from the second wage spell onwards are considered for companions who stay with their training firm. For part-time employed workers (who work less than 30 hours per week), wages are reweighted to convert them into full-time equivalents (FTE): wages for those working 18-30 hours are divided by 0.6, while wages for those working less than 18 hours are divided by 0.3. To reduce missings and misreporting in the education variable I use the imputation method of Fitzenberger et al. (2006).

Further, I exclude irregular, marginal and seasonal employment. The sample is restricted to individuals with at least one spell in apprenticeship. High-skilled individuals with a technical college or university degree sometime during their future career are excluded from the sample. I drop individuals with at least one spell as unpaid family members (*mithelfende Angehörige*) and individuals for which the training occupation is not yet determined. Further, individuals who work at home (*Heimarbeit*) and individuals with an overall apprenticeship training duration of less than one year and more than four years are excluded. I further exclude individuals who change firms during their apprenticeship training, as well as individuals who start more than one apprenticeship, or who work as interns. Further, labor market entrants are excluded who are older than 30 years of age. Since craft firms are on average small to medium firms, workers in firms with more than 1,500 employees are excluded. I end up with a sample of 27,724 workers, of which around 3,937 are trained in deregulated occupations and 24,912 in still regulated occupations.

3.4.3 Descriptive Statistics

Comparing occupation-level characteristics for deregulated and still regulated occupations before and after the reform, Table 3.1 reports means in 2003 (Panel A) and in 2008 (Panel B), four years after the reform, based on the ZDH data. Starting out with the pre-reform characteristics, Panel A shows that the average number of apprentices is around 21 times larger in the still regulated trades than in the deregulated trades, which matches the treatment assignment criterion of training intensive occupations in the institutional background. Panel B shows that after the reform the number of firms in the deregulated trades sizably increased to around 4,432 firms; as such the number of registered firms more than doubled compared to the pre-reform year, leading to increased competition in the deregulated trades. At the same time the number of

Master craftsman examinations in the deregulated trades declined by 59% after the reform relative to 2003. Thus, creating sizable variation in the deregulated trades.

Table 3.1 Descriptive Statistics for the ZDH Data

| | Deregulated Occupations | | Regulated Occupations | |
| | Mean | S.D. | Mean | S.D. |
	(1)	(2)	(3)	(4)
Panel A: Pre-Reform (2003)				
New apprenticeship contracts	212.474	406.361	4,034.294	5,449.877
Number of apprentices	577.368	1,049.988	12,144.350	17,190.370
Passed trade tests	194.053	376.453	3,371.765	4,601.163
Passed Master craftsman exams	54.368	105.200	713.853	934.860
Firms:				
Number of firms	1,806.658	2,781.833	17,202.320	20,260.630
Firm entry	121.921	241.123	1,199.559	1,541.888
Firm exits	142.842	210.803	1,270.500	1,528.669
Employees:				
Female	35.363	24.260	14.635	22.396
Foreign	9.589	6.003	7.591	5.256
High school diploma (Abitur)	1.792	1.932	1.447	1.814
Panel B: Post-Reform (2008)				
New apprenticeship contracts	195.605	369.585	3,931.382	5,401.025
Number of apprentices	531.553	985.843	11,310.440	16,121.130
Passed trade tests	152.605	283.031	2,859.559	4,038.040
Passed Master craftsman exams	22.421	32.290	603.294	896.052
Firms:				
Number of firms	4,432.053	10,863.740	17,636.060	21,430.420
Firm entry	865.816	2,432.615	1,108.588	1,507.753
Firm exits	615.632	1,760.702	1,142.794	1,417.104
Employees:				
Female	34.932	23.949	15.065	22.696
Foreign	9.184	5.440	7.356	5.042
High school diploma (Abitur)	2.213	2.201	1.606	2.046
Number of observations	494		442	

Notes: Columns (1) and (3) compare means for deregulated and still regulated occupations in the pre-reform year 2003 (Panel A) and in the post-reform year 2008 (Panel B). The corresponding standard deviations are given in columns (2) and (4). The total number of firms is based on the number of registered firms and includes self-employed craftsmen. *Source*: ZDH Occupations as Reflected in Statistics, ZDH-IAB sample, 1999-2010.

Table 3.2 Summary Statistics in 2003

| | Deregulated Occupations | | Regulated Occupations | |
| | Mean | S.D. | Mean | S.D. |
	(1)	(2)	(3)	(4)
Currently in apprenticeship	0.317	0.466	0.404	0.491
and male	0.755	0.431	0.865	0.341
and female	0.245	0.431	0.135	0.341
High school diploma (Abitur)	0.040	0.195	0.014	0.115
Apprenticeship duration (in days)	1,013.150	247.356	1,051.730	292.184
Age	23.085	3.522	22.179	3.314
Age at beginning of first job	21.743	2.184	21.372	1.827
Daily real wage	86.254	24.901	74.513	21.158
FTE daily real wage	86.819	25.323	74.832	21.138
Log FTE daily real wage	4.420	0.305	4.272	0.303
Log FTE daily real wage (2-year window)	0.105	0.378	0.064	0.239
Log FTE daily real wage (5-year window)	0.061	0.221	0.040	0.169
Part-time	0.009	0.094	0.006	0.079
Female	0.189	0.391	0.125	0.331
East German	0.197	0.398	0.228	0.420
Foreign cizizenship	0.068	0.251	0.043	0.202
Apprentices at the Firm:				
1-3	0.352	0.478	0.408	0.492
4-10	0.172	0.378	0.193	0.394
11-20	0.089	0.285	0.070	0.256
>20	0.158	0.365	0.112	0.316
Age Groups:				
16-25	0.750	0.433	0.825	0.380
26-50	0.250	0.433	0.175	0.380
Firm Size:				
1-19	0.365	0.482	0.517	0.500
20-100	0.218	0.413	0.277	0.447
101-500	0.275	0.447	0.139	0.346
501-1500	0.135	0.342	0.052	0.222
Industry:				
Agriculture and Fishing	0.001	0.036	0.001	0.038
Energy and Mining	0.004	0.062	0.004	0.066
Food	0.031	0.173	0.052	0.222
Consumer Goods	0.225	0.418	0.025	0.156
Producer Goods	0.230	0.421	0.040	0.197
Investment Goods	0.083	0.275	0.145	0.352
Construction	0.219	0.414	0.342	0.474
Retail Trade	0.068	0.253	0.226	0.418
Transport and Communications	0.000	0.000	0.009	0.095
Finance and Insurance	0.000	0.000	0.000	0.012
Hotel and Restaurant Industry	0.001	0.036	0.004	0.062
Educational Services	0.026	0.159	0.028	0.164
Health, Veterinary and Social Services	0.005	0.072	0.008	0.090
Corporate Services	0.089	0.285	0.040	0.196
Other Services	0.018	0.133	0.075	0.264
Observations	785		7,043	

Notes: The table compares means in 2003 for deregulated and still regulated occupations in columns (1) and (3) and the respective standard deviations in columns (2) and (4).
Source: IAB post-apprenticeship sample, 2000-2010.

Turning to the social security records, Table 3.2 provides summary statistics for deregulated and still regulated trades in the pre-reform year. Columns (1) and (3) show that apprenticeship duration with around 3 years on average is the same for deregulated and still regulated trades before the reform. Also the age at labor market entry is quite similar for both groups. The average distribution of the number of apprentices at the firm is quite similar in deregulated and still regulated occupations with a higher share at the bottom, i.e. having 1-3 apprentices at the firm, than at the top with more than twenty apprentices at the firm. Since craft businesses are on average smaller firms, containing less than five employees, and the data doesn't distinguish between craft and industry firms, the results of this study will deliver stronger reform effects for small firms.[23] Table 3.2 shows indeed that the number of firms with 1-19 employees has the highest fraction, both in deregulated and still regulated trades.

3.4.4 Empirical Strategy

Baseline Estimation at the Occupation-Year Level

In a first step, based on the administrative ZDH sample, I analyze the effect of deregulation on vocational training at the occupation-year level, specifically apprenticeship entry, the number of apprentices and companionship entry by comparing occupations that are deregulated in 2004 to still regulated occupations after the reform. Thus, variants of the following model are estimated:

$$
\begin{aligned}
Y_{j(o)t} = \alpha + \delta\ Treated_o + \sum_{t=2004}^{2010} \beta_t\ (Treated_o * dYear_t) \\
+ \sum_{t=1999}^{2002} \gamma_t\ (Treated_o * dYear_t) + \tau_t + \mu_{j(o)} + \varepsilon_{j(o)t},
\end{aligned}
\tag{3.1}
$$

where the dependent variable $Y_{j(o)t}$ denotes a labor market outcome for occupation o, in trade group j, at time t. Specifically, the labor market outcomes are the natural logarithm of the number of new apprenticeship contracts, the number of apprentices, the number of passed trade tests and the ratio of passed Master craftsman examinations relative to passed trade tests. $Treated_o$ is a dummy variable, indicating whether an occupation is deregulated (being one) or still regulated (being zero). The variable $dYear_t$ comprises calendar year indicators. For the main analysis, four years before the reform (from 1999 to 2002) are considered and up to seven years after the reform (i.e. from 2004 to 2010). Equation (3.1) controls for calendar year fixed effects (τ_t),

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[23]Based on the Federal Statistical Office (2013), around 93 percent of all craft businesses have 1-19 employees in 2010, where most of the craft businesses employ less than five employees.

accounting for aggregate shocks that are common across trade groups, and trade group fixed effects ($\mu_{j(o)}$) to account for time-invariant differences across trade groups. In some specifications, also trade group-specific trends are included to control for differential trends at the trade group level that vary across time. Standard errors are clustered at the occupation level, thus allowing for the error terms to be correlated within occupations over time.

Here, the key identifying assumption is that outcomes in the control (still regulated) occupations and in the treated (deregulated) occupations evolved parallel in the pre-reform period. The validity of this assumption can be tested by assessing if treated and control occupations experience a similar trend in $Y_{j(o)t}$ prior to the reform in 2004. Figure B.1 in the Appendix shows the growth rate in the number of new apprenticeship contracts for deregulated and still regulated trades, depicting parallel trends in the pre-reform period. After 2004, the growth rate in new apprenticeship contracts declines steadily in the deregulated occupations relative to 2003. Strikingly, the gap between deregulated and still regulated trades widens up until 2009 with a sizable decline of around 20% less new apprenticeship contracts in the deregulated trades. Further, Section 3.5 shows that the γ_t coefficients are close to zero and not statistically different from zero.

3.4.5 Empirical Model for Post-Apprenticeship Wages

To analyze the effect of deregulation on companions' wages at labor market entry, I estimate variants of the following model based on the sample of social security records:

$$
\begin{aligned}
Y_{i(o)st} = {} & \alpha + \delta\, Treated_{i(o)t} + \sum_{t=2004}^{2010} \beta_t\, (Treated_{i(o)t} * dYear_t) \\
& + \sum_{t=2000}^{2002} \gamma_t\, (Treated_{i(o)t} * dYear_t) + \delta_t X_{i(o)t} + \tau_t + \mu_o + \theta_s + \varepsilon_{i(o)st},
\end{aligned}
\tag{3.2}
$$

where the superscript t denotes the calendar year for individual i, in occupation o and in state s; and the dependent variable, $Y_{i(o)st}$, is the outcome variable of interest, i.e. the natural logarithm of a companion's post-apprenticeship wage three years before ($2000-2002$) and seven years after ($2004-2010$) the reform. $Treated_{i(o)t}$ is a dummy variable indicating whether an individual works in an occupation that is deregulated (being one) or still regulated (being zero). The variable $dYear_t$ comprises calendar year indicators. Equation (3.2) controls for calendar year fixed effects (τ_t) to account for aggregate shocks that are common across individuals, occupation fixed effects (μ_o) to account for time-invariant differences in occupations, and state fixed effects (θ_s) to account for state-level differences in earnings. Including covariates, $X_{i(o)t}$ comprises

information on age, age squared, a dummy for gender, a dummy for foreign nationality, a dummy for a high school degree (*Abitur*) and log firm size. Equation (3.2) is estimated for companions within a two-year window starting at labor market entry to capture the effects of deregulation on post-apprenticeship wages. The coefficients of interest are β_{2004} to β_{2010}, which measure the reform effect on wages for treated individuals in the post-reform period relative to 2003.

My identification here relies on the assumption that individuals in treated and control occupations experience similar trends in the dependent variable prior to the deregulation. I show in Section 3.5 that the coefficients γ_{2000} to γ_{2002} in equation (3.2) are close to zero and statistically insignificant.

3.4.6   Potential Confounding Events

Another main assumption of the estimation strategy used here is that no other confounding factors, such as regulatory changes or events, affect the deregulated trades within the sample period from 1999-2010. As part of the Hartz reforms, small business startups, stemming from the Me Inc. (*ICH AG*) start-up subsidies for unemployed, were introduced at the beginning of 2003 and ended in 2006 (Deutscher Bundestag, 2003, pp. 4055-4056).[24] Replacing this subsidy in 2006, a new start-up grant was introduced, the so-called *"Gründungszuschuss"* (Caliendo et al., 2009). One concern is that these subsidies might have encouraged entry into the deregulated trades that would not have happened without the subsidy programs. Since these subsidies were mainly targeted at solo self-employed, the events are not likely to confound the post-apprenticeship wage results as they are based on dependent employment. Yet, they may potentially affect the vocational training results via increased competition in the deregulated professions, which may bias the results upwards. However, using the German Micro Census, Rostam-Afschar (2014) finds that the effect of deregulation on self-employment is not driven by the entrepreneurial subsidies.

Further, the EU enlargements in 2004 and 2007 coincide with the reform period, opening the labor market for craftsmen from ten new accession countries in 2004 and two additional countries in 2007.[25] Germany restricted the immediate and full labor market access of the new members by introducing a transitional period of seven years until full freedom of movement was granted. However, this transitional regulation does not apply to self-employed workers. New EU citizens from the accession countries could become self-employed on the condition of having sufficient monetary funds and health care

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[24]See also Baumgartner and Caliendo (2008).

[25]Estonia, Latvia, Lithuania, Poland, Slovak Republic, Slovenia, Czech Republic and Hungary joined the EU as of 01-05-2004 and Bulgaria and Romania joined the EU on the 01-01-2007.

coverage.[26] These confounding events do not directly affect my results on vocational training, since these new crafts and trades businesses needed to be registered in the German VET system in order to train apprentices, and presumably will affect mostly solo self-employment. Consequently, these events may indirectly affect my results by increasing competition in the deregulated trades through self-employment and depress incumbent's incentives to invest in training. However, Rostam-Afschar (2014) and Gathmann and Lembcke (2019) show that the reform effect on self-employment is not driven by the Eastern European EU enlargement.

3.5 Empirical Results

I start out investigating the effects of the reform for the vocational training market. Human capital theory predicts an increase in job opportunities and consequently higher apprenticeship entry due to the removal of necessary career investments in the deregulated trades. Yet, if competition increases, these additional job opportunities may be taken by more experienced incumbent or foreign craftsmen. Further, firms might hire less apprentices on the demand side if product market competition is strong, which decreases the incentives to invest in training. Thus, my estimates would identify a net effect of the responses on the supply and demand side.

3.5.1 Deregulation and New Apprenticeship Contracts

Turning first to entry into the apprenticeship training market, I begin by studying new apprenticeship contracts after the reform. Figure 3.3 plots the coefficient estimates of the baseline specification according to equation (3.1), additionally controlling for linear trade group-specific time trends. All pre-reform coefficient estimates are not statistically different from zero, suggesting that the log number of new apprenticeship contracts evolved parallel in deregulated and still regulated trades prior to the reform. After 2004 the effects change, although negative at first, the effects are not statistically different from zero in the reform year, nor one year later. But two years after the reform the number of new apprenticeship contracts declines sizably by around 19% in the deregulated trades compared to the still regulated trades and up to 25% in 2008. One possible explanation for this delayed effect is that firms' adjustment in terms of apprenticeship training takes place some time after other competitors have entered the market.

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[26]For an overview see Fehrenbacher (2012).

Fig. 3.3 Effect of Deregulation on New Apprenticeship Contracts

Table 3.3 shows the coefficients for deregulated trades for four pre-reform years (1999−2002) and seven years after the reform (2004−2010), relative to 2003. Column (1) depicts the coefficient estimates of the baseline specification according to equation (3.1), controlling for year fixed effects and trade group fixed effects; while column (2) additionally controls for linear trade group-specific time trends. Two years after the reform the number of new apprenticeship contracts declines sizably by around 20% in the deregulated trades compared to the still regulated trades and up to 27% in 2008.[27] When including trade group-specific trends these effects become somewhat smaller but similar in magnitude.

Mirroring the decline in new apprenticeship contracts, Figure B.2 in the Appendix shows a drop in the demand for apprentices, proxied by the growth of training slots, of around 15% in 2005 relative to the reform year, which decreases even further with around 20% five years after the reform. The evidence suggests that increased competition led to stronger demand-side responses and that few supply-side adjustments took place.

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[27]In terms of effect size, these effects amount to around -43 new apprenticeship contracts per occupation two years after the reform and around 58 less new apprenticeship contracts in 2008.

Table 3.3 Effect of Deregulation on the Vocational Training Market

|	New Apprenticeship Contracts		Apprentices		Passed Trade Tests	
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulated (t=1999)	0.003	-0.012	0.030	0.015	-0.044	-0.003
	(0.083)	(0.087)	(0.066)	(0.076)	(0.105)	(0.119)
Deregulated (t=2000)	0.087	0.077	0.005	-0.006	-0.063	-0.033
	(0.110)	(0.111)	(0.056)	(0.062)	(0.116)	(0.126)
Deregulated (t=2001)	-0.013	-0.021	0.007	0.000	-0.096	-0.076
	(0.073)	(0.074)	(0.042)	(0.044)	(0.117)	(0.123)
Deregulated (t=2002)	0.139	0.137	-0.007	-0.011	-0.042	-0.032
	(0.102)	(0.103)	(0.024)	(0.026)	(0.079)	(0.082)
Deregulated (t=2003)						
Deregulated (Reform year)	-0.027	-0.023	0.035	0.038	-0.081	-0.091
	(0.077)	(0.077)	(0.043)	(0.044)	(0.105)	(0.103)
Deregulated (t=2005)	-0.085	-0.077	-0.030	-0.023	0.019	-0.001
	(0.072)	(0.076)	(0.050)	(0.054)	(0.098)	(0.094)
Deregulated (t=2006)	-0.203**	-0.191*	-0.139*	-0.129*	0.077	0.045
	(0.090)	(0.098)	(0.071)	(0.074)	(0.140)	(0.133)
Deregulated (t=2007)	-0.207**	-0.191*	-0.177**	-0.163**	-0.225*	-0.266**
	(0.098)	(0.100)	(0.076)	(0.080)	(0.116)	(0.117)
Deregulated (t=2008)	-0.271**	-0.252**	-0.205**	-0.188*	-0.043	-0.097
	(0.123)	(0.121)	(0.098)	(0.098)	(0.143)	(0.150)
Deregulated (t=2009)	-0.195*	-0.175	-0.315***	-0.294***	-0.332**	-0.395**
	(0.114)	(0.110)	(0.110)	(0.106)	(0.147)	(0.163)
Deregulated (t=2010)	-0.198	-0.177	-0.390***	-0.365***	-0.308*	-0.380**
	(0.135)	(0.134)	(0.129)	(0.124)	(0.159)	(0.166)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	-	-	-	-	-	-
Trade group-specific trend	-	Yes	-	Yes	-	Yes
Observations	857	857	864	864	857	857
Adj. R-squared	0.457	0.454	0.463	0.460	0.416	0.413

Notes: The table reports estimates of deregulation on the number of new apprenticeship contracts (columns (1)−(2)), the number of apprentices (columns (3)-(4)) and the number of passed trade tests (columns (5)-(6)) based on equation (3.1) including a constant. The dependent variable in all specifications is measured in natural logs. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: ZDH-IAB sample, 1999-2010.

One of the main reasons for this is that opening up a business at an early career stage still represents an entry barrier for young companions compared to more experienced workers. Further, the tendency to enter tertiary education has increased from 2005 up until 2010 (see Figure B.3 in the Appendix).

Turning next to the question how the total number of apprentices responds to the reform, Figure 3.4 and columns (3)−(4) in Table 3.3 show the results for the post-reform

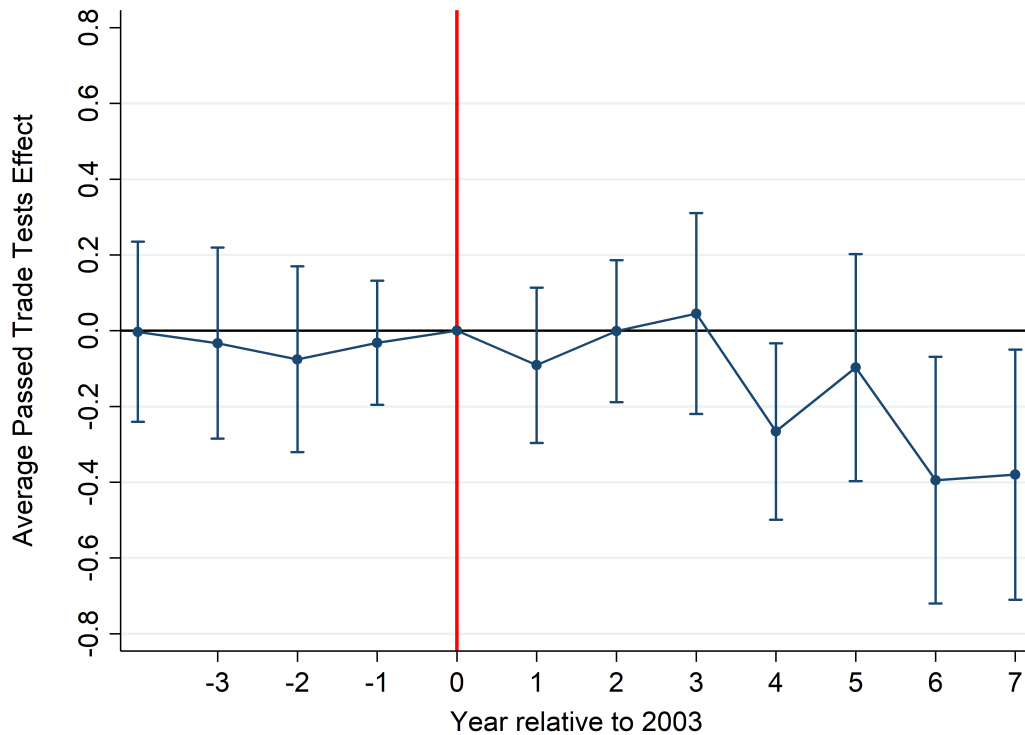Fig. 3.4 Effect of Deregulation on Apprentices
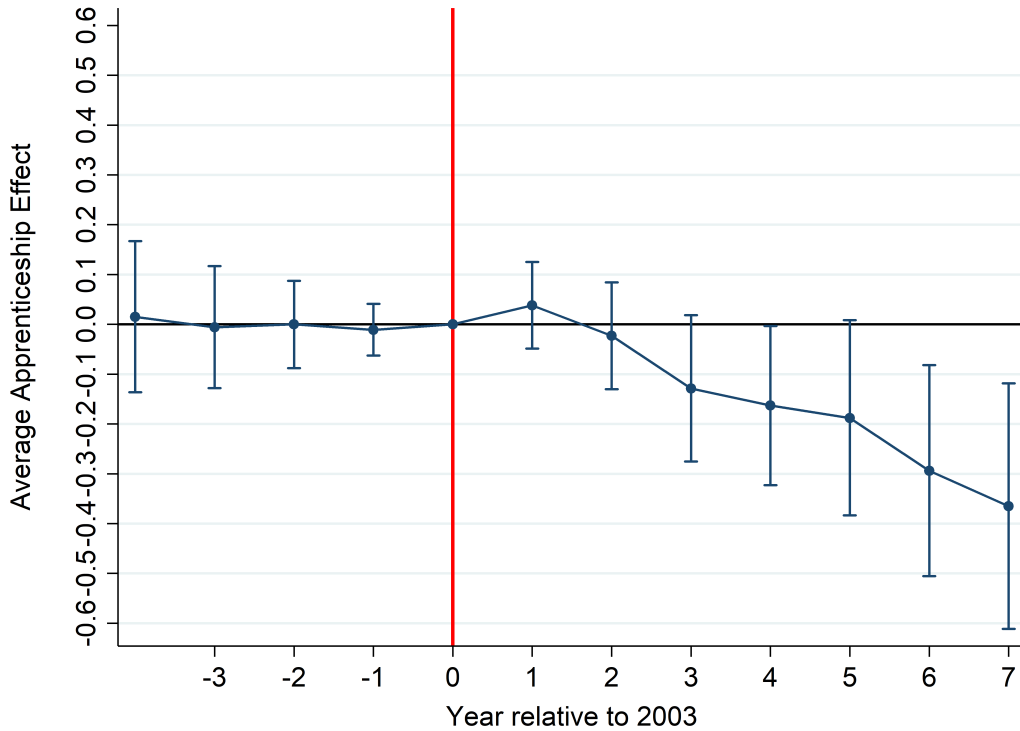
Notes: The figure plots estimates and the 95% confidence interval of the effect of deregulation on the number of apprentices based on equation (3.1), including a constant and controlling for crafts sector-specific trends. The dependent variable is the natural log of the total number of apprentices at the occupation-year level. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: ZDH-IAB sample, 1999-2010.

effects in the deregulated occupations relative to 2003. The specifications are the same as before. In fact, the reform effect is positive in 2004, but not statistically different from zero in the deregulated trades compared to the still regulated trades. After 2004 columns (3)−(4) provide a mirror image of the decline in apprenticeship entry in the deregulated trades. Column (4) shows a strong negative reform effect on the number of apprentices, with the deregulated trades experiencing a decline of around 18% three years after the reform and up to 32% in 2009, accumulating further to 39% in 2010.[28] When including trade group-specific trends in column (4) these effects become somewhat smaller but remain highly significant, as can be seen in Figure 3.4.[29]

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[28]Evaluated at the pre-reform mean these effects are sizable and amount to a decline of around 102 apprentices per occupation shortly after the reform, of 182 apprentices in 2009 and 225 apprentices in the long run.

[29]There appears to be a stronger decline in the number of apprentices in 2009. As mentioned in Section 3.2, the reintroduction of the trainer ordinance on aptitude test may be one explanation for this decline. After 2009, where a trainer certificate was again mandatory in the deregulated trades, one observes large negative reform effects which almost double in size compared to the early post-reform years.

Fig. 3.5 Effect of Deregulation on Passed Trade Tests

Given that the results on apprenticeship entry are lower in terms of magnitude, it seems surprising that apprenticeship levels decline faster than apprenticeship entry after the reform. Yet, lower entry barriers and stronger competition in the deregulated crafts and trades sector might increase the number of apprenticeship graduates. Or in contrast, more apprentices might leave their training firms without a degree, because of the new career opportunities. Analyzing first skilled entry into the crafts and trades (here the number of passed trade tests), columns (5)−(6) of Table 3.3 rerun the same specifications as on the left hand side. Similar to the apprenticeship results, Figure 3.5 shows sizable long-run effects of deregulation. Especially in 2007, 2009 and 2010 the effects on passed trade tests in the deregulated trades accumulate from 27% up to 38% less successful apprenticeship graduates as in the still regulated trades.[30] These results suggest that apprenticeship graduation cannot solely explain the strong negative effect on the number of apprentices in the deregulated trades. Particularly, the large negative long-run effect on apprentices five years after the reform coincides with the drop in

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[30]Figure B.4 in the Appendix plots the growth rate of passed trade tests relative to the potential pool of companions, indicating that relative to 2003 less apprentices enter companionship in 2004 and 2009.

training supply (as shown in Figure B.2 in the Appendix). This suggests that a higher rate of apprentices who leave without a degree may explain the strong negative effect on training levels. Confirming this interpretation, Figure B.5 in the Appendix shows the growth rate of prematurely cancelled training contracts, which increases notably in 2009 by about 3.5% relative to 2004. In terms of magnitude, this increase is sizable relating to around 1,750 prematurely cancelled training contracts in the crafts and trades compared to 2004, which explains the discrepancy between the two outcomes.

Overall, these results are in line with the theoretical competition channel that stronger competition in the product market decreases economic rents, which in turn discourages firms to invest in vocational training (Bassanini and Brunello, 2011). In particular, the effects imply substantial changes in terms of apprenticeship entry, the number of apprentices and firms' training investments in response to the reform. Although the costs of human capital investments to become a business owner decline after the reform, I observe small labor supply effects given that entry barriers are still high for apprenticeship graduates compared to more experienced craftsmen.

3.5.2 Deregulation and Wages of Companions at Labor Market Entry

The evidence so far shows that the deregulation had a negative long-run impact on the vocational training market. As a consequence, the declining number of apprentices might result in a reduction in the supply of trained companions. Yet, wages in the post-apprenticeship bargaining period might increase if firms want to keep their skilled workers since companion's outside options have increased. At the same time, they might decrease if firms pass on the reduction in rents to their workers. Thus, I next investigate how the reform has affected wages of companions at labor market entry.

Table 3.4 shows the coefficient estimates of the reform for companions in the deregulated occupations compared to companions in still regulated occupations within a two-year window starting at labor market entry. Column (1) controls for year fixed effects, occupation fixed effects and state fixed effects; while column (2) presents the baseline estimates, additionally controlling for the covariates age, age squared, dummies for gender and foreign nationality, a dummy for a high school degree (*Abitur*) and log firm size. Column (3) adds industry fixed effects and column (4) further includes linear industry-specific trends. Figure 3.6 plots the corresponding coefficient estimates from column (4). In the pre-reform years the coefficient estimates are small and insignificant, indicating that the parallel trend assumption is fulfilled. In 2004, the reform effect is negative but not different from zero. Starting one year after the reform the wage effects

Fig. 3.6 Effect of Deregulation on Post-Apprenticeship Wages
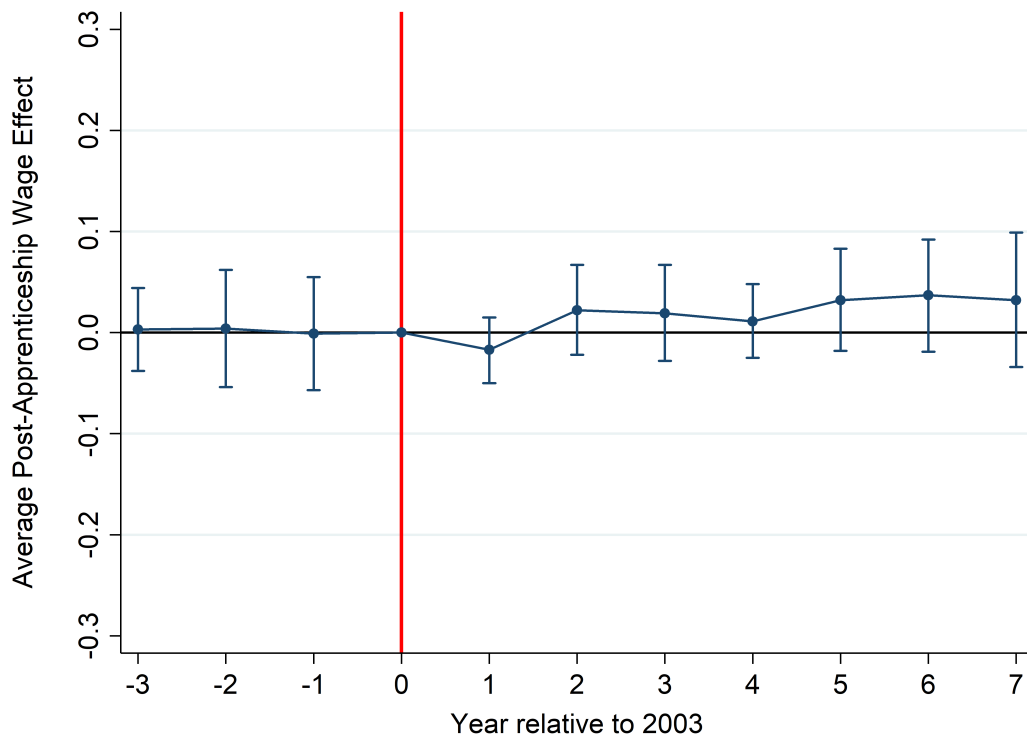
Notes: The figure plots estimates and the 95% confidence interval of the effect of deregulation on log fte-adjusted daily real wages (in 2014 prices) of West German labor market entrants based on equation (3.2). The dependent variable is the log fte-adjusted daily wage of companions within a two-year post-apprenticeship window. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: IAB post-apprenticeship sample, 2000-2010.

turn positive but remain insignificant throughout the sample period. As such, I observe a zero net effect in terms of average wages.[31]

The absence of average wage effects could mask substantial heterogeneity in wage responses for different firm sizes. Since craft businesses are typically smaller firms with less than 20 employees, I next turn to the question how the reform affects the wage structure of companions at labor market entry in terms of firm size heterogeneity. The reform might affect small craft firms stronger compared to large craft firms, since they rely primarily on apprenticeship training to recruit their skilled workforce, instead of hiring graduates from other firms, technical colleges or universities (ZDH, 2019).

I present four specifications: specification (1) is the baseline specification with year dummies, occupation dummies, state dummies and without covariates. Specification (2) further controls for age of the craftsman, age squared, the gender and foreign nationality of the craftsman. Specification (3) includes in addition to specification (2) industry

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[31]Figure B.6 in the Appendix plots the coefficient estimates of post-apprenticeship wages based on equation (3.2) for West German craftsmen only. In fact, they earn shortly after the reform around 6% more in the deregulated trades. Yet, the effect has no long-lasting impact.

Table 3.4 Effect of Deregulation on Post-Apprenticeship Wages

	(1)	(2)	(3)	(4)
Deregulated (t=2000)	0.013	0.002	0.007	0.003
	(0.022)	(0.020)	(0.021)	(0.021)
Deregulated (t=2001)	-0.002	-0.004	0.006	0.004
	(0.029)	(0.026)	(0.030)	(0.029)
Deregulated (t=2002)	-0.006	-0.002	0.000	-0.001
	(0.028)	(0.025)	(0.029)	(0.028)
Deregulated (t=2003)				
Deregulated (Reform year)	-0.017	-0.015	-0.019	-0.017
	(0.021)	(0.017)	(0.016)	(0.016)
Deregulated (t=2005)	0.018	0.015	0.020	0.022
	(0.028)	(0.026)	(0.023)	(0.022)
Deregulated (t=2006)	0.012	0.017	0.015	0.019
	(0.029)	(0.026)	(0.024)	(0.024)
Deregulated (t=2007)	0.014	0.012	0.007	0.011
	(0.022)	(0.019)	(0.019)	(0.018)
Deregulated (t=2008)	0.004	0.017	0.027	0.032
	(0.029)	(0.025)	(0.024)	(0.025)
Deregulated (t=2009)	0.007	0.023	0.030	0.037
	(0.036)	(0.033)	(0.031)	(0.028)
Deregulated (t=2010)	0.016	0.024	0.025	0.032
	(0.038)	(0.036)	(0.037)	(0.033)
Year fixed effects	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	-	-	Yes	Yes
Industry-specific trend	-	-	-	Yes
Covariates	-	Yes	Yes	Yes
Observations	14,066	14,066	14,066	14,066
Adj. R-squared	0.506	0.554	0.591	0.593

Notes: The table reports estimates of deregulation on log fte-adjusted daily real wages (in 2014 prices) for companions within a two-year post-apprenticeship window based on equation (3.2), all regressions include a constant. Covariates include age, age squared, dummies for gender and foreign nationality, a dummy for an upper secondary degree (Abitur) and log firm size. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: IAB post-apprenticeship sample, 2000-2010.

dummies and specification (4) adds further linear industry-specific trends. Table 3.5 shows now the results for craftsmen at labor market entry in small firms with less than 20 employees (columns (1)−(3)) and in larger firms with 20 to 100 employees (columns (4)−(6)). The estimates indicate that deregulation significantly increases post-apprenticeship wages by 11% two to three years after the reform in firms with less than 20 employees. Ruling out that industry-specific shocks affect the results, column (3)

Table 3.5 Effect of Deregulation on Companion's Wages by Firm Size

	Small Firms			Large Firms		
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulated (t=2000)	0.058	0.068	0.067	0.027	0.028	0.019
	(0.042)	(0.044)	(0.044)	(0.027)	(0.026)	(0.026)
Deregulated (t=2001)	0.033	0.041	0.041	-0.004	0.001	-0.021
	(0.036)	(0.037)	(0.037)	(0.050)	(0.049)	(0.049)
Deregulated (t=2002)	0.020	0.024	0.023	0.050	0.053	0.044
	(0.042)	(0.044)	(0.043)	(0.032)	(0.035)	(0.034)
Deregulated (t=2003)						
Deregulated (Reform year)	0.009	0.020	0.021	-0.010	-0.017	-0.020
	(0.036)	(0.035)	(0.035)	(0.034)	(0.036)	(0.039)
Deregulated (t=2005)	0.039	0.049	0.051	-0.027	-0.003	-0.001
	(0.048)	(0.047)	(0.046)	(0.044)	(0.043)	(0.046)
Deregulated (t=2006)	0.109**	0.116**	0.119***	-0.023	-0.002	0.001
	(0.046)	(0.045)	(0.044)	(0.046)	(0.047)	(0.048)
Deregulated (t=2007)	0.102**	0.107**	0.111**	-0.028	-0.015	-0.010
	(0.051)	(0.051)	(0.051)	(0.043)	(0.040)	(0.040)
Deregulated (t=2008)	0.041	0.055	0.059	0.002	0.033	0.043
	(0.055)	(0.054)	(0.052)	(0.040)	(0.044)	(0.043)
Deregulated (t=2009)	-0.027	-0.023	-0.020	0.030	0.049	0.066
	(0.049)	(0.050)	(0.049)	(0.045)	(0.045)	(0.045)
Deregulated (t=2010)	-0.011	-0.014	-0.006	-0.013	-0.028	-0.015
	(0.046)	(0.044)	(0.041)	(0.058)	(0.056)	(0.063)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	-	Yes	Yes	-	Yes	Yes
Industry-specific trend	-	-	Yes	-	-	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,139	7,139	7,139	3,868	3,868	3,868
Adj. R-squared	0.585	0.591	0.593	0.490	0.534	0.538

Notes: The table reports estimates of deregulation on log fte-adjusted daily real wages (in 2014 prices) for companions within a two-year post-apprenticeship window based on equation (3.2), all regressions include a constant. The dependent variable in columns (1)−(3) is the log daily fte-adjusted daily real wage for companions in small firms with 1-19 employees and in columns (4)-(6) for companions in larger firms with 20-100 employees. Covariates include age, age squared, dummies for gender and foreign nationality, a dummy for an upper secondary degree (Abitur) and log firm size. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: IAB post-apprenticeship sample, 2000-2010.

further controls for industry-specific fixed effects and industry-specific shocks, showing that the point estimate remains significant and unchanged. In the next columns, I report results for companions at labor market entry in larger firms. The coefficient estimates are now negative, but not significantly different from zero.

Fig. 3.7 Effect of Deregulation on Post-Apprenticeship Wages
including Workers in very large Firms
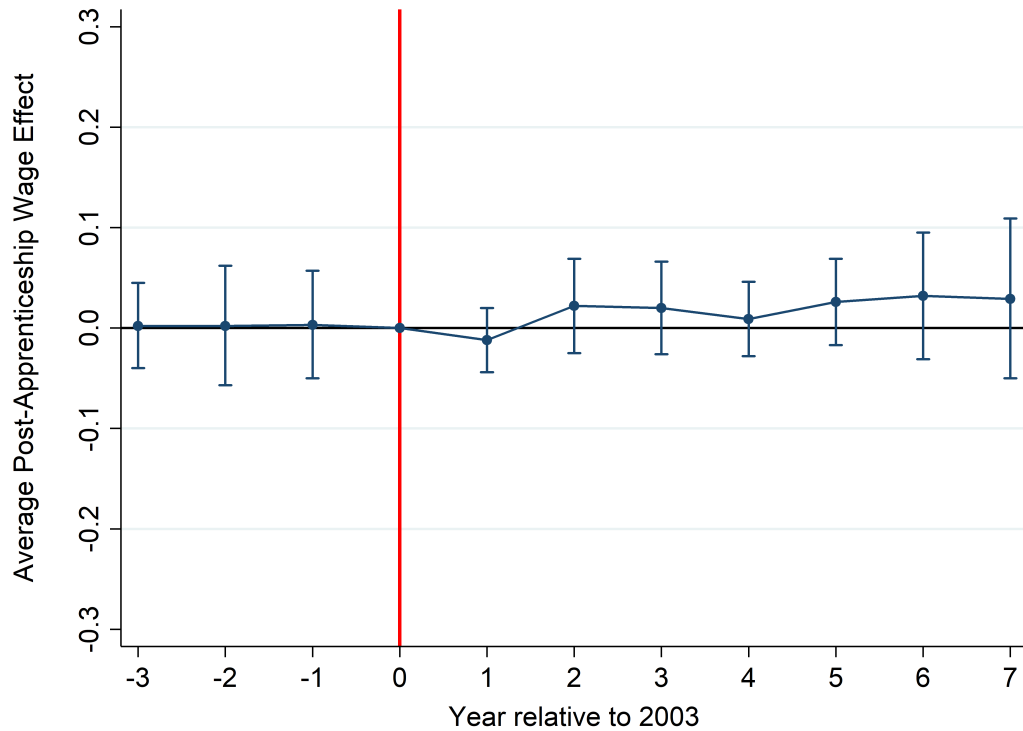
To summarize, the results on apprenticeship training and post-apprenticeship wages suggest that less new apprentices enter the labor market; the number of apprentices declines and that on average companions do not receive lower wages. Yet, companions in smaller firms receive higher wages at labor market entry following shortly after the deregulation. These results are consistent with a model of increased product market competition due to the removal of entry restrictions; which in turn puts downward pressure on economic rents, that could be recuperated before (Blanchard and Giavazzi, 2003). As a result the incentives to invest in apprenticeship training should decline, which results in a decrease of demand for apprentices. Figure B.2 in the Appendix confirms this hypothesis, providing evidence on a sizable long-run negative reform effect in terms of training investments. As such, firms may pass on the negative rent effect

to their employees and pay lower wages. In contrast, firms may offer higher wages to their companions after the training period, since outside options for companions have increased (Becker, 1962). Overall, these effects suggest that the deregulation in 2004 has led to a decline in apprenticeship training through a negative demand effect and that the evidence in terms of post-apprenticeship wages is mixed at best.

Table 3.6 Effect of Deregulation on Companion's Wages without Firm Size Restrictions

	(1)	(2)	(3)	(4)
Deregulated (t=2000)	0.014	0.001	0.002	-0.001
	(0.022)	(0.019)	(0.021)	(0.020)
Deregulated (t=2001)	0.001	-0.004	0.002	0.000
	(0.028)	(0.024)	(0.030)	(0.028)
Deregulated (t=2002)	-0.002	0.000	0.003	0.003
	(0.028)	(0.024)	(0.027)	(0.026)
Deregulated (t=2003)				
Deregulated (Reform year)	-0.018	-0.013	-0.012	-0.011
	(0.020)	(0.017)	(0.016)	(0.015)
Deregulated (t=2005)	0.012	0.017	0.022	0.024
	(0.027)	(0.027)	(0.024)	(0.022)
Deregulated (t=2006)	0.016	0.023	0.020	0.024
	(0.027)	(0.026)	(0.023)	(0.022)
Deregulated (t=2007)	0.010	0.013	0.009	0.012
	(0.020)	(0.019)	(0.019)	(0.018)
Deregulated (t=2008)	-0.001	0.019	0.026	0.030
	(0.026)	(0.023)	(0.022)	(0.023)
Deregulated (t=2009)	0.004	0.028	0.032	0.037
	(0.031)	(0.033)	(0.032)	(0.027)
Deregulated (t=2010)	0.019	0.033	0.029	0.035
	(0.036)	(0.039)	(0.040)	(0.033)
Year fixed effects	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	-	-	Yes	Yes
Industry-specific trend	-	-	-	Yes
Covariates	-	Yes	Yes	Yes
Observations	14,717	14,717	14,717	14,717
R-squared	0.499	0.581	0.618	0.619
Adj. R-squared	0.495	0.577	0.614	0.616

Notes: The table reports estimates of deregulation on log fte-adjusted daily real wages (in 2014 prices) for companions within a two-year post-apprenticeship window based on equation (3.2). The dependent variable in all columns is the log fte-adjusted daily real wage for companions and all regressions include a constant. Covariates include age, age squared, dummies for gender and foreign nationality, a dummy for an upper secondary degree (Abitur) and log firm size. The reference year is 2003. Standard errors are clustered at the occupation level. Significance levels: 1% ***, 5% ** and 10% *.
Source: IAB post-apprenticeship sample, 2000-2010.

3.6   Robustness Analysis

In a next step, I conduct specification checks to assess the robustness of my findings. Including individuals who work in firms with more than 1,500 employees, Figure 3.7 plots the coefficient estimates on post-apprenticeship wages based on equation (3.2), additionally controlling for industry fixed effects. Table 3.6 provides the coefficient estimates for the baseline in column (1), and additional specifications are rerun in columns (2)−(4) as before. The inclusion of workers in firms with more than 1,500 employees has little effect on the post-apprenticeship wages for companions. Compared to the original estimate, the coefficient estimate in the reform year is similar in terms of magnitude, but not statistically significant as shown across all columns. Thus, the additional results suggest that the empirical results are robust to including larger firm sizes.

3.7   Discussion and Conclusion

This paper investigates the effect of deregulating entry requirements on vocational training and post-apprenticeship wages. Empirically, I exploit the German crafts and trades reform in 2004 that partially deregulated 53 out of 94 occupations, leading to a removal of the Master craftsman requirement to open and run a business in these trades. Germany provides an interesting context to study the effect of deregulation on vocational training due to its large-scale apprenticeship program.

My empirical results indicate four main insights on the consequences of deregulation for the vocational training market. First, the number of companions who pursue a career path of becoming a Master craftsman substantially decline after the reform. The second insight is that new apprenticeship contracts decline substantially in the medium-run in response to the reform. This decline could be explained by stronger competition in the product market, which in turn puts a downward pressure on the size of economic rents and consequently discourages incumbent firms' incentives to invest in training (see Blanchard and Giavazzi, 2003, Acemoglu and Pischke, 1999b and Bassanini and Brunello, 2011). Given that the increase in competition consists mostly of self-employed craftsmen without employees, the negative rent effect is stronger for incumbent firms, discouraging training investments.

Third, I find even stronger long-lasting negative effects on the total number of apprentices, which are partly due to apprentices leaving their training firm without a degree. One potential explanation is that after the reform, individuals can become self-employed in the deregulated trades even without a degree. Fourth, I document

that average wages for companions at labor market entry do not respond to the reform. This partly explains why the supply effect does not dominate the demand effect. Yet, in contrast to the overall population, I find mixed results for companions in firms with less than 20 employees. For them, post-apprenticeship wages respond positively two years after the reform. The temporary wage increase could be the consequence of firms offering higher post-apprenticeship wages because outside options for companions have increased after the removal of entry barriers (Becker, 1964).

My research project has important policy implications. The German Monopolies Commission (*Monopolkommission*) is in favor of deregulating all crafts and trades occupations, while there is increased pressure of the German Confederation of Skilled Crafts (ZDH) to re-introduce the Master craftsman degree as entry restriction (see for example FAZ, 2017). One of their key arguments has been that deregulation may lead to negative consequences for the apprenticeship sector. A policy implication of my analysis is that the deregulation sizably decreased the number of Master craftsman entrants relative to potential Master craftsmen after 2004. Further, although the number of apprentices declined in all crafts and trades occupations, the reform seems to have accelerated the decline. Finally, I observe few effects on earnings of those who remain in the market.

Appendix B

Fig. B.1 Trends in New Apprenticeship Contract Growth by Deregulated and Regulated Trades
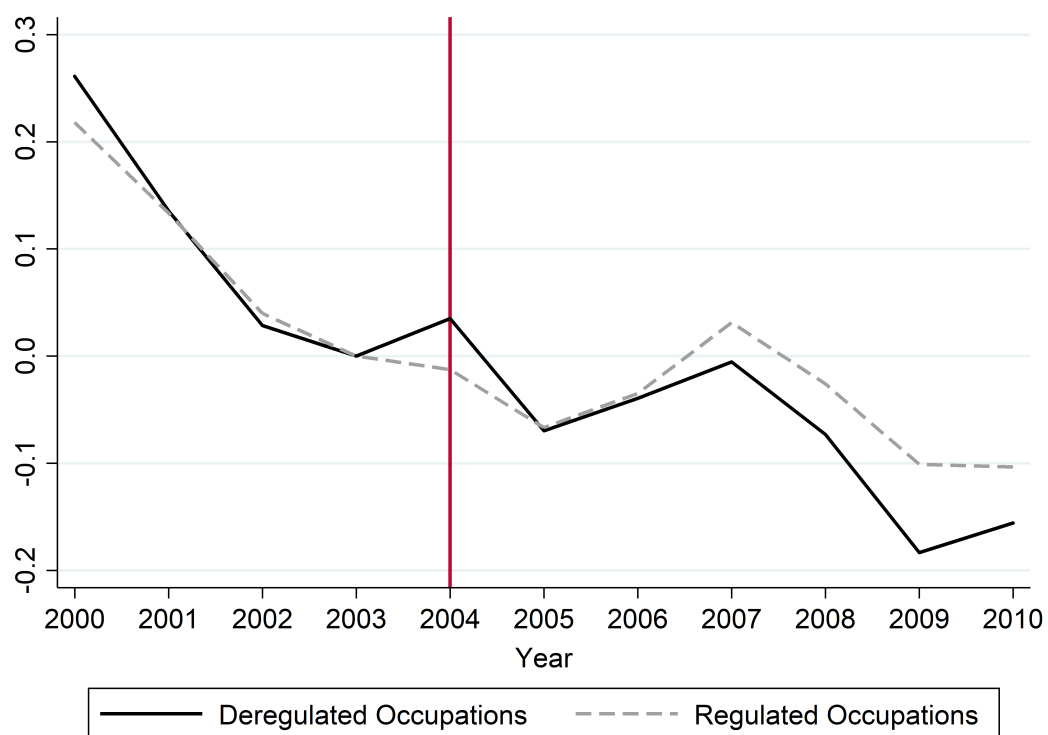
Notes: The figure plots the growth rate of new apprenticeship contracts relative to 2003 (pre-reform period) by deregulated and still regulated occupations. The vertical line indicates the reform year in 2004.
*Source*: ZDH sample, 2000-2010.

Fig. B.2 Trends in the Supply of Training Slots by Deregulated and Regulated Trades
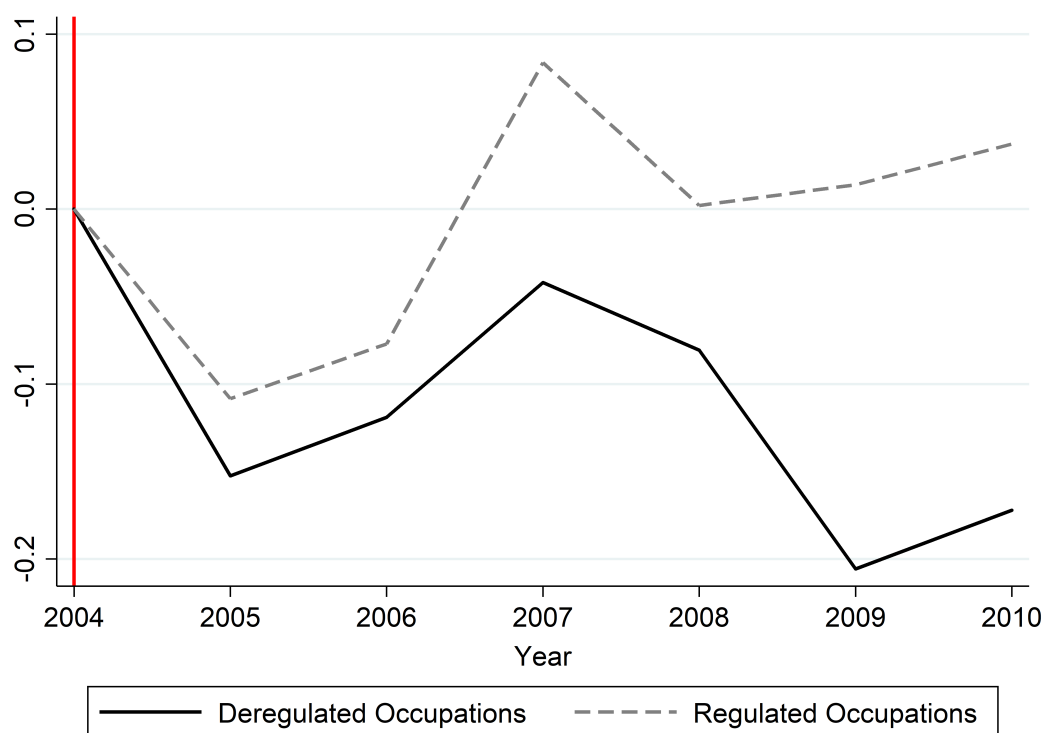
Notes: The figure plots the growth rate in the supply of apprenticeship training slots relative to 2004 by deregulated and still regulated occupations. The supply of training slots is proxied by the number of new apprenticeship contracts and the number of vacant training places. The vertical line indicates the reform year in 2004.
Source: BiBB sample, 2004-2010.

Fig. B.3 Apprenticeship Entry in the Crafts and Trades and Entry at the University
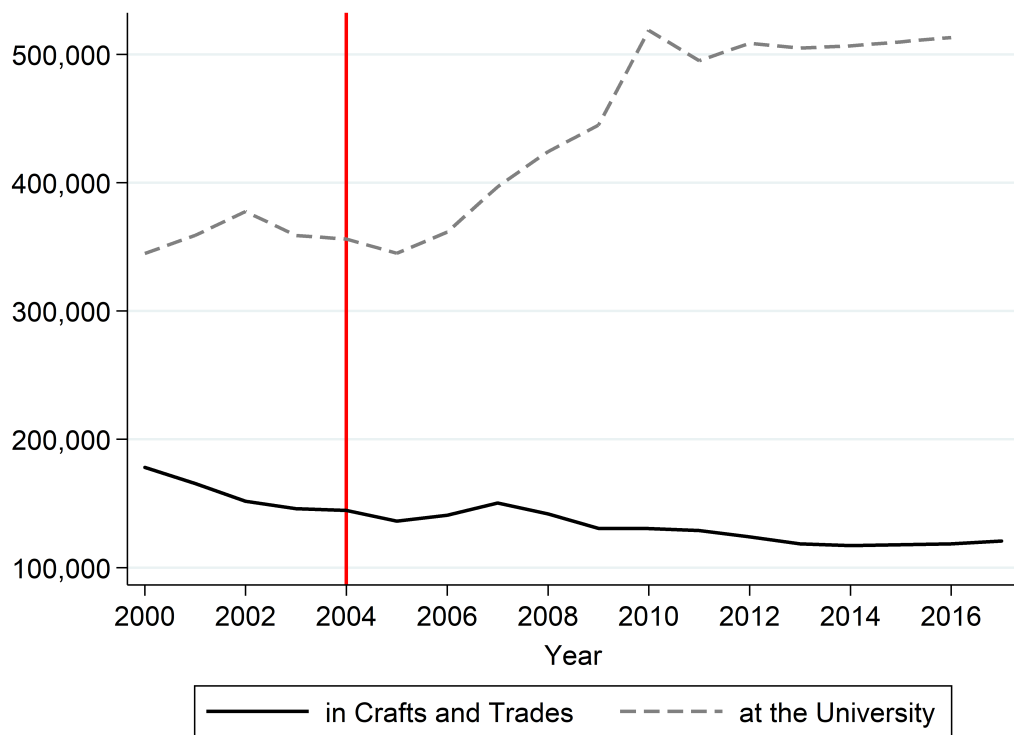
Notes: The figure plots the total number new apprenticeship contracts in the crafts and trades and the total number of university entrants over time. The vertical line indicates the reform year in 2004.
Sources: Destatis and ZDH data, 2000-2016.

Fig. B.4 Trends in the Ratio of Successful Trade Tests to Apprentices
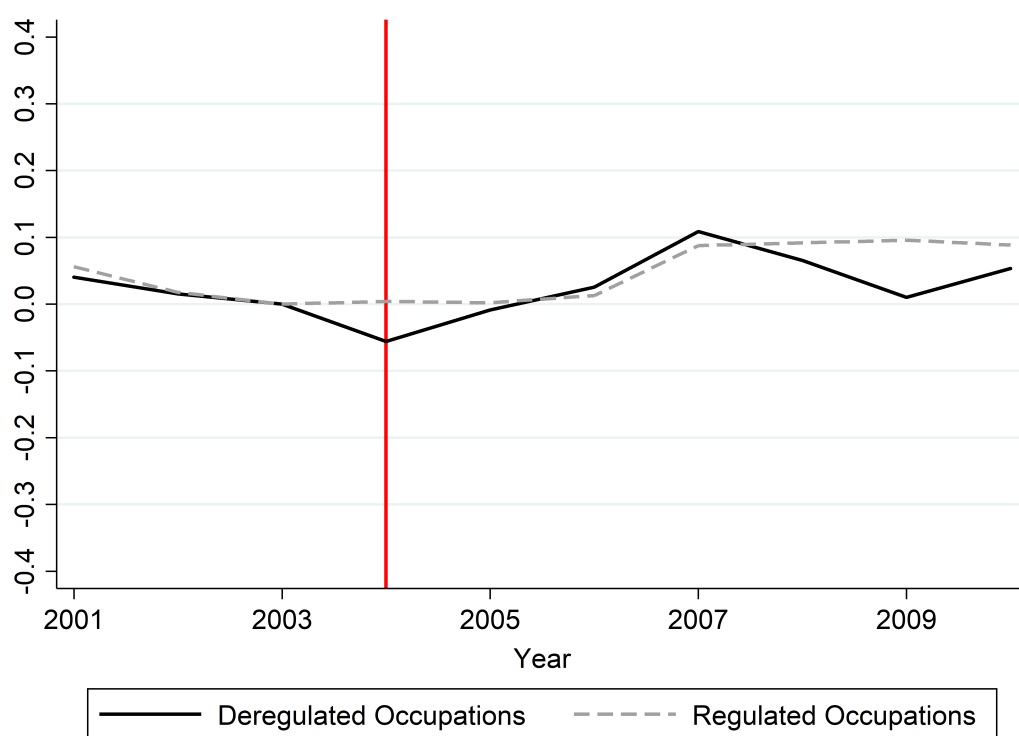
Notes: The figure plots the growth rate in the share of passed trade tests relative to the number of potential examination takers and relative to 2003 by deregulated and still regulated occupations. The number of potential examination takers is proxied by a 3-year lag of the number of apprentices. The vertical line indicates the reform year in 2004.
Source: ZDH sample, 2001-2010.

Fig. B.5 Trends in Prematurely Cancelled Training Contracts in the Crafts and Trades
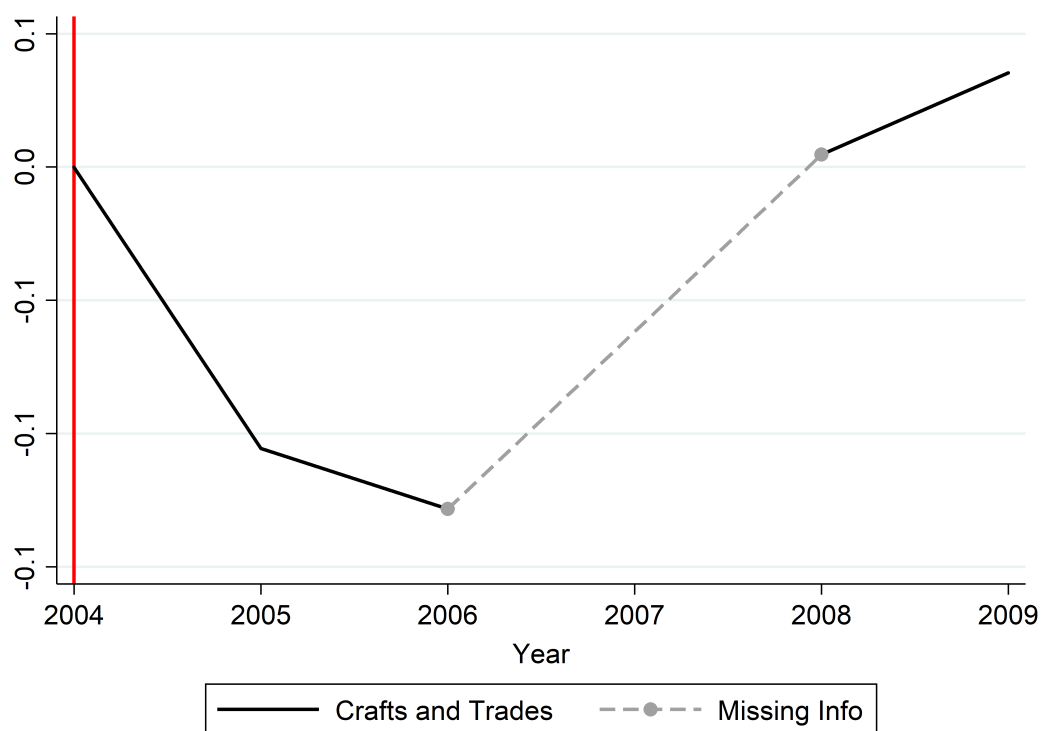
Notes: The figure plots the growth rate in the number of prematurely cancelled training contracts relative to 2004 for crafts and trades occupations. Due to the revision of the vocational training statistics in 2007 the number of prematurely cancelled training contracts is missing in that year. The vertical line indicates the reform year in 2004.
Source: BiBB sample, 2004-2009.

Fig. B.6 Effect of Deregulation on West German Post-Apprenticeship Wages

Table B.1 Regulated and Deregulated Craft Occupations

Regulated Occupations		Deregulated Occupations	
1	Bricklayer and Concretor	1	Tile Slab and Mosaic Layer
2	Stove and Air Heating Mechanic	2	Cast Stone and Terrazzo Maker
3	Carpenter	3	Screed Layer
4	Roofer	4	Vessel and Equipment Constructor
5	Road Contruction Worker	5	Clockmaker
6	Thermal and Acoustic Insulation Fitter	6	Engraver
7	Well Sinker	7	Metal Former
8	Stonemason	8	Galvaniser
9	Plasterer	9	Metal and Bell Founder
10	Painter and Lacquerer	10	Cutting Tool Mechanic
11	Scaffolder	11	Goldsmith and Silversmith
12	Chimney Sweep	12	Parquet Layer
13	Metal Worker	13	Shutter and Sunshade Mechatronics
14	Surgical Instrument Maker	14	Model Builder
15	Coachbuilder	15	Turner (Ivory Carver) and Wooden Toy Maker
16	Precision Engineer	16	Wood Carver
17	Motorbike and Bicycle Mechanic	17	Cooper
18	Refrigeration Mechanic	18	Basket Maker
19	Communication Technician	19	Costume Tailor
20	Automotive Mechatronics Technician	20	Embroiderer
21	Mechanic for Agricultural and Construction Machinery	21	Milliner
22	Gunsmith	22	Weaver
23	Plumber	23	Sailmaker
24	Installer and Heating Fitter	24	Furrier
25	Electrics Technician	25	Shoemaker
26	Electrical Machine Engineer	26	Saddler
27	Joiner	27	Interior Decorator
28	Boat Builder	28	Miller
29	Rope Maker	29	Brewer and Maltster
30	Baker	30	Wine Cellarperson
31	Pastry-cook	31	Textile Cleaner
32	Butcher	32	Wax Chantler
33	Dispensing Optician	33	Building Cleaner
34	Hearing Aid Acoustician	34	Glass Finisher
35	Orthotic Technician	35	Precision Optician
36	Orthopaedic Shoemaker	36	Glass and China Painter
37	Dental Technician	37	Precious Stone Engraver and Cutter
38	Hairdresser	38	Photographer
39	Glazier	39	Bookbinder
40	Glass Blower and Glass Apparatus Maker	40	Typesetter and Printer
41	Mechanic for Tyres and Vulcanization	41	Screen Printer
		42	Flexographer
		43	Ceramist
		44	Organ and Harmonium Maker
		45	Piano and Harpsichord Maker
		46	Reed and Organ Musical Instrument Maker
		47	Violin Maker
		48	Bow Maker
		49	Metal Wind Instrument Maker
		50	Wooden Wind Instrument Maker
		51	Plucked Instrument Maker
		52	Gilder
		53	Sign and Illuminated Advertisement Maker

Notes: The table lists all crafts and trades occupations that are regulated according to Annex A and deregulated occupations according to Annex B1 of the German Crafts and Trades Code.

Chapter 4

Permanent Changes in the Wage Structure and the East German Fertility Crisis

4.1 Introduction

After the fall of the Berlin Wall, the total fertility rate (TFR) in East Germany, which describes the average number of children a woman will have over her childbearing years, tumbled from 1.7 (1989) to 0.7 (1994) children per woman on average. Total fertility rates below one are rare events during peacetime − which has prompted many observers to call this episode the "East German fertility crisis".[1] Figure 4.1 shows the total fertility rate in West and East Germany prior to and after unification. In the decade prior to unification, the East German fertility rate consistently exceeds the West German fertility rate by about 0.3-0.4 children per woman. In the years after unification, total fertility in East Germany drops sharply, started to recover after 1995 and approaches West German levels of about 1.3-1.4 children per woman only by 2008.

A large literature has documented the fertility crisis in East Germany (see e.g. Eberstadt, 1994; Billingsley, 2010; Kreyenfeld and Mika, 2004). Yet, little is known what exactly explains the dramatic change in fertility behavior among East German women after unification. An early attempt attributes the rapid decline in East German

[1]Similar, though considerably less dramatic declines in the fertility rate were observed in other transition countries, most notably Russia or the Czech Republic (see, for example, Billingsley, 2010; Sleebos, 2003; Sobotka, 2003).

Fig. 4.1 Evolution of Total Fertility Rate in East and West Germany
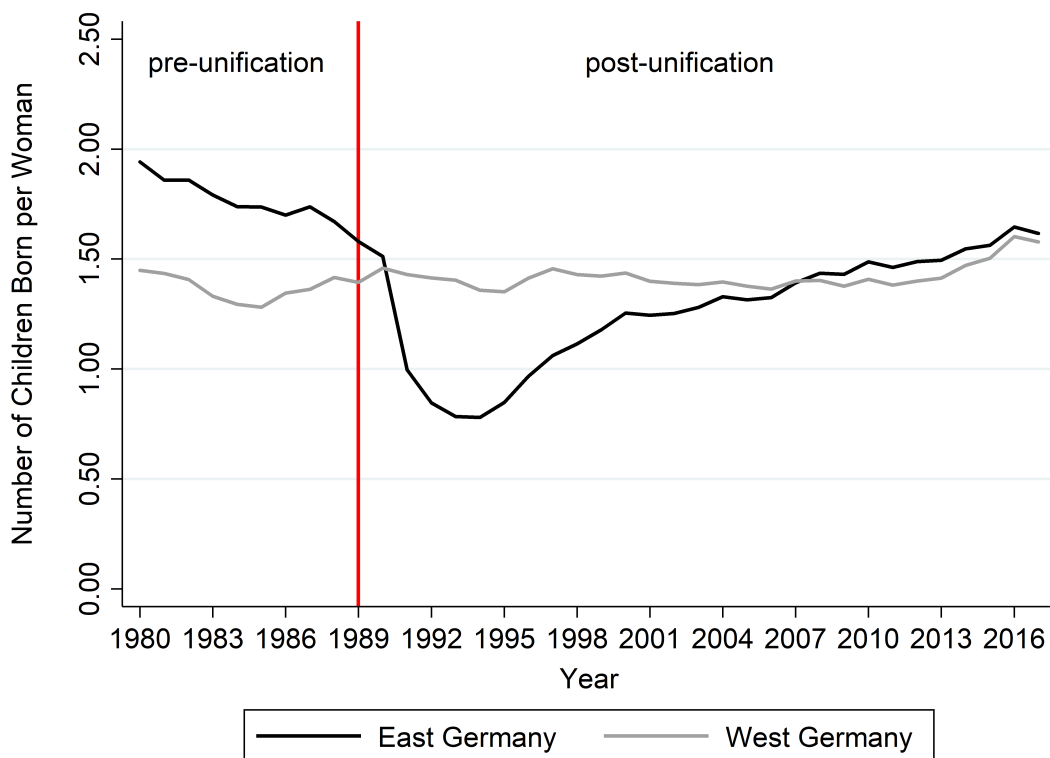
Notes: The figure shows the total fertility rate by region and year, i.e. the sum of age-specific fertility rates in each year among women in their childbearing years.
Source: Human fertility database.

fertility rates to the economic uncertainty and sense of crisis in the first years after unification (Witte and Wagner, 1995; Kreyenfeld, 2010b and Chevalier and Marie, 2017).[2] However, the crisis argument has difficulties to explain why fertility behavior did not bounce back in the 1990s when the uncertainty surrounding unification was resolved.[3]

An alternative explanation argues that the fertility crisis is nothing but an adjustment process to the lower fertility levels in West Germany (see e.g. Conrad et al., 1996; Lechner, 2001). After 1990, East Germany basically inherited the legal and institutional framework from West Germany. Facing similar institutional and economic incentives, so the argument goes, East German women converge in their fertility to their West German counterparts. Despite the common institutional framework however, East and West Germany differ along important socio-economic characteristics even today (see

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[2]In contrast, Liepmann (2018) suggests that structural changes in the labor demand impacted East German women differently in 1989.

[3]There is mixed empirical support for the crisis explanation even for the initial years after unification. Bhaumik and Nugent (2005) report a negative correlation between employment risk and fertility, while Kreyenfeld (2010b) fails to find a statistical relationship between job uncertainty and fertility in East Germany after 1990.

also Kreyenfeld and Mika, 2004; and Goldstein and Kreyenfeld, 2011). East German women have higher employment rates and fewer women are employed in part-time work, for example. And both wages and household income are still lower in East Germany than in West Germany even more than two decades after unification. Given these persistent differences, it remains an open question what explains the dramatic changes in East Germany fertility.

Here, we propose a novel explanation for the observed fertility decline. Rather than a crisis phenomenon or mere adaptation to West Germany, we explore the link between permanent changes in the labor market and fertility behavior. After 1990, the transition process of the East German economy resulted in substantial changes in its wage and employment structure. Jobs were destroyed at a massive scale as many companies were unable to compete with firms from West Germany. Wages began to reflect market principles in the early 1990s. As a consequence, returns to education, which were modest in the socialist economy, rose rapidly after unification, especially for the high-skilled. Returns to work experience rose substantially − creating steep age-earnings profiles that young women entering the labor market faced after 1990. As a result, substantial wage penalties for time away from work for childbirth and childcare emerge after unification, especially for very young and for high-skilled women in East Germany.

Economic models of fertility suggest that permanent shifts in the wage structure affect the demand for children through the opportunity costs of having children (often measured as wages and employment opportunities of the mother) and the income effect (often measured as male wages and other income in the household). In addition, the restructuring of the economy after unification should affect the timing of childbirth as well. In particular, women postpone births as skill depreciation or fewer career opportunities after time off work reduce lifetime earnings associated with early childbirth.

To quantify these mechanisms, we make use of unique administrative data from the German Federal Pension Fund, which provide comprehensive fertility histories of a 1% sample of all West and East German women born between 1940 and 1977. We combine the fertility histories with data on earnings and household income from the German Socio-Economic Panel (GSOEP) for the pre- and post-unification period; and data on employment and unemployment in East and West Germany from administrative social security records.

Merging these data sources, we exploit variation at the age × time × region level and across education groups to study the consequences of permanent changes in wages and employment on fertility behavior. As the variation in wages, income and employment are the consequence of a much broader restructuring of the East German economy according

to market principles, these changes are plausibly exogenous from the perspective of the individual East German woman. Our empirical strategy then identifies the role of permanent wage and employment changes for the demand for children and the timing of birth in East Germany after unification.[4]

Our estimates suggest that prior to unification, wages, income and wage differentials by skill had little influence on fertility choices. After unification, wages and in particular the penalties associated with time off work become significant determinants of the demand for children and the timing of births. Rising wage penalties of childbearing (through time off work) substantially reduce fertility at early ages, especially among high-skilled women.

Like earlier studies, we find that male wages and household income have a positive effect on current fertility. Furthermore, male unemployment rates have a negative effect on current period fertility, suggesting that expected lower income through employment risk reduces fertility. In contrast, female unemployment rates have a positive effect on current-period fertility. This result would be consistent with periods of high unemployment, reducing the opportunity cost of female time (an important input in the case of infants and small children). Together, the permanent changes in wages and unemployment can explain a substantial share of the observed decline in the demand for children as well as the postponement of births.

Our paper contributes to three different literatures. The paper is most closely related to studies on the link between economic incentives and fertility. An older literature identifies strong links between opportunity costs of a child, typically measured as the current wage, skill level or occupation, and fertility (Heckman and Walker, 1990; Merrigan and St.-Pierre, 1998; Schultz, 1985; Happel et al., 1990; Gustafsson, 2001 provides a nice survey of the earlier literature). One of the key challenges in this literature is to find plausibly exogenous variation in income, wages and employment to identify their effects on fertility behavior (see Hotz et al., 1997; but see Schultz, 1985). A second challenge is to distinguish the effects of transitory changes in wages and employment induced, for example, by business cycle fluctuations and individual job loss from permanent changes in the wage structure.

More recently, the income effect has been identified using job displacements of the husband (Lindo, 2010 for the US; Del Bono et al., 2012 for Austria) or policy reforms (see Milligan, 2005 for Canada; Björklund, 2006 for Sweden; Cohen et al., 2013 for Israel; Laroque and Salanié, 2013 for France) as source of identification. For very different

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[4]In a paper complementary to ours, Adda et al. (2017) develop a dynamic life-cylce model of occupation choice, labor supply and fertility, which they use to study the career costs of childbearing in West Germany.

contexts, these studies typically find a positive income effect.[5] Our analysis in turn exploits plausibly exogenous permanent changes in the wage structure for identification.

Our paper also contributes to a recent literature on how access to birth control affects women's careers and earnings (Goldin and Katz, 2002; Bailey, 2006; Bailey, 2012).[6] Our paper employs a similar estimation strategy exploiting aggregate changes that are plausibly exogenous from the individual's perspective for identification. Yet, we focus on the reverse relationship: how permanent changes in wages and career opportunities affect family formation and fertility behavior.

In our East German setting, unemployment and job loss are important and long-lasting features of the transition process in East Germany. As such, our paper is also related to the literature on the cyclicality of fertility (Dehejia and Lleras-Muney, 2004; see Butz and Ward, 1979 for an early contribution) and the influence of job displacement on fertility (see Del Bono et al., 2012 for Austria, Huttunen and Kellokumpu, 2016 for evidence from Finland).

Finally, our paper also contributes to studies on the fertility effects of education. The literature typically finds that more education results in postponement of births (e.g. Black et al., 2008). The effect on total completed fertility is less clear: some studies report negative effects (León, 2006 for the US; Lavy and Zablotsky, 2011 for Israel), while others find no such effect (McCrary and Royer, 2011 for the United Kingdom). Our results clearly suggest that higher education leads to a postponement of births — but only after unification, when skill differentials become large.

Understanding the determinants of low fertility rates has important policy implications. Many countries in the European Union (e.g. Italy, Spain or Germany) and elsewhere (e.g. Russia or Japan) have fertility rates similar to East Germany (Sleebos, 2003). The total fertility rate in the European Union was only 1.6 in 2009, Russia has currently a total fertility rate of about 1.7 children per woman — well below the replacement level of 2.1 children per woman. As a consequence, these countries have declining populations, raising important questions about the sustainability of social security (if built on a pay-as-you-go system) or future economic competitiveness, to name just a few pressing issues. Rather than just managing those challenges, we need to identify mechanisms through which childbearing can be encouraged. For example, if low fertility is mostly determined by low financial resources, family policies that support families with children seem most appropriate. If in contrast, child penalties in

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[5]Dickert-Conlin and Chandra (1999) find that tax incentives might also influence the timing of birth in the United States.

[6]In contrast, Hotz et al. (2005) and Miller (2011) use fertility shocks (like miscarriages) to analyze the effect of fertility on career wages and other labor market outcomes in the United States.

the labor market or skill depreciation during time off work are important, the focus of policy-makers should be on policies like childcare provision or maternity leave legislation.

The paper proceeds as follows. The next section provides a more detailed picture of fertility and wage changes after unification. Section 4.3 discusses the theoretical mechanisms between economic restructuring after unification and fertility behavior. Section 4.4 discusses the data sources we combine on fertility histories, wages, income, employment, and other control variables in East and West Germany before and after unification. Section 4.5 introduces our empirical strategy and reports the main results. Section 4.6 probes the validity of our findings to alternative explanations and specifications. Finally, Section 4.7 concludes.

4.2 The Fertility Crisis and Changes in Wages in East Germany

### 4.2.1 Fertility Patterns before and after Unification

We now take a closer look at the specific changes underlying the evolution of the aggregate total fertility rate in Figure 4.1. The East German transition process was characterized by a sharp increase in the mean age at first birth (see also Kreyenfeld, 2009; Kreyenfeld et al., 2012).

Figure 4.2 shows the share of first births that occur at a certain age of a cohort. Women born prior to 1965 had their first child in the socialist regime at age 20 or 21. In the cohorts born 1970 or later (who were 19 years-old in 1989), there is a clear tendency to shift their first birth to later ages. This postponement of births is also reflected in aggregate statistics, which show an increase in the mean age at first birth in East Germany by about five years after unification (see Figure C.1 in the Appendix). As a consequence, the mean age at first birth slowly converges to, but remains below West German levels (see also Arránz Becker et al., 2010; Kreyenfeld, 2009).[7]

A second development is that the fraction of women who have two and three or more children declines substantially after 1990 (see also Kreyenfeld, 2009 for related

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[7]A postponement of births (a "tempo effect") results in a lower total fertility rate than actual completed fertility by women aged 18 to 45. To study how much tempo effects reduce TFR, one can calculate an adjusted TFR that accounts for the change in mean age for each birth order (see Bongaarts and Feeney, 1998 for details of the calculation). Luy and Pötzsch (2011) find that the tempo-adjusted TFR still falls by 0.4 between 1986-90 and 1996-2000 compared to 0.47 for the unadjusted TFR. Compared to West Germany, East Germany's adjusted TFR is higher (by 0.3) in the decade prior to unification, but converges to lower West German levels of about 1.6 by around 2007. Hence, both tempo and level effects drive the change in TFR observed in Figure 4.1. See Table C.1 in the Appendix.

Fig. 4.2 Age at First Birth in East Germany by Birth Cohort
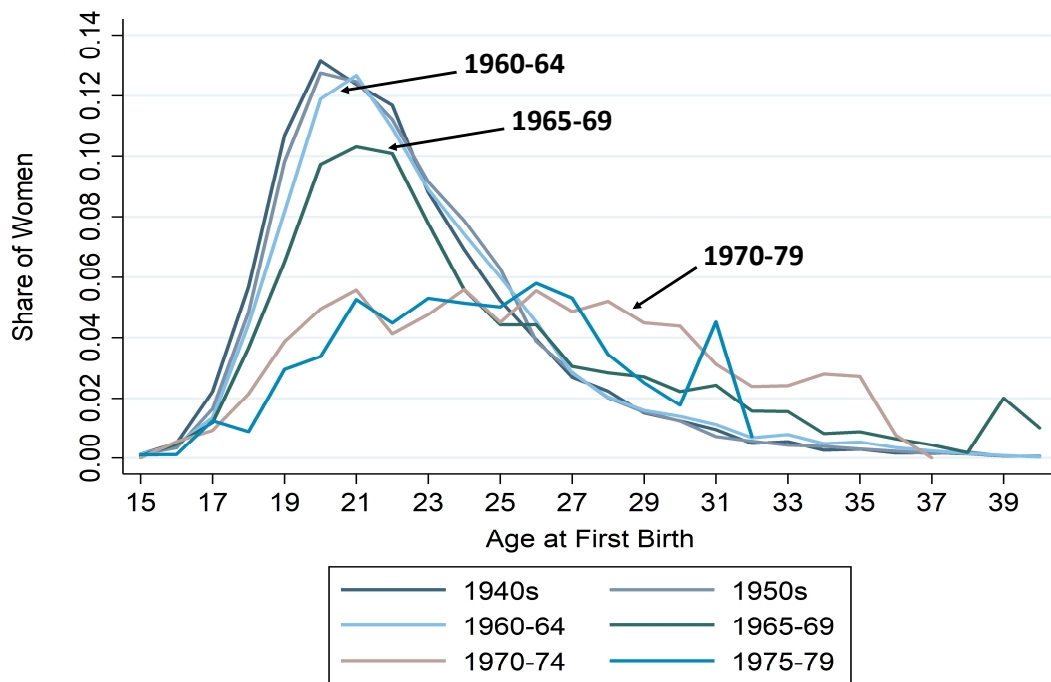
Notes: The figure plots the fraction of females by age of first birth for different birth cohorts in East Germany.

evidence). Figure 4.3 decomposes the total fertility rate in East Germany (the top line) by first births, second birth and higher-order births. The picture again reflects the temporary decline in first births due to the postponement of births among younger women. For second and higher-order birth, however, there is a permanent decline: whereas second births contributed 37% to the TFR in 1989, the share declines to 29% in 2001. Similarly, three or more children contributed 15% to the total fertility rate in 1989, but only 11% in 2001. As a consequence, the probability of having two or more children is significantly lower in East Germany than in West Germany (see also Arránz Becker et al., 2010 for additional empirical evidence).[8]

In contrast to age at first births, we see no dramatic change in the mean age for second or higher-order births (see Figure C.2 in the Appendix). While there is a clear upward trend in the mean age in both East and West Germany, there is no observable trend break after unification.[9]

The third interesting development is that changes in fertility behavior after unification are not distributed uniformly across skill groups (see, for example, Kreyenfeld,

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[8]Existing analyses of desired fertility match this pattern. Survey evidence suggests that East Germans are significantly more likely than West Germans to say that they prefer to have a single child (Dobritz, 2007).

[9]At the same time, East German women are still less likely to remain childless (see Figure C.3 in the Appendix) than women in West Germany (see also Dobritz and Schwarz, 1996). Note that the upward trend for younger cohorts occurs because they have not yet completed their fertility.

Fig. 4.3 Contribution of Different Birth Orders to TFR in East Germany
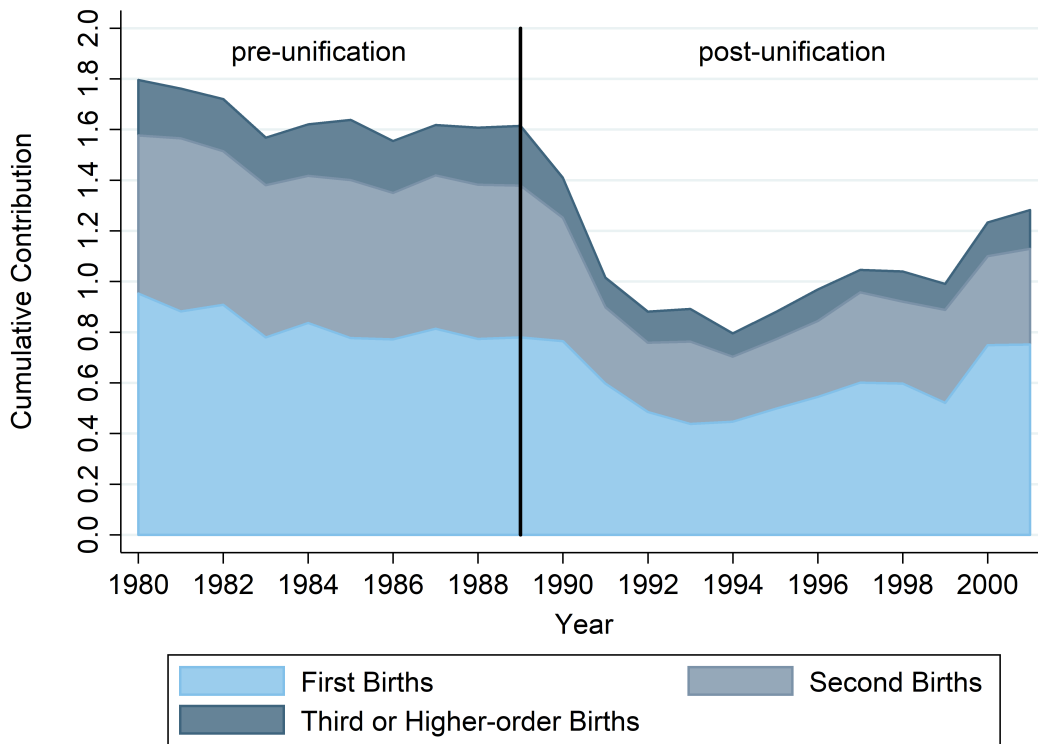
Notes: The figure shows the cumulative contribution of first, second and higher-order births to the total fertility rate. The contribution of the first births is calculated by summing over the age-specific fertility rates for first births among all women aged 15-49. Second and higher-order births are calculated correspondingly. The top line shows the evolution of the total fertility rate in East Germany.

2006b for a similar observation). Before unification, total fertility rates are quite similar across skill levels and hoovered around 1.6-1.7. Figure 4.4 shows that total fertility changes little for low-skilled women after unification. Yet, there is dramatic fertility decline among medium-skilled and especially among high-skilled women. Among high-skilled women, the total fertility rate fell by 0.99 between 1989 and 1995, whereas total fertility fell only by 0.1 for low-skilled women over the same period. The heterogeneous response persists well into the 2000s.

Overall, the descriptive evidence suggests that unification induced three important shifts in fertility behavior: (1) a sharp increase in the age at first birth (a tempo effect); (2) a decline in the share of women with two and more children (a level effect); and (3) a much stronger response among high-skilled women (heterogeneity). We will next document the major changes in the labor market that occurred in East Germany after unification.

Fig. 4.4 Total Fertility Rate in East Germany by Skill Level
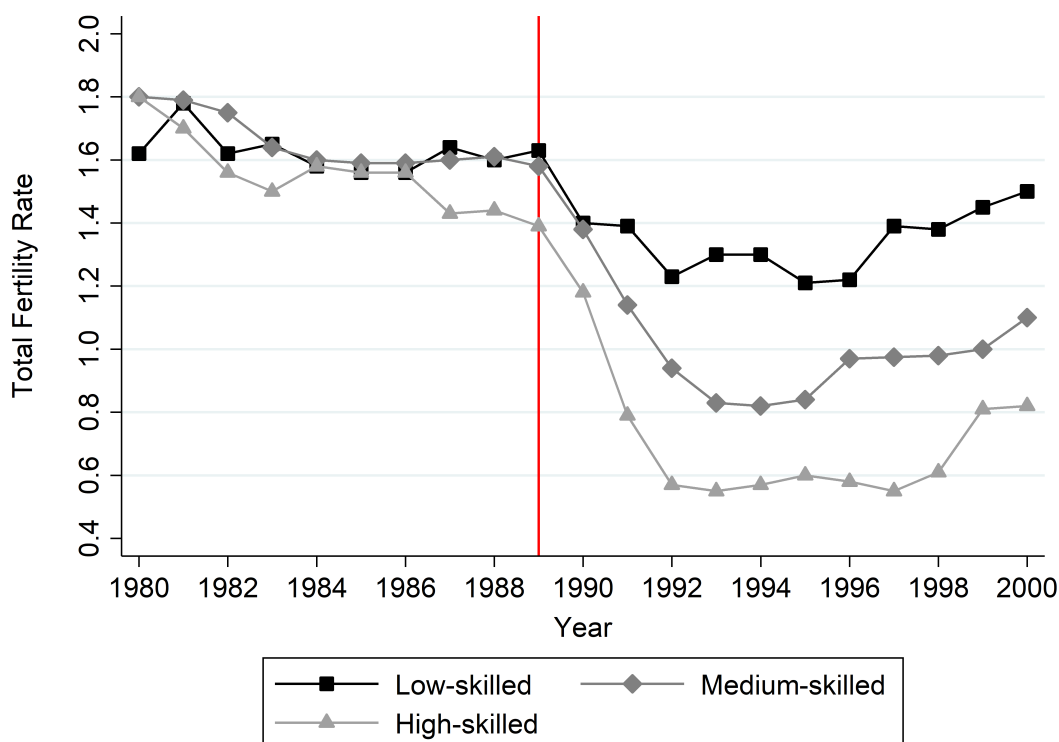
Notes: The figure shows the total fertility rate, i.e. the sum of age-specific fertility rates in each year among women aged 15 to 49 by skill group. The numbers shown are calculated from using a 3-year moving average. Low-skilled are women with no vocational degree; medium-skilled women have a high school or vocational degree; and high-skilled women have tertiary education.

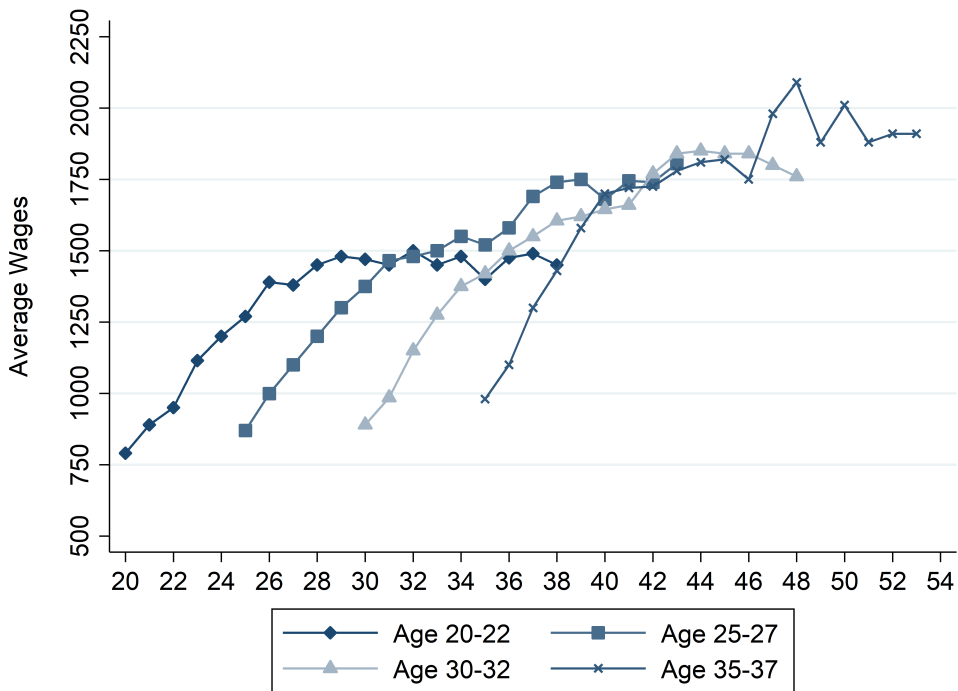## 4.2.2 Evolution of Wages and Income after Unification

With unification in October of 1990, East Germany adopted the political and institutional framework of West Germany. The dismantling of the East German economy began shortly after the fall of the wall in November 1989 and continued in the following years. During the transition to a market economy, many jobs in the socialist economy were destroyed. As a consequence, unemployment rose rapidly after unification (see Figure C.4 in the Appendix). In addition, aggressive wage bargaining between unions (mostly from the West) and East German employers (or their representatives) resulted in rapid growth in average wages shortly after unification (see Figure C.5 in the Appendix).

As the East German economy was transformed into a market economy, the wage and employment structure underwent dramatic changes after unification that were largely exogenous from the point of view of the individual. To trace changes in the structure of wages net of employment changes, we work with wage information that includes imputed wages for non-workers. Details on how we imputed wages are provided in the data section below, as well as in the Data Appendix.

Fig. 4.5 Age-Earnings Profiles of East German Women

(a) Cross-Section
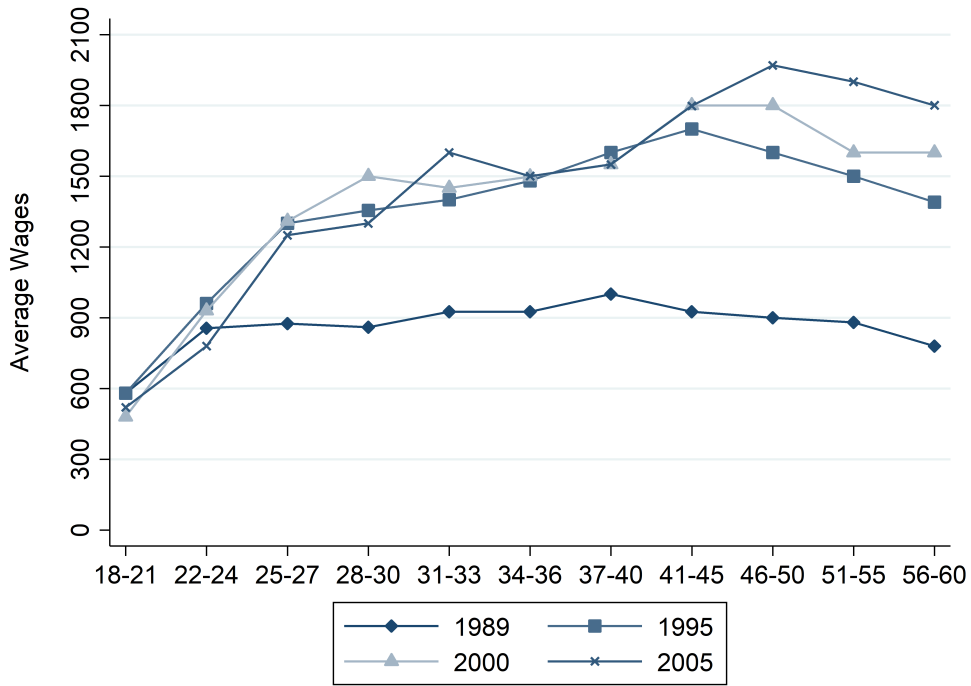
(b) After Unification, Panel Data

The first important development is that returns to work experience rise substantially during the East German transition. In the socialist economy, age-earnings profiles were
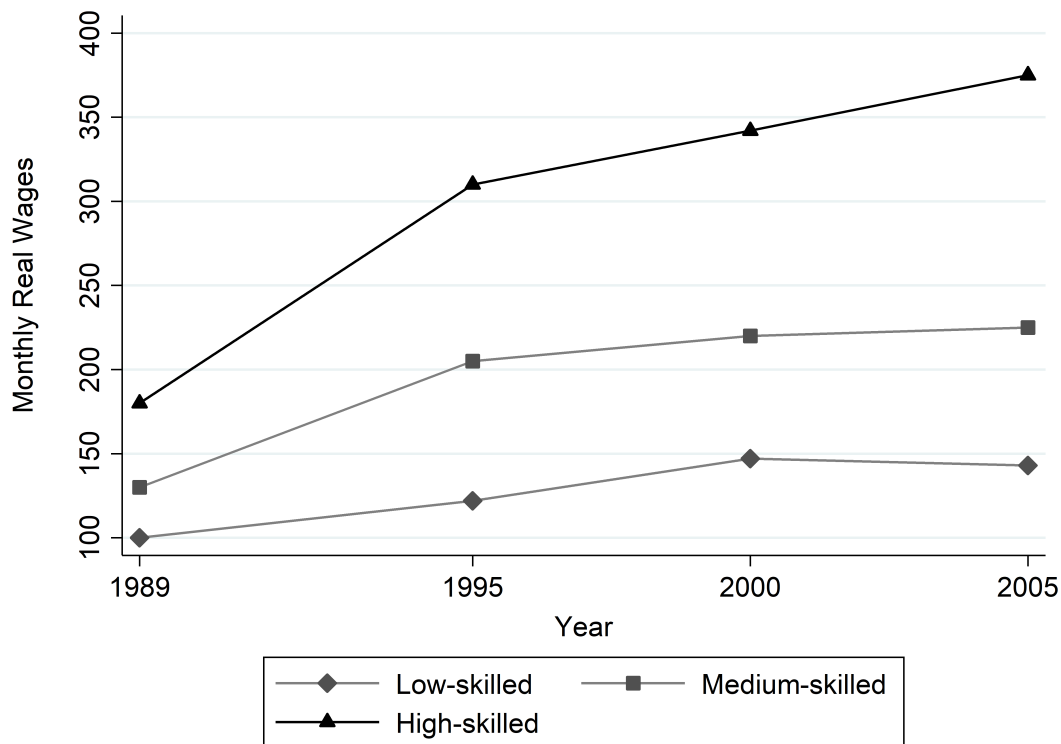
relatively flat (see also Bird et al., 1994 and Krueger and Pischke, 1995 for related evidence). Figure 4.5a shows the evolution of age-earnings profiles using a synthetic cohort approach. The age-earnings profile for 1995, for example, shows what East Germany women between the ages of 20 to 60 earned in 1995. If skills accumulated in the socialist regime depreciate (or there is some other cohort effect) however, the synthetic cohort approach underestimates the actual earnings young women today will have in twenty years. Figure 4.5b exploits the panel dimension instead to trace the earnings of women aged 20, 25, 30 or 35 years in 1990 over the period from 1990 to 2009. The panel evidence suggests that actual earnings profiles were even steeper, suggesting substantial returns to work experience for young women in East Germany after unification.

A second important shift concerns the returns to education. Figure 4.6 plots wage differentials by skill group in the socialist regime (1989/1990) and for selected years after unification (1995, 2000 and 2005) relative to low-skilled wages prior to unification (1989/90). Wage differentials across skill groups were non-trivial even in the socialist regime: high-skilled women in Germany earned about 85% more per month than low-skilled women. During the 1990s, however, skill differentials rise substantially. By 2005, high-skilled women in East Germany earn 375% more than low-skilled women in 1989/90 − and still 265% more than low-skilled women in East Germany in 2005.

As a result of the shifting wage structure and high unemployment rates, the penalty for leaving the labor market for childbirth and childcare rose dramatically in East Germany during the 1990s. To demonstrate this, we calculate predicted career wages for women, giving birth to a child at different ages both for the pre- and post-unification period. These career wages are based on a Mincer earnings regression of log monthly wages on potential experience, potential experience squared, indicators for medium- and high-skilled and interactions between the skill level and experience profile. In addition, we include dummy variables whether a woman gave birth at age 20-24, age 25-29, age 30-34, or age 35 and above, and interactions between those dummy variables and the skill group and experience profile. To allow for differences in skill prices across time and space, the regression is further run separately for East and West Germany, as well as for the pre-unification period and several post-unification periods (1990-1994, 1995-1999 and 2000-2004). Career wages are then calculated as predicted wages from that regression summed over the next fifteen years of a woman's labor market career for all women aged 20 to 40.[10] Figure 4.7a shows the loss in career wages for a woman giving birth to a child at a certain age relative to the career wages of a childless woman.

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[10]These career wages underestimate the true wage cost of childbearing for two reasons: first, we do not count wage losses from time out of the labor market prior to and after the birth or for parental leave. Second, we estimate career wages from actual choices; to the extent that potential wage losses are an important factor in fertility decisions; observed fertility and its timing is chosen to minimize

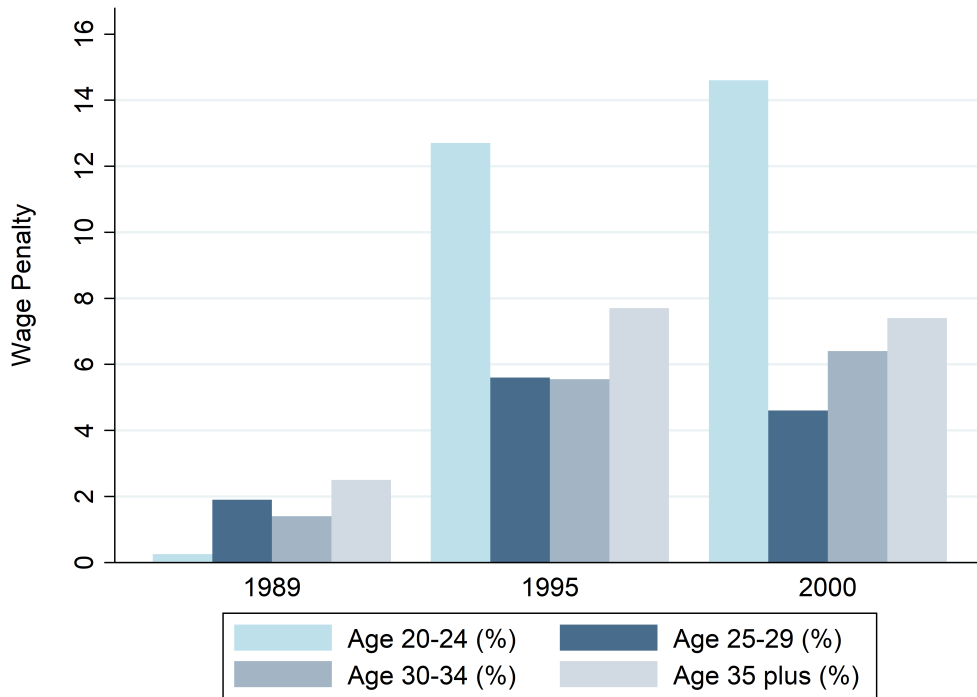Fig. 4.6 Evolution of Skill Differentials among East German Women

In East Germany, wage penalties for childbirth − even at very young ages − carried essentially no wage penalty. This situation changes completely after unification. Child penalties, especially for women with a childbirth between 20 and 24 rise to about 13% relative to a woman without a child. Wage penalties at later ages are also larger than in the socialist economy, but with 5.7% to 7.8% much lower than for young mothers. The comparison with West German women in the bottom panel also shows that wage penalties for childbearing are still much lower in East Germany than in West Germany. In West Germany, child penalties for women aged 20-24 and women aged 25-29 (relative to a childless woman) may exceed 30%.
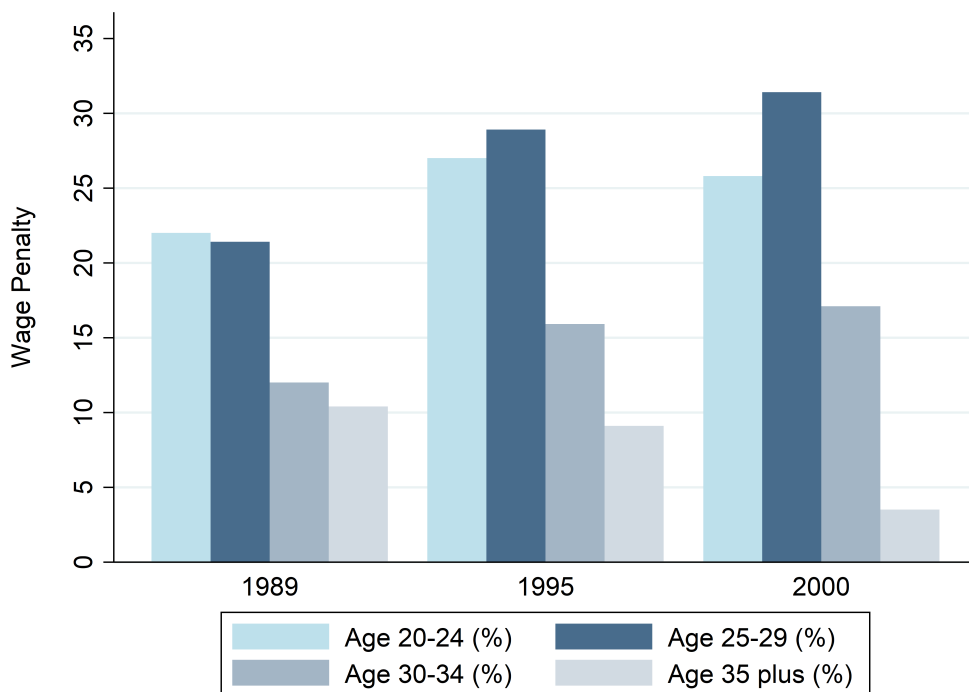
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wage losses (among other factors). The costs of purely accidental birth could be even larger. As such, we think that our wage penalties are a conservative estimate.

Fig. 4.7 Wage Penalty for Child Birth at Different Ages

(a) East Germany

Legend: Age 20-24 (%) | Age 25-29 (%) | Age 30-34 (%) | Age 35 plus (%)

(b) West Germany

Legend: Age 20-24 (%) | Age 25-29 (%) | Age 30-34 (%) | Age 35 plus (%)

Notes: The figure shows the average wage penalty over the next 15 years when a woman in East or West Germany has a child in the age interval indicated (relative to having no child at any age). The wage penalties are based on Mincerian wage regressions which are run separately for East and West Germany and for the pre-unification period (1989), the early transition years (1990-1994) and later years (1995-1999 and 2000-2004). Control variables include potential experience and potential experience squared, indicators for skill group, the interactions between skill group and the experience profile. Furthermore, indicators whether a child was born at a certain age (20-24, 25-29, 30-34 and 35 and above) are included, as well as interactions with the experience profile and the skill groups to allow wage penalties to vary across skill groups and for different experience levels.

4.3 Theoretical Considerations

Static fertility models (Becker, 1960, 1965; Willis, 1973) provide a framework to analyze the impact of wage changes after unification on completed fertility. Taking children to be normal goods, a permanent increase in household income would raise the demand for children.[11] Permanent wage increases (holding other income constant), in turn, induce both income and substitution effects. Assuming that women are the primary caretaker of very young children, growing wages for men should primarily have an income effect and hence, increase the demand for children.

For female wages, in contrast, the substitution effect, raising her opportunity cost and hence, the price of having a child becomes important. Several studies have reported negative effects of female wages on completed fertility (see Heckman and Walker, 1990; Merrigan and St.-Pierre, 1998; Schultz, 1985; Ward and Butz, 1980, for example). Rising women's wages should then reduce completed fertility in East Germany.[12]

Even holding the total number of children constant, however, couples time their fertility over the life-cycle to maximize lifetime income (Heckman and Walker, 1990 and Hotz et al., 1997). With no skill depreciation (or otherwise lower future earnings) after time away from the labor market, fertility will be high when opportunity costs, i.e. wages, are low. In that case, women tend to have children at the beginning of their career, or even when they are still in education.

No skill depreciation and low wage penalties of motherhood fit the situation prior to unification. Since opportunity costs were low at the beginning of the career and the wage penalties of leaving the labor market were low, women of all skill groups had their first child in their early twenties.[13]

The situation changes completely after unification. Steeper age-earnings profiles, if paired with skill depreciation, imply substantial wage penalties for leaving the labor market. Together, these impose additional opportunity costs of having a child early in the life-cycle because wages are lower later in the career as well. In response, women

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[11]The predictions from the Becker model refer to the quality-adjusted demand for children. Note that quantity predictions are generally ambiguous once quality considerations are taken into account. In the theoretical discussion, we abstract from potential effects on child quality.

[12]If changes in wages after unification are only transitory, there would be no effect on total fertility, but timing of births would still respond. The descriptive evidence, however, clearly suggests that changes in the East German wage structure and income were permanent adjustments to a market economy. Further, if households are uncertain about the nature of wage changes, they might still respond to a permanent shock as if it was transitory. Given the rapid adjustment in East Germany under the guidance of West Germany, we would argue, however, that most households knew the changes in East Germany were permanent.

[13]Most women could return to their old employer after a maternity leave and free fulltime childcare was provided by the state. There were also other benefits of early parenthood, such as access to rationed housing and other family subsidies, which we turn to in the robustness section.

should postpone their births to a later age, if the future career costs exceed the value of having a child early in the life-cycle (see Hotz et al., 1997 for an excellent survey).

In addition, time off the labor market will also interrupt a woman's career and reduce not only future employment, but also one's future career options. If women face fewer career options after time off work (as in a so-called 'mommy track'), they might postpone births until she moves up the age-earnings profile. Germany seems to fit well into this pattern. The labor market in West Germany, which was by and large instituted in East Germany after 1989, is associated with a lot of part-time work by mothers and a substantial wage penalty associated with part-time work.[14] Career options would then be a second explanation why East German women would postpone births to a later period in their life-cycle after unification.[15]

We also expect that high-skilled women respond more strongly to the new labor market incentives after unification. Since human capital loss and wage penalties for time away from the labor market are more important for skilled women, high-skilled women should postpone births even more than low-skilled women (see Gustafsson, 2001 for evidence from Sweden).[16]

4.4 Data Sources

We combine several data sources to analyze the impact of permanent shifts in employment and wage structure on fertility in the East German transition process. We describe each of them in turn. A more detailed description of the sample and the construction of our variables is provided in the Data Appendix.

4.4.1 Fertility Histories

To study changes in fertility decisions of East German women after unification, we need high-quality data on fertility histories in East Germany before and after unification. Such data have recently become available from the German Pension Fund.

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[14]Furthermore, family policy provides generous maternity benefits for an extended leave and the tax system encourages part-time work of the second earner (through income tax splitting).

[15]Imperfect capital markets could be another influence on the timing of fertility. With imperfect capital markets, the path of future wages of other household members, most notably the partner or husband, will influence fertility decisions. In particular, couples will postpone births to periods when the income of the partner is high, as female wages are used to smooth consumption. With human capital loss and career opportunities, it is, however, not clear-cut that fertility will be higher in periods where the partner's wage is high.

[16]In addition, rising returns to education would further delay births among women born 1970 or later who invest in more education after unification (see Black et al., 2008, for example). We will return to this issue below.

This administrative dataset contains the labor market and fertility history of a 0.25% sample of all individuals with at least one pension-relevant spell but no pension receipt by the end of 2007. Overall, the dataset covers approximately 96% of the population born between 1940 and 1982 (see Richter and Himmelreicher, 2008). For these cohorts, the data record all pension-relevant spells on a monthly basis starting from age 15. These include employment in the private and public sector, certain self-employed professions (e.g.artists), long-term illness covered by health insurance, periods of care for an elderly person or children under the age of three.

Most importantly for our purpose, the pension data also record the birth dates of all children which are verified by birth certificates. The reliability of the fertility and employment histories are considered very high. For our empirical analysis, we restrict the sample to women born between 1940 and 1977 because their records have been verified by the federal pension agency and the insured woman.

We define an East German as someone born and raised in East Germany prior to unification. However, the pension data only record the region of residence in 2007. Based on an individual's employment history, we define an East German as someone with a pension-relevant spell in East Germany prior to unification. For younger cohorts, we define East German origin if the first employment spell is in East Germany. If we cannot classify a person based on these rules, we use the region of residence in 2007 instead. Because fertility decisions differ by formal education, we also distinguish three skill levels: the unskilled, the medium-skilled (those with high school or vocational degree) and the high-skilled (those with tertiary education).

To focus on the changes shortly before and after unification, we further restrict the empirical analysis to women aged 20 to 40 years in the period from 1980 to 2000. For each woman, we thus have an unbalanced panel whether a child was born during a year or not, and when the mother was employed or not. Overall, our dataset contains information about 4,400 East German women and 11,400 West German women who had close to 9,000 birth during a total of 157, 434 person-years between 1980 and 2000.

4.4.2 Wages and Household Income

To measure changes in wages and household income for East and West Germans both before and after unification, we use data from the German Socio-Economic Panel (GSOEP). The household panel surveys around 24,000 individuals each year, of which 6,000 are from East Germany, about their wages, household income, employment and household structure. The survey started in 1984 in West Germany and was extended in 1990 to East Germany. Most importantly, the survey asked East Germans in 1990

about their wages and income prior to the fall of the Berlin Wall (May 1989) and prior to German unification (May 1990).

We use the wage information for 1989 and 1990 to construct wages and income for the pre-unification period in East Germany and the data between 1991 and 2009 to characterize wages and income in the post-unification period. For West Germany, we use wage and income information from 1984 to 1989 for the pre-unification period and data from 1991 to 2009 for the post-unification period. Wages are measured as monthly gross earnings and deflated to 2005 Euros. We use several income categories available for 1989 and 1990 (labor and capital income as well as public transfers) to construct a consistent series of household income for the pre- and post-unification period.

As in the pension data, we define three skill groups: low-skilled (those without a vocational training or high school degree), medium-skilled (those with a high school degree and/or a vocational degree) and high-skilled (those with tertiary education). Since the GSOEP elicits the location of residence prior to the fall of the Berlin Wall in 1989, we know exactly who is of East German origin irrespective of their current residence.

4.4.3 Employment and Unemployment Rates

For measures of unemployment and employment rates, we use social security records from the Institute for Employment Research. The data contain social security records of close to 1.4 Mio. individuals in Germany, covering employment spells in the private and public sector as well as unemployment spells (if transfers from the unemployment insurance are received) between 1975 and 2004.

Based on this information, we are able to define employment, unemployment and non-employment rates by age (3-year age bands from age 20 to 40) and skill group (low-, medium-, and high-skilled), separately for East and West Germany and year. We calculate the unemployment rate as the share of unemployed women (men) among all women (men) who are either employed or unemployed on June 30 of each year. Similarly, we calculate employment rates as the share of employed women (men) among all women (men) recorded in the data as either employed, unemployed or non-employed.

For East Germany, the social security records start in 1992. We assign the 1992 information to the missing post-unification year 1991. For the pre-unification period up to 1990, we assign a zero unemployment rate because involuntary unemployment (and unemployment benefits) did officially not exist in the socialist regime. Analogously, we assume that all East Germans were employed prior to unification. Table 4.1 contains

descriptive statistics of our sample as well as the major explanatory variables that we estimate using the following two datasets.

Table 4.1 Summary Statistics

| | East Germany | | West Germany | |
	Mean	Std. Deviation	Mean	Std. Deviation
Probability of a birth	0.05	0.22	0.06	0.24
Career wages if child now	2138.93	483.78	1960.88	604.56
Career wages if child in future	2276.92	500.28	2049.46	634.94
Mean female wages	1240.78	356.01	1859.6	403.96
Mean male wages	1434.52	462.38	2253.12	545.55
Mean household income	1416.05	575.74	1862.45	435.98
Female unemployment rate	7.02	6.97	4.35	1.35
Male unemployment rate	5.92	6.16	5.10	2.05
Female employment rate	83.59	23.59	81.08	5.33
Male employment rate	84.99	23.57	84.7	5.63
Low-skilled	0.07	0.26	0.20	0.40
Medium-skilled	0.81	0.39	0.70	0.46
High-skilled	0.11	0.32	0.10	0.31
Observations	45,099		112,335	

Notes: The sample are women aged 18 to 40 in East and West Germany. Low-skilled do not have any vocational degree, medium-skilled have either completed high school or a vocational degree; and high-skilled have tertiary education.
Source: Scientific-Use File of Administrative Pension Data.

4.5 Estimation and Main Results

4.5.1 Empirical Strategy

We first use the pre-unification period to explore the link between the labor market and fertility in the socialist regime. In a second step, we expand the period of analysis to East German women in the post-unification period. Finally, we pool East and West German women over the full period, which enables us to control for common institutional effects (for example, family policies and other labor market institutions) in the post-unification period.

Our empirical strategy relies on the fact that women were differentially affected by the transition process after 1989. Women who spent most of their childbearing years in the socialist regime (the cohorts born between 1940 and 1955, for example) had completed their fertility before 1990. For these cohorts, unification and the associated changes in the labor market will have little effect on their behavior. Cohorts born

between 1955 and 1966 which were between 23 and 35 were partially affected by unification. Given the young mean age at first births, many of these women have given births to their first child in the socialist regime. Unification mostly affects their decision to have a second child, or more children. The cohorts born 1967 or later − and especially those born after 1970 only enter their childbearing years after unification. For them, the decisions for all births will be affected by the changes in the East German labor market after unification. Faced with substantial skill premia, we also expect that fertility responses are strongest among high-skilled women.

Overall, we exploit variation by birth cohort, time, region and education to estimate variants of the following model:

$$Y_{iaert} = \beta_1 * ChildNow_{aert} + \beta_2 * ChildNext_{aert} + \delta_1 * AverageWage_{aert} +$$
$$+ \delta_2 * Unemployment_{aert} + \gamma_1 * Post1990_t + \gamma_2 * East_r + \quad (4.1)$$
$$+ \gamma_3 * Post1990_t * East_r + \lambda' X_{iaert} + \epsilon_{iaert}$$

where the dependent variable Y_{iaert} are fertility measures for an individual i, at age a, of education group e, in region r and in year t.

Our main variables of interest are $ChildNow_{aert}$ and $ChildNext_{aert}$ which provide a summary measure of the child penalty associated with child birth. Both measures calculate the average career wage per month (corrected for selection in the labor market) when a woman has a child in the current year versus in later years. We expect that higher career wages in the current period delay or reduce childbearing ($\beta_1 < 0$), whereas higher career wages in the future encourage childbearing in the current period ($\beta_2 > 0$).

We also control for aggregate wage growth (of men and women separately) and for unemployment (or employment) rates in an age × year × region × education cell. We further allow fertility to differ between pre- and post-unification, to differ between East and West and allow for a differential shift in East Germany after unification. Finally, we control for education and age effects; in some specifications, we allow for differential age patterns in East Germany. Finally, we include an indicator for the post-unification period to capture other influences in unified Germany, a differential effect for East Germany (and the interaction between the two).

4.5.2 Main Results

Table 4.2 shows marginal effects from a probit model for all births. The first column restricts the sample to the pre-unification period in East Germany. The estimates show that wages, or the costs of having a child now versus later, have little effect on the

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decision to have a child. It is also interesting to note that fertility differences across skill groups are very small. As suggested by the descriptive evidence (see Figure 4.4), medium- and high-skilled women were equally likely to have a child in the socialist economy than low-skilled individuals.[17]

Table 4.2 Effect of Wages and Unemployment on Fertility

| | East Germany | | East and West Germany | |
| | Prior to 1990 | 1980-2001 | 1980-2001 | |
	(1)	(2)	(3)	(4)
Career Wages if Child Now	-0.012	0.006***	0.032***	0.063***
	(-0.43)	(3.58)	(5.53)	(7.55)
Career Wages if Child in the Future	-0.025	-0.007***	-0.016**	-0.048***
	(-1.22)	(-4.57)	(-3.01)	(-6.34)
Mean Wage Women	0.004	0.004**	0.028***	0.030***
	(0.47)	(2.78)	(5.36)	(5.37)
Mean Wages Men	0.004	-0.001	0.020***	0.002
	(0.61)	(0.72)	(4.49)	(0.44)
Unemployment Rate Men		0.000	-0.001	0.000
		(0.43)	(0.14)	(0.01)
Unemployment Rate Women		0.000	-0.005	-0.001
		(-0.87)	(1.05)	(0.22)
Low-skilled	-0.041	0.007	0.309***	0.244***
	(-1.46)	(1.43)	(13.64)	(10.19)
High-skilled	0.586	0.009	-0.543***	-0.412***
	(0.77)	(1.58)	(-11.33)	(-8.26)
Years 1990-1995		-0.018***	-0.064***	-0.046**
		(-6.95)	(-4.54)	(-3.24)
Years 1996-2001		-0.015**	-0.089***	-0.057***
		(-3.23)	(-5.92)	(-3.67)
Post-unification×East			-0.418***	-0.369***
			(-7.52)	(-6.58)
Age fixed effects	Yes	Yes	Yes	Yes
East fixed effects	-	-	Yes	Yes
Age fixed effects × East fixed effect	-	-	-	Yes
Observations	13,574	40,665	158,544	158,544
R-squared	0.102	0.088	0.057	0.059

Notes: The table reports marginal coefficients from a probit model, where the dependent variable is equal to one if a woman has a child in the current year and zero otherwise. Wages, income and unemployment rates are averages at the age, year, region and education level. T-statistics are reported in parentheses.

Subsequent specifications pool pre- and post-unification period for East German women (column (2)) and then include West German women as well (column (3)). The relationship between labor market variables and fertility changes completely. After unification,

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[17]We do not include unemployment in this specification as the official unemployment rate was zero in the GDR.

higher career wages in the current period increase fertility, while higher career wages in future periods reduce fertility. Both male wages (a proxy for other income) and female wages show a positive effect suggesting a positive income effect (which dominates the substitution effect in the case of female wages). The effect of career wages is substantial: a one standard deviation in career wages in the current period would reduce fertility by 4 percentage points (or about 0.2 of a standard deviation in birth rates).

Unemployment rates have opposing effects on fertility: higher unemployment among men reduce fertility − likely driven by an income effect. Female unemployment rates in turn increase fertility suggesting that periods of unemployment reduce the time cost of having a child. Finally, stark differences between high- and low-skilled women emerge after unification: while low-skilled women have higher fertility, high-skilled women have lower fertility than the medium-skilled.[18]

Table 4.3 explores how wages and unemployment affect first versus higher-order births. The results indicate that career wages have a much stronger effect on the timing of the first birth (column (2)). In contrast, the relationship between career wages in the current period and a higher-order birth is weak. Table 4.3 also shows that high employment rates for women reduce the propensity to have an additional child. Further, high-skilled women are more likely to have a second child − in contrast to the pattern observed for first births.

4.6   Robustness Analysis

In this section, we explore the validity of our results to alternative specifications and additional controls. Our main results use a (narrow) definition of unemployment to control for the labor market opportunities. However, actual employment rates in the same age $\times$ education $\times$ region $\times$ year cell might be a better proxy for job opportunities. The first specification in Table 4.4 shows that the basic effect of wages and fertility remains unchanged when we include male and female employment rates as controls.

The second specification includes household income in addition to our wage measures. As expected, we find a positive income effect. The observed increase in household income observed in East Germany after unification should therefore increase the demand

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[18]Perry (2004) shows for the United States that the income effect dominates for high-wage earners (positive effect of female wages on fertility), while the substitution effect dominates for low-wage earners (negative effect of female wages on fertility). Hence, we might expect that fertility would decline for the low-skilled, but increase for the high-skilled after unification. Our evidence points to the opposite pattern for the post-unification period.

Table 4.3 First Births versus Higher-order Births

Probit Model	East and West Germany Pooled		
	All Births	First Births	Higher-order Births
	(1)	(2)	(3)
Career Wages if Child Now	0.033***	0.003***	-0.004*
	(5.65)	(-6.04)	(-2.00)
Career Wages if Child in the Future	-0.019***	-0.001	0.002
	(-3.50)	(-1.68)	(0.90)
Mean Wage Women	0.028***	0.002***	0.005**
	(5.32)	(5.06)	(-2.69)
Mean Wages Men	0.018***	0.001***	-0.004**
	(3.98)	(3.44)	(-2.86)
Employment Rates Men	-0.004	0.000	0.001
	(-1.26)	(-0.40)	(-1.34)
Employment Rates Women	0.017***	0.001***	-0.002*
	(5.17)	(4.81)	(-2.35)
Low-skilled	0.402***	0.039***	-0.017*
	(13.99)	(10.06)	(-2.27)
High-skilled	-0.545***	-0.025***	0.056***
	(-11.77)	(-24.66)	(4.30)
Years 1990-1995	-0.074***	-0.002*	-0.034***
	(-5.24)	(-2.19)	(-9.19)
Years 1996-2001	-0.014	0.000	-0.030***
	(-0.70)	(0.27)	(-5.40)
Post-unification × East	-0.290***	-0.011***	-0.043***
	(-6.12)	(-4.61)	(-4.15)
Time period used for estimation	1980-2001	1980-2001	1980-2001
Age fixed effects	Yes	Yes	Yes
East fixed effect	Yes	Yes	Yes
Observations	158,544	120,827	37,717
R-squared	0.057	0.074	0.051

Notes: The table reports coefficients from a probit model, where the dependent variable is equal to one, if a woman has a child in the current year, and zero otherwise (in colum (1)); whether a woman has her first birth in the current year (in colum (2)), or has a second birth or higher-order births in the current year (in colum (3)). Wages, income and unemployment rates are averages at the age, year, region and education level. T-statistics are reported in parentheses.

for children − which works against the negative fertility shock in the post-unification period shown in Figure 4.1.[19]

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[19]Unification has not only affected wages and employment opportunities in East Germany. In addition, East Germany basically introduced the legal and institutional framework from West Germany. To control for the influence of other policy changes, a future step is to explore the robustness of our results to the inclusion of other public policies. See Kreyenfeld and Mika (2004) for a review of public

Table 4.4 Robustness Check

| Probit Model | East and West Germany Pooled over 1980-2001 | | | | | |
| | Employment Rates | | Household Income | | Supply of Childcare | |
	(1)	(2)	(3)	(4)	(5)	(6)
Career Wages if Child Now	0.033***	0.061***	0.032***	0.061***	0.060***	0.052***
	(5.65)	(7.39)	(5.48)	(7.35)	(7.30)	(6.10)
Career Wages if Child in the Future	-0.019***	-0.048***	-0.019***	-0.049***	-0.049***	-0.045***
	(-3.50)	(-6.36)	(-3.56)	(-6.52)	(-6.51)	(-5.86)
Mean Wage Women	0.028***	0.030***	0.024***	0.024***	0.024***	0.020***
	(5.32)	(5.42)	(4.26)	(3.99)	(3.99)	(3.40)
Mean Wages Men	0.018***	0.001	0.017***	-0.001	-0.001	-0.001
	(3.98)	(0.16)	(3.87)	(-0.11)	(-0.16)	(-0.19)
Household Income			0.000	0.0001**		
			(0.00)	(0.00)		
Employment Rates men	-0.004	-0.005	-0.003	-0.005	-0.005	-0.014**
	(-1.26)	(-1.72)	(-1.24)	(-1.71)	(-1.55)	(-2.99)
Employment Rates Women	0.017***	0.013***	0.018***	0.014***	0.014***	0.041***
	(5.17)	(3.91)	(5.34)	(4.20)	(4.20)	(7.00)
Household Income			0.006	0.009**	0.009**	0.013***
			(1.85)	(2.97)	(3.07)	(4.34)
Supply of Childcare Slots					-0.000**	-0.000*
					(-2.58)	(-2.39)
Low-skilled	0.402***	0.308***	0.398***	0.298***	0.300***	0.354***
	(13.99)	(9.97)	(13.86)	(9.63)	(9.68)	(10.96)
High-skilled	-0.545***	-0.425***	-0.532***	-0.401***	-0.395***	-0.311***
	(-11.77)	(-8.74)	(-11.36)	(-8.16)	(-8.03)	(-6.12)
Years 1990-1995	-0.074***	-0.054***	-0.082***	-0.065***	-0.065***	-0.054***
	(-5.24)	(-3.75)	(-5.53)	(-4.39)	(-4.34)	(-3.56)
Years 1996-2001	-0.014	-0.003	-0.031	-0.03	-0.028	0.024
	(-0.70)	(-0.16)	(-1.45)	(-1.44)	(-1.35)	-1.06
Post-unification × East	-0.290***	-0.280***	-0.286***	-0.275***	-0.297***	-0.304***
	(-6.12)	(-5.80)	(-6.04)	(-5.71)	(-6.12)	(-5.42)
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
East fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Age FE × East FE	-	Yes	No	Yes	Yes	Yes
Observations	158,544	158,544	158,544	158,544	158,544	158,544
R-squared	0.057	0.060	0.057	0.060	0.060	0.060

Notes: The table reports marginal coefficients from a probit model, where the dependent variable is equal to one, if a woman has a child in the current year, and zero otherwise. Wages, income and unemployment rates are averages at the age, year, region and education level. T-statistics are reported in parentheses.

4.7 Conclusion

Our results suggest that the fertility crisis in East Germany was the combined result of a postponement of first birth and a decline in higher-order births among East German women. These fertility changes are systematically related to changes in the East German labor market. Most importantly, as wages started to reflect market prices

policies in East Germany before unification. For the evolution of household income in East and West Germany, see Figure C.7 in the Appendix.

for human capital, young women faced high potential returns to education and work experience after unification. As expected, these changes encourage career investments at the expense of early childbearing. These changes occurred despite the excellent infrastructure of childcare, outmigration to West Germany or abroad or changes in family policies and financial support. As such, the evidence supports the idea that work incentives in the labor market have an important influence on childbearing decisions.

Appendix C

C.0.1 Figures and Tables

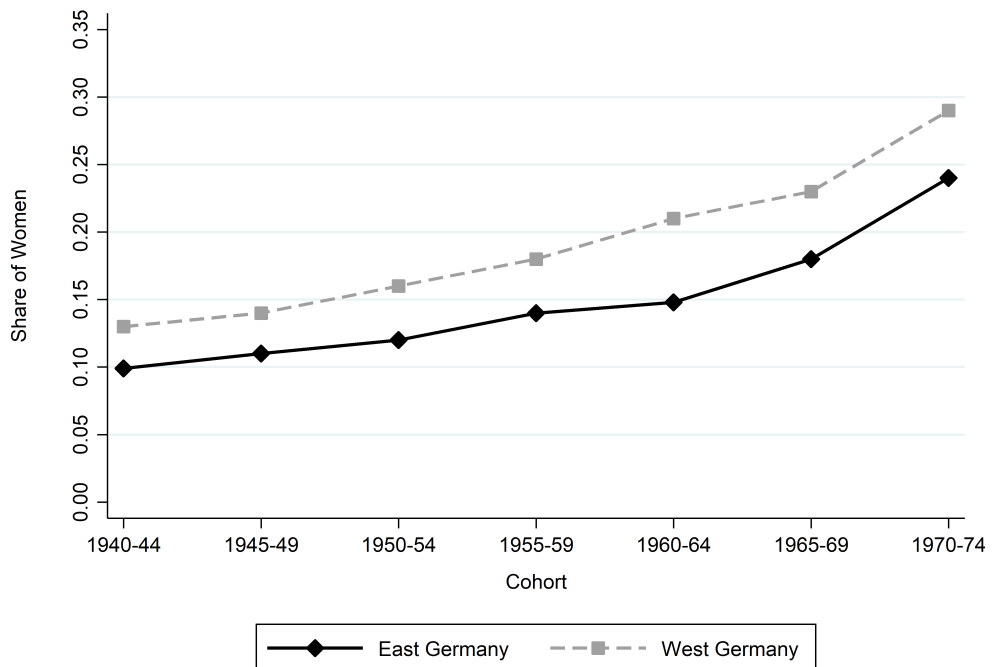Fig. C.1 Mean Age at First Birth in East and West Germany
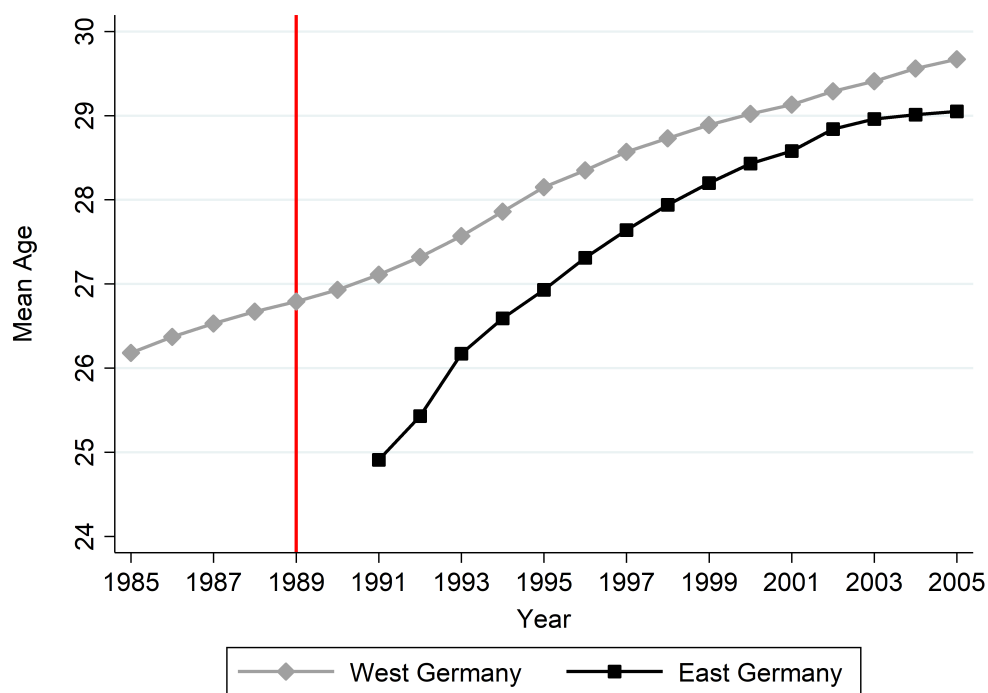
Source: Federal Statistical Office.

Fig. C.2 Mean Age at Higher-order Births in East and West Germany
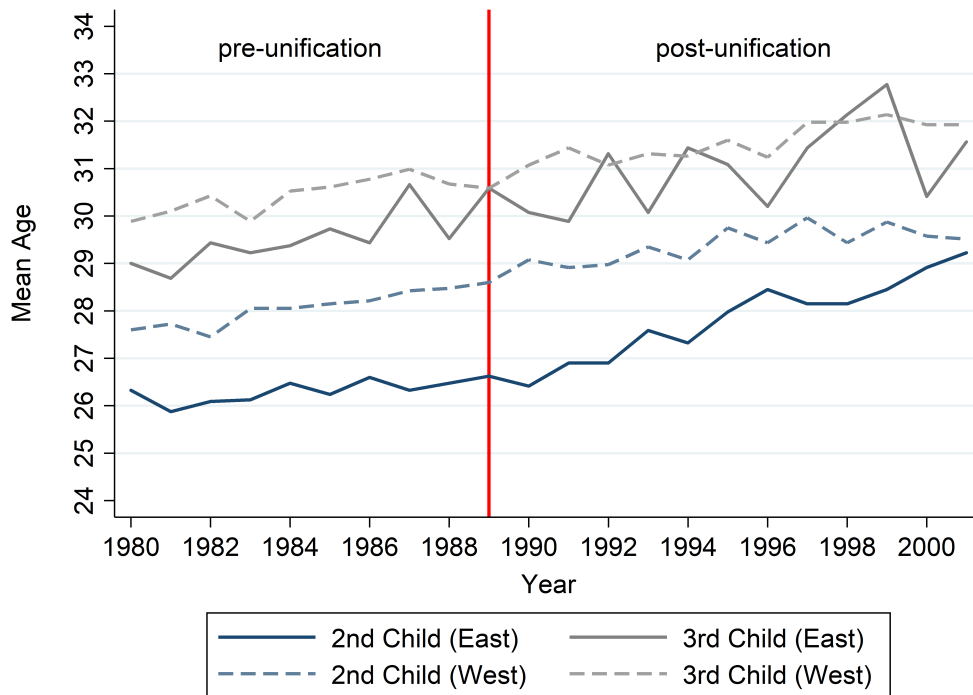
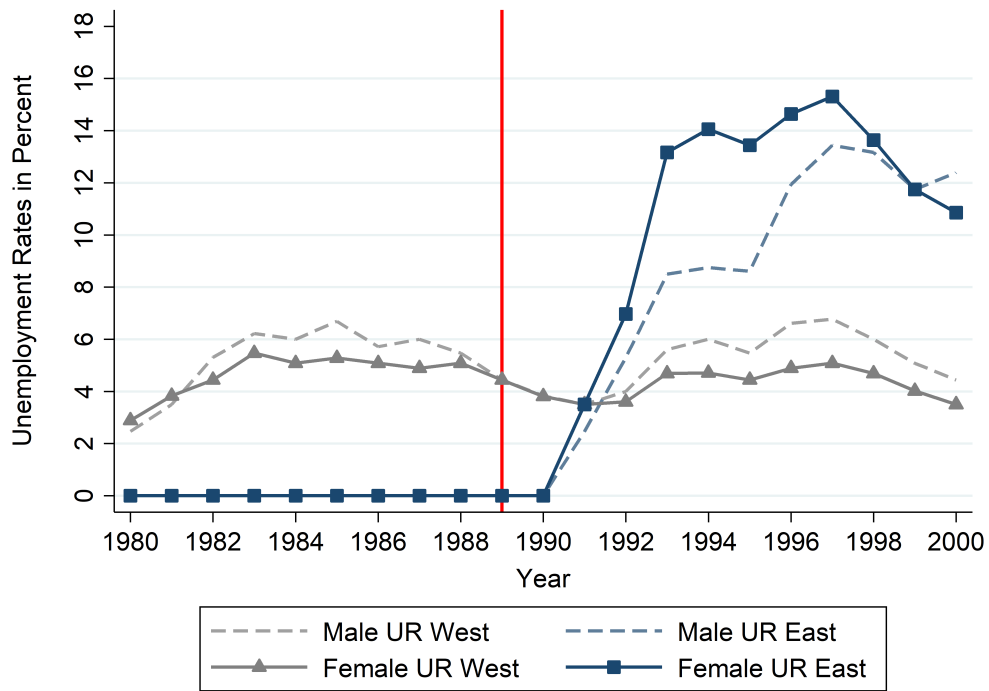*Source*: German Aministrative Pension Data.

Fig. C.3 Share of Women without Children in 2008 by Cohort

Notes: The Micro Census in 2008 reports the total number of children ever born. East Germany refers here to current residence in East Germany.
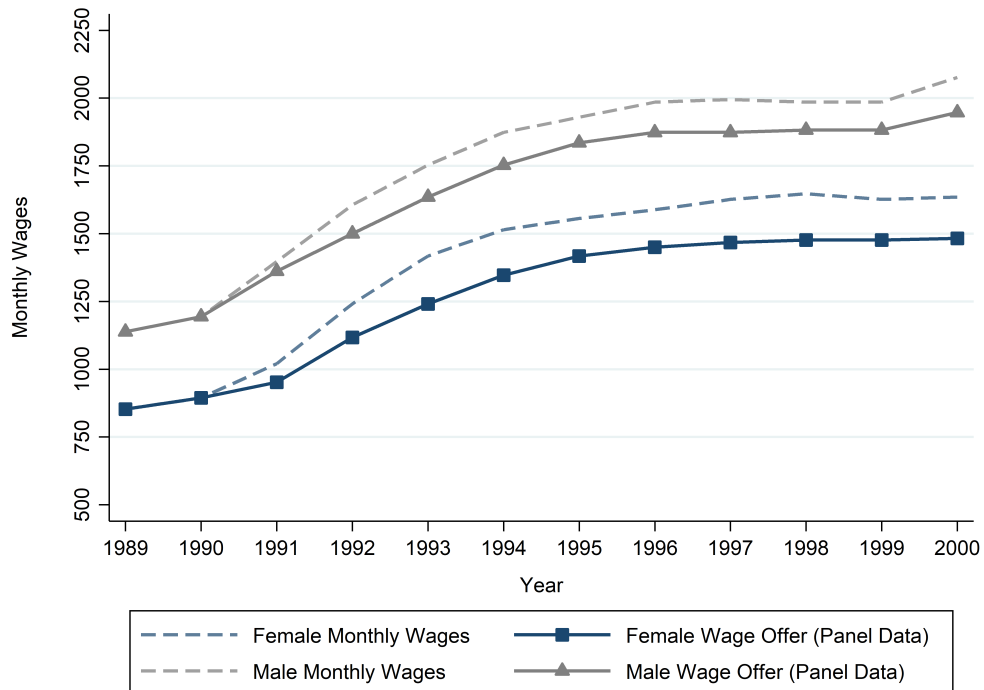*Source:* Micro Census (2008).

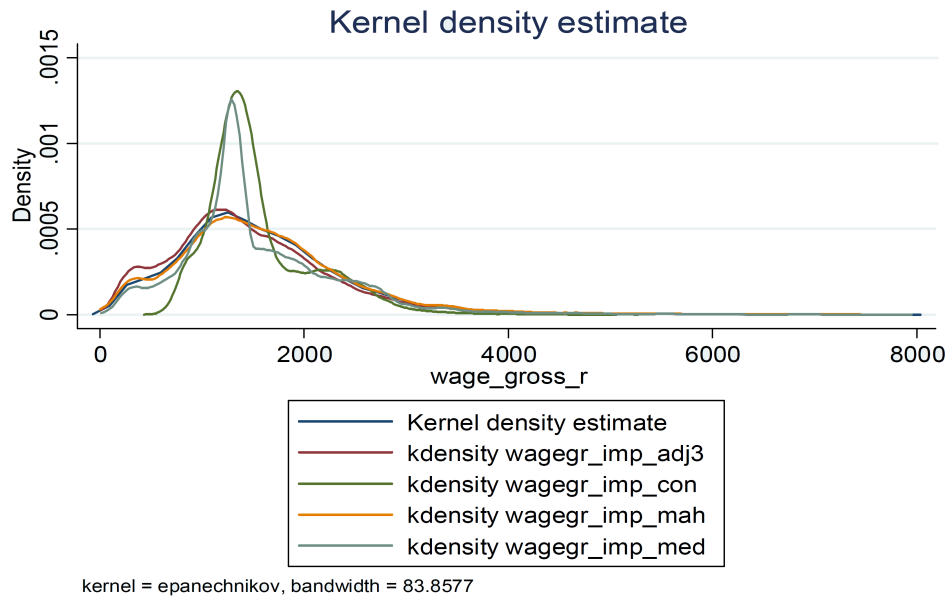Fig. C.4 Unemployment Rates in East and West Germany between 1980 and 2000

Fig. C.5 Average Wage Growth in East Germany between 1989 and 2000

Fig. C.6 Kernel Density Estimates of Monthly Real Wages using Different Imputation Techniques

Kernel density estimate

kernel = epanechnikov, bandwidth = 83.8577

Notes: The figure shows kernel density estimates of actual and imputed monthly real wages for East German women. The imputation methods are: exploiting the panel structure to assign wages (wahegr_imp_adj3), using a control function approach (wagegr_imp_con), using a matching approach (wagegr_imp_mah) and using median restrictions (wagegr_imp_med).

Fig. C.7 Evolution of Household Income in East and West Germany
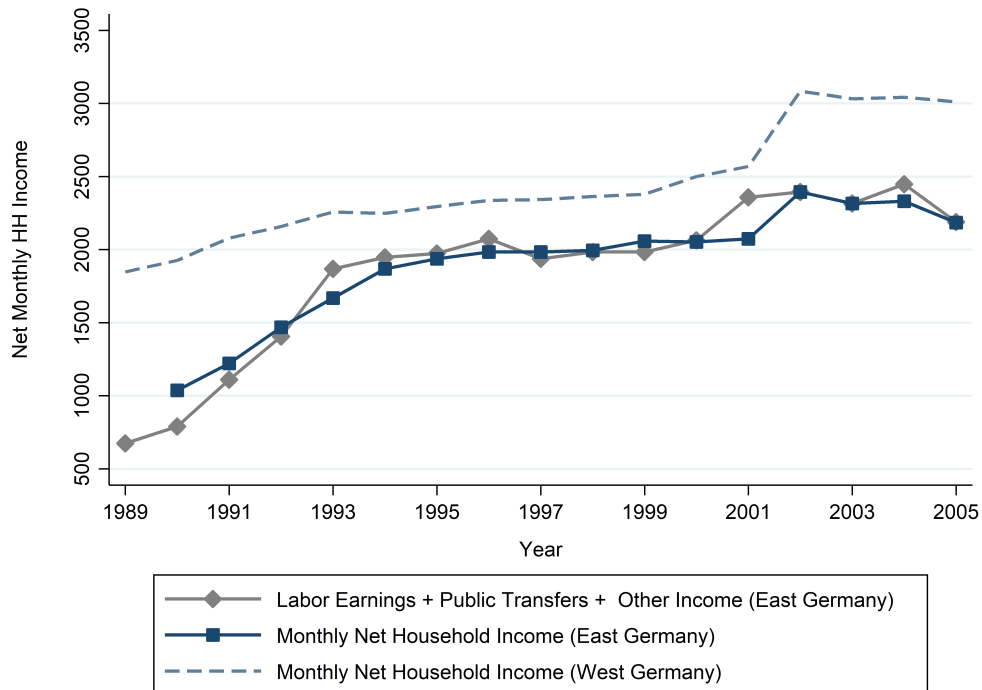
Notes: The figure shows data on net monthly household income from 1990 to 2005 and an alternative calculation of net household income as the sum of labor earnings, public transfers and other income (e.g.rental income or other capital income).
Source: German Socio-Economic Panel (GSOEP).

Table C.1 TFR and Tempo-Adjusted TFR in Germany

	East Germany		West Germany	
	TFR	Adj. TFR	TFR	Adj. TFR
1980-1985	1.82	1.83	1.37	1.62
1986-1990	1.53	1.91	1.4	1.5
1991-1995	0.87	-	1.39	1.5
1996-2000	1.06	1.51	1.41	1.55
2001-2005	1.26	1.57	1.38	1.62

Notes: The table reports the total fertility rate, i.e. the sum of age-specific fertility rates in each year for women between age 18 and 45. The adjusted fertility rate employs the method by Bongaarts and Feenstra (1998) to adjust for tempo effects using the birth order and age of the mother. Adjusted TFR in East Germany between 1986 and 1990 are based on the years 1986-1988, the adjusted TFR for 1996-2000 are available for 1997-2000 only.
Source: Human Fertility Database (2013); Luy and Pötzsch (2011).

C.0.2 Fertility Histories

The data for fertility histories before and after German unification come from the German Pension Fund (FDZ-RV), the so-called VSKT 2007. It is the most comprehensive data source for individual fertility and employment biographies in Germany. In particular, it covers full fertility and employment records for a 0.25% sample of all actively insured and not yet retired individuals who have at least one pension-relevant spell by the end of 2007.

Since employment in the private and public sector is subject to mandatory pension contributions, the vast majority of the German population has at least one record in the pension data. Additional pension-relevant spells include certain self-employed professions (e.g. artists), periods of long-term illness covered by health insurance and periods of care for an elderly person or children under the age of three.

The reliability of these administrative records is very high. Prior to be used for calculating pension payments, the account has to be authorized by the individual concerned. A person can voluntarily authorize her pension account at any time. For individuals aged 30 or older, the pension fund actively requests the authorization of pension accounts. As a consequence, the data for 2007 are most reliable for cohorts born until 1977, which are 30 years or older in 2007. The data contain a relatively high share of non-authorized accounts for cohorts born after 1977. Since authorized accounts tend to underreport births, we restrict the analysis to cohorts born between 1940 and 1977.

The data contain the birth dates of children ever born until 2007 by the respective birth cohorts. The total fertility rate we calculate based on these data slightly underestimates the actual fertility rate because children born to civil servants or women not in the pension system in 2007 (because of death or prior pension receipt) are not recorded. Prior evidence suggests that the data miss only about 5% of births. Hence, we think that the bias is small, especially compared to other survey data (see Kreyenfeld, 2006a).[1] The pension data thus provide the most comprehensive data on fertility histories in Germany.

Children are almost always recorded in the mother's pension record. Only about 0.04% of children are recorded in the father's pension record, mostly when the father is widowed (see also Kreyenfeld, 2006a). In the authorization process, the existence of children recorded are verified by birth certificates.

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[1] The comparison refers to completed pension records ('vollendete Versichertenkonten'). Alternatively, we could simply compare our TFR estimates with the official estimates by the Federal Statistical Office.

The pension data, like other administrative data sources, only record the current residence in 2007. Current residence in 2007, however, does not account for East German women who moved to West Germany and vice versa. To define women of East German origin, we proceed as follows: we know the region of origin of all women with an employment spell prior to unification. For all other women, we rely on the employment history from the social security records that has been merged to the pension data. We use the region of the first recorded employment spell after unification to identify a woman from West or East Germany. For the remaining missings, we use the place of residence in 2007 to assign East and West German origin. Any measurement error from this procedure will be stronger for the youngest cohorts because they might not have had an employment spell. We return to the issue of East-West migration in the robustness section.

For our analysis, we distinguish between unskilled women, women with a vocational training degree and women with tertiary education. The pension data report educational attainment only for the employment spell that is recorded in 2007. To fill in missing values, we use the same strategy as for the region of origin above. Based on the full employment histories from the social security records between 1975 and 2007, we assign the educational information available there. Based on this additional information, we are able to reduce missings on formal education to 9.8% for East German and 16.4% for West German women. Most of the missings in East Germany are for older women who completed their fertility history prior to unification, and hence, should have little effect on our results.

C.0.3   Wage and Household Income Data

To construct measures of wages and income before and after unification, we use the German Socio-Economic Panel (GSOEP).[2]

The annual panel surveys around 12,000 households about their wages, household income, employment and household structure. Currently, about 24,000 individuals, of which 6,000 are from East Germany, participate in the survey. The survey started in West Germany in 1984 and extended to East Germany in 1990. We restrict the sample to men and women between the ages of 20 and 60.

In 1990, the survey asked East Germans about their wages and income prior to the fall of the Berlin Wall (May 1989) and prior to German unification (May 1990). We use the wage information for 1989 and 1990 to construct wages and income for the pre-

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[2]While the pension records contain some indication about the level of earnings during an employment spell (the so-called "Entgeltpunkte"), it is difficult to infer actual earnings from this information in a consistent way.

unification period in East Germany; we use data between 1991 and 2009 to characterize wages and income in the post-unification period. For West Germany, we use wage and income information from 1984 to 1989 for the pre-unification period and data from 1991 to 2009 for the post-unification period. Wages are measured as monthly gross earnings and converted to 2005 Euros using the national consumer price index.

East Germans were also asked in 1989 and 1990 about several sources of income. We combine those with similar categories for 1991-2009 to construct a consistent series of household income for both the pre- and post-unification period. Household income is measured as the sum of labor, capital and public transfers. Labor income includes earnings as well as bonus payments. Public transfers are calculated as the sum of maternity benefits, maternity leave payments, regular pensions, pensions for widows and orphans, student grants, unemployment benefits and welfare benefits.

As in the pension data, we distinguish three skill groups: low-skilled (those without a vocational training or high school degree), medium-skilled (those with a high school degree and/or a vocational degree) and high-skilled (those with tertiary education). Finally, the GSOEP elicits both the current residence (East or West Germany), as well as the location of residence prior to the fall of the Berlin Wall in 1989. For the main analysis, we use women of East German origin (who lived in East Germany in 1989), irrespective of their current residence.

C.0.4 Employment and Unemployment Rates

For measures of unemployment and employment rates, we use administrative data from the social security records by the Institute for Employment Research. These data contain a 2% sample of all social security records in Germany, thus covering spells of employment in the private and public sectors between 1975 and 2004. The data only exclude the self-employed and civil servants. In addition to the employment spells, we have information on the receipt of unemployment insurance benefits.

Based on this information, we are able to define three types of spells: employment, unemployment and non-employment. Employment spells are clearly identified in the data. The distinction between unemployment and non-employment in contrast, is not always clear-cut because a person is only recorded as unemployed in the data if she receives unemployment insurance benefits. While unemployment benefits are paid by the unemployment insurance to all unemployed with a minimum work record, the receipt of unemployment assistance (after unemployment insurance benefits are exhausted) is means-tested. As a consequence, a long-term unemployed person may not be recorded

as unemployed in the social security data. Instead, the spell might be recorded without any further information on the labor market status of the person involved.

To solve this issue, we use two alternative measures: a narrow and a broad definition (see Fitzenberger and Wilke, 2010 for details). The narrow definition defines the unemployment rate as the share of women (men) who are unemployed among all unemployed and employed women (men). As this unemployment rate is tied to benefit receipt, it is a lower bound to the true unemployment rate. The broad definition of unemployment also includes spells without any further information, as long as there has been an initial receipt of unemployment benefits. We then define the employment rate as the share of women (men) who are employed among all employed, unemployed and non-employed women (men). The resulting employment rates provide an upper bound to actual employment; because the data does not include working-age individuals who have never been employed prior to becoming non-employed.

All unemployment and employment rates are calculated for June 30 of each year between 1980 to 2000. We calculate separate rates by skill group, age, East and West Germany. As in the other data sets, we distinguish low-skilled (those without a vocational training or high school degree), medium-skilled (those with a high school degree and/or a vocational degree) and high-skilled (those with tertiary education). We further distinguish between West and East Germany based on current residence. Finally, we calculate the unemployment rates for seven age groups between age 20 to 40 (age 20-22, 23-25, 26-28, 29-31, 32-34, 35-37, 38-40).

For West Germany, we have 441 cells of unemployment/employment rates (21 years × 7 age groups × 3 education groups). For East Germany, we only have post-unification information starting in 1992. We apply the 1992 information to the missing post-unification year 1991. For the pre-unification period up to 1990, we assign an unemployment rate of zero because involuntary unemployment (and hence, unemployment benefits) did not really exist in the socialist regime. Similarly, we assume that all working-age individuals were employed in East Germany prior to unification.

C.0.5 Construction of the Wage Offer Distribution

While most East German women worked in the GDR, employment rates declined substantially after unification. To adjust wages for selective non-participation, we employ methods commonly used in the literature on racial and gender wage gaps (Chandra, 2003; Johnson et al., 2000; Neal, 2004; Olivetti and Petrongolo, 2008).

The first method exploits the panel structure of the GSOEP. We use available information in adjacent waves (± 1 year or ± 3 years) to fill in missing wages. In addition, we

construct mean wages using adjacent wages after adjusting for aggregate wage growth (by adding or subtracting a growth factor).

As an alternative, we use a semiparametric control function approach. In a first step, we predict employment with a probit model. As exclusion restriction, we use household size and the presence of a partner in the household. In the second step, we include a flexible cubic function of the predicted employment probability in a Mincer earnings regression.

Our third approach uses mahalanobis matching to predict wages for those not employed in a year. Specifically, we match on education, labor market experience (linear and squared), interactions between education and labor market experience, marital status, household size and the presence of a partner, as well as state and year fixed effects.

Finally, we use various assumptions about the position of non-workers in the wage distribution to impute missing wages. The basic assumption is that all non-workers would earn wages below the median. A somewhat weaker assumption is that non-workers would earn below the median conditional on their education, age, gender and region. Alternatively, we assume that non-workers (conditional on education, age, gender, region and year) earn below the median if they have not worked in the past two years; otherwise, non-workers earn the median wage.

Figure C.6 in the Appendix shows wage densities for alternative imputation methods and compares it to the observed wages without imputation. The main empirical analysis uses average wages, where missing values are imputed from the panel structure (available data in the three adjacent waves), or a median restriction (assuming that non-workers earn wages below the median wage in the respective year). In the robustness section, we probe the validity of our main results to alternative wage imputation methods.

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