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Microeconometric Analyses on Determinants of Individual Labour Market Outcomes

Lisa K. Simon

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Lisa K. Simon

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I dedicate this thesis
to Uwe, Babcia i Dziadek, und Elke.
I know you were there.

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Preface

Lisa K. Simon prepared this study while she was working at the ifo Center for the Economics of Education. The study was completed in September 2018 and accepted as doctoral thesis by the Department of Economics at the University of Munich. It consists of four distinct empirical analyses: Chapters 2 and 3 deal with the impact of some individual characteristics on labour market outcomes, while Chapters 4 and 5 deal with the impact of some external factors. Chapters 2 and 3 employ experimental methods, while Chapters 4 and 5 use observational data, especially administrative social security data.

Keywords: Education, Labour Market Outcomes, Human Capital, Cognitive Skills, Refugees, Education Level, Labour Market Deregulation, Reform Evaluation, Survey Experiment, Social Security Data, Occupation Choice, Vocational Occupation, Trade Shocks

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1 Introduction

This thesis is about how individuals fare on the labour market and the different determinants affecting this. The determinants investigated here range from signals of skills and migration, to the regulatory framework and changing industry structures. The common denominator is education and how it helps to navigate the labour market and the challenges it poses. In the most immediate way of course, there is a well-established relationship between education and labour market outcomes, such as positive returns to years of schooling (*e.g.,* Card, 1999). The channels which drive this relationship, have been subject to much interest in the literature on education and labour economics.

One way in which education helps to navigate the labour market is by acting as a signal, meaning a transfer of information on productivity, adaptability, skilfulness or reliability. An individual's high school degree signals to potential employers, not only that the individual fulfilled the formal requirements needed to finish high school, but also that this person carries important personal characteristics such as reliability and persistence, which allowed him or her to graduate from high school. In his seminal paper, Spence (1973) derives a model in which education is used as a signal of productivity on the job market, where signalling costs and ability are inversely related. This explains why individuals obtain varying signals of education, *e.g.,* why some obtain a high school degree and others a PhD. In contrast to the theory of screening (Arrow, 1973), this thesis makes the assumption that education actually transmits human capital and increases productivity, and is not only a mere signal of other related adeptness. Either way, signals of education and skills play a paramount role in informing the labour market and future employers, about the individual's suitability for a certain job. Chapter 2 of this thesis investigates in depth, which skill signals matter in getting a job. Chapter 3 looks at the importance of education for refugees and natives' attitudes towards refugees.

Another way in which education helps to navigate the labour market, is by acting as an insurance against changing conditions, because it provides the ability to adapt to new situations and to learn new skills (*e.g.,* Nelson and Phelps, 1966; Welch, 1970; Schultz, 1975; Heckman, 2000). Having skills which allow to adapt to new challenges, is paramount in an ever faster-evolving economy. Globalisation (*e.g.,* Autor, Dorn and Hanson, 2013) and technological progress (*e.g.,* Levy and Murnane, 1992) are two important drivers of change. Globalisation, with international trade, free mobility

of labour and ever cheaper transport and communication, puts increasing competitive pressure on the domestic production of goods and services. This requires domestic industries to remain competitive, through higher productivity, better quality, better service or other ways of differentiation. Technological progress with automation increases productivity of many processes, but also requires higher-skilled labour to complement the increasingly automated production processes. A well-educated labour force equipped with adaptable skills is therefore necessary to navigate these challenges. Chapter 5 focusses on educational choices of young individuals that are faced with changing industry structures in their surroundings. Chapter 4 on the other hand, looks at the labour market effects of a change in competition dynamics through a lift in regulation.

Globally, the first two chapters of this thesis deal with the effects of certain individual characteristics on labour market outcomes, while the last two chapters focus on certain external factors. In the remainder of this introduction, Sections 1.1 and 1.2 introduce the respective key concepts and literatures in turn. Section 1.3 discusses some of the empirical challenges when one is interested in causal inference on these topics and introduces the empirical strategies used in this thesis. Section 1.4 provides an overview of the four chapters, and Section 1.5 summarises the main insights and the policy implications one can draw from them.

1.1 Selected Individual Characteristics and Labour Market Outcomes

This section discusses selected individual-level determinants of labour market outcomes, in particular the role of educational attainment – a personal characteristic in which individuals invest in. First, education and skills as signals of productivity on the labour market are discussed, and then the role of education for immigrants and refugees is introduced.

1.1.1 Education as a Signal

Education, in the sense of educational degrees (*e.g.,* high school diploma, bachelors degree), can be used as a signal, for example about one's ability and productivity to potential future employers (Spence, 1973; Arrow, 1973; Stiglitz, 1975). It acts as an information transfer or message about a trait that is not directly observable. Because future employers cannot directly observe skills and productivity of candidates they

want to hire for a job opening, they have to make a decision based on partial information available to them, which is based on the signals the candidates transmit.

Chapter 2 looks at the relationship between education signals and labour market outcomes in more depth, namely at the importance of skill signals in the hiring process of labour market entrants. Which skills denoted on a CV matter in particular to employers? In the literature, various categories of skills have been found to matter, usually distinguishing between cognitive and non-cognitive skills.[1]

Cognitive skills, defined as the ability of an individual to perform mental activities associated with learning and problem solving, are often measured as achievement on standardized tests.[2] For the economy as a whole, *e.g.,* Hanushek and Woessmann (2008) show the importance of cognitive skills for economic growth and development. In terms of individuals, Chetty et al. (2011) and Hanushek et al. (2015) document that cognitive skills are positively related to employment and earnings. Non-cognitive skills are not as precisely defined nor understood, but are receiving growing attention in the literature, in particular as better defined subcategories. Social skills have been recognised as particularly important, due to the growing importance of team production and interpersonal interactions on the labour market. As far as signalling social skills goes, Baert and Vujić (2018) and Heinz and Schumacher (2017) find that social volunteering may be used to credibly signal the willingness to cooperate, but it may also be correlated with other skills valued by employers. In Chapter 2, we argue that social volunteering is particularly related to personality traits that include maturity, conscientiousness, perseverance, and curiosity, which is another highly relevant subgroup of non-cognitive skills to employers. How important these traits are to employers is also shown in Heckman, Humphries and Mader (2011), who find that even conditional on cognitive skills, high school graduates outperform GED[3] recipients in terms of labour-market outcomes and show that this difference is driven by personality traits of reliability and perseverance.

Education has further been shown to not only affect cognitive ability, but also to have an impact on crime, health, fertility or political participation (Hanushek and Woess-

[1] Recent literature does recognise that the strict dichotomy between cognitive and non-cognitive skills does not necessarily hold, as skills from both categories also have elements of the respective other to varying degrees. For example, a good high school GPA contains both classic cognitive elements as doing well on tests, but also the ability to interact with teachers and peers and the discipline to study or do homework, which are more non-cognitive skills (*e.g.,* Balart, Oosterveen and Webbink, 2018).

[2] Earlier literature measures cognitive skills in terms of years of schooling, but it has been duly argued that school quality varies too much and that actual skills are a much better predictor of abilities (see *e.g.,* Hanushek et al., 2015).

[3] The GED is a test that is usually used by high school drop-outs to earn their high school equivalence credential.

mann, 2008), and can therefore act as a signal of these traits. In Chapter 3, education acts as a signal of the potential and willingness to integrate in the labour market for refugees, and in Chapter 4, it acts as a signal of quality to consumers.

1.1.2 Migration and Education

Migration can change a country's labour market skill composition, when immigrants have different levels of skills than the native population on average, or when particularly high or low education natives leave the country to emigrate (*e.g.,* Dustmann and Glitz, 2011). This can have varying impacts on the labour market; for example, Ottaviano and Peri (2012) find that comparative skill advantages of migrants and natives lead to occupational specialisation and therefore increased productivity.

In 2014 and 2015, Europe experienced an unprecedented influx of refugees. In 2015 alone, more than 1.5 million individuals applied for asylum in Europe, with Germany registering the highest number of some 440,000 applications (Eurostat, 2016). There was much uncertainty surrounding the true education level of the incoming refugees due to poor documentation and lacking data. To implement feasible refugee and integration policies, it is important that these are supported by domestic voters in order to successfully implement these policies and to preserve solidarity with refugees. The fact that public support for anti-immigration parties increased markedly in several European countries during the refugee crisis, suggests that voters' scepticism towards refugees and national asylum policies have not been fully appreciated by policy makers.

Chapter 3 tests how beliefs about the education level of refugees impact natives' attitudes towards refugees. Economic models on attitudes towards immigration emphasize the importance of the education level of migrants, and natives' beliefs about it. Hainmueller and Hiscox (2010) discuss two competing theories on how the skill level of immigrants affects natives' attitudes towardss them. According to the labour market competition model, natives are most opposed to immigrants with a skill level similar to their own because they expect these immigrants to compete for the same types of jobs (*e.g.,* Mayda, 2006; Scheve and Slaughter, 2001). The fiscal burden model, on the other hand, predicts that natives in general are more opposed to low-skilled immigrants because they impose larger fiscal burdens on the public than high-skilled immigrants.

The education level of refugees acts as a signal of the ability to integrate, participate and compete on the labour market, as well as of future potential earnings and tax contributions. Of course, beliefs about refugees' education may affect general attitudes

through other channels than labour market competition concerns and fiscal concerns (*e.g.,* Bauer, Lofstrom and Zimmermann, 2000; Dustmann and Preston, 2007). Therefore, Chapter 3 also assesses the relevance of alternative concerns such as increasing crime levels as potential channels.

1.2 Selected External Factors and Labour Market Outcomes

This section introduces some external and exogenous factors that determine labour market outcomes, *i.e.* aspects that individuals cannot alter. It becomes quickly evident that education is important to protect individuals from unexpected changes that are outside their control.

1.2.1 Occupational Licensing as Entry Barrier

Entry barriers are costs that new entrants have to pay to become active in a given product market such as standardization of products, minimum capital requirements, and time-consuming registration procedures to licenses that are required to run a business (Djankov, 2009; Kleiner, 2000). Occupational licences are one particular form of entry barriers and are usually an educational requirement that individuals have to posses in order to practice an occupation. Doctors or lawyers are two obvious examples of professions, which require occupational licences. Advocates for entry barriers argue that they can secure and improve the quality of the products and services provided (Arruñada, 2007). For example, Anderson et al. (2016) find that occupational licensing in medical professions can be highly beneficial by showing that the licensing of midwives in the early 20th century in the United States led to reductions in infant and mother mortality. Opponents argue that entry barriers lead to inefficient allocations of resources because they restrict competition and create rents for incumbent firms (Peltzman, 1976; Posner, 1975; Stigler, 1971).

Chapter 4 analyses the effects of a reform that removed an occupational requirement in the crafts sector that was previously necessary to own a business in the respective craft occupation. The *Master Crafts Certificate*, a costly (both in terms of money and time) advanced professional degree, acted as a substantial entry barrier for craftsmen to become self-employed, and was removed for roughly half of all craft occupations in 2003. Standard economic theory predicts that reducing entry barriers for firms leads to increasing entrepreneurial activity through increasing the number of active firms (*e.g.,* Bertrand and Kramarz, 2002; Mullainathan and Schnabl, 2010). This is exactly

what happened in the case of this reform: the number of firms in deregulated occupations increased by up to 300 percent.

The predictions on how wages and employment evolve following an increase in competition are less clear cut. If incumbent firms and new entrants are effective competitors, meaning that they engage in similar markets and compete for similar resources (Chen, 1996), then incumbent firms may react by increasing investments (Alesina et al., 2005) and innovative activity (Aghion et al., 2004; Griffith, Harrison and Simpson, 2010) to keep their long-run competitive advantage. However, new firm entry can also be detrimental to innovation and growth by diminishing rents and thereby decreasing incentives to innovate and invest (Aghion et al., 2005).

It is important to understand the potential earnings and employment outcomes of workers who work in a firm that is affected by a deregulation. In many cases, the main beneficiaries of entry regulations are incumbent firms, which are protected from competition by entry barriers and can therefore raise economic rents through charging markups on prices.[4] There is strong evidence that firms share their economic rents with their employees, causing higher wages in many regulated markets and industries.[5] To protect their product market position, firms may try to save costs following a deregulation reform by revising wages of their workers downwards. However, to retain their competitive advantage, firms may also choose to invest in the human capital of their workforce because firms with more skilled labour work more efficiently. Evidence on this channel comes from Fernandes, Ferreira and Winters (2014) and Guadalupe (2007) who show that returns to skills increase after a deregulation and from Bassanini and Duval (2006) who show that firms invest more in training their employees. Whether deregulation increases or decreases wages of incumbent workers is therefore an empirical question that Chapter 4 seeks to answer.

1.2.2 Education as Insurance Against Change

Education teaches adaptability and acts as an insurance against changing conditions (*e.g.,* Nelson and Phelps, 1966). Since skill begets skill (Heckman, 2000), possessing a solid basis of education enables individuals to acquire new skills in the future, when new technologies, occupations or industries require them. Much attention in the literature has been paid to general versus skill-specific education, focussing on university versus vocational education, arguing that the former is preferable because it teaches more general and therefore transferable skills. Krueger and Kumar (2004) show that

[4] See, *e.g.,* Djankov (2009); Gittleman, Klee and Kleiner (2018); Kleiner and Krueger (2013); Weeden (2002).

[5] See, for example, Arai and Heyman (2009); Card, Devicienti and Maida (2014); Christofides and Oswald (1992); Blanchflower, Oswald and Sanfey (1996); Guertzgen (2009); Hildreth and Oswald (1997); Rusinek and Rycx (2013).

on a country level, economies that favour vocational education grow slower than countries that focus on general education, due to slower adaptations of new technologies, in particular when the pace of technological advancement increases. The authors offer this as potential explanation for differential growth rates between the US (more general education) and European countries (more vocational focus), especially since the rate of technological advancement picked up in the 1980s. At the individual level, Hanushek et al. (2017*a*) and Hampf and Woessmann (2017) show that vocational education eases entry into the labour market for young individuals but increases the risk of unemployment in later life and also reduces lifetime income. The reason is that while vocational education provides a more seamless transition from the apprenticeship into regular employment, it does not impart enough adaptive skills in case of unemployment later in life. With general education and transferable skills, the risk of unemployment is reduced because individuals are better able to adapt to new occupations and new tasks. With technological advancement that favours skilled labour (Acemoglu and Autor, 2011; Autor, Levy and Murnane, 2003), and increased occupational complexity (Spitz-Oener, 2006), this trade-off becomes particularly relevant.

One can go further in the distinction between general and specific skills, namely at the level of occupations. Lazear (2009) provides a useful framework for occupational specificity, called skill-weights approach, which assumes that occupations use different skills with different respective weights, so-called skill bundles. Skill bundles of occupations have different distances to the skill bundle of the labour market on average. The further away a skill bundle is from the average of the labour market, the more specific that occupation is and the more costly it is for a person with such skills to change occupations. Conversely, skill bundles that are similar to the labour market on average, enable individuals to switch occupations at lower costs. Geel, Mure and Backes-Gellner (2011) operationalise the skill-weights approach for German occupations using skills from a German employment survey and find that the more specific an occupation, the higher apprentice training costs for the firm and the lower occupational mobility. Eggenberger, Rinawi and Backes-Gellner (2018) find a clear trade-off between higher wages in more specific occupations but lower occupational mobility and therefore higher risk of unemployment.

Chapter 5 focusses on an important German education institution, namely the vocational education training (VET) system, sometimes called the dual system. It is dual in that there is cooperation between companies and publicly funded vocational schools. Individuals in VET spend part of their time working as an apprentice at a company and the other part at a vocational school. The system plays a large role in post-secondary

education in Germany; these days about 40 percent of a cohort enter vocational education (Statistisches Bundesamt, 2018*b*). The system is often praised as the reason for low youth unemployment in Germany, because it eases the transition from education into the labour market.[6] There are over 320 different occupations which require vocational education, which range from manual and technical to service, merchants or public service related occupations. The analysis in Chapter 5 looks at the skill-specificity of the occupations that individuals choose upon entering vocational education training. In particular, the focus is on vocational occupation choices of individuals who grew up in regions that are exposed to structural change.

Structural change is the slow transition of a labour force from one sector to another, such as the slow change from a predominantly manufacturing to service-based economy, which started in Germany like in most Western countries in the 1970s. It can be said to be driven by both automation (*e.g.,* Levy and Murnane, 1992) and trade (*e.g.,* Dauth and Suedekum, 2016), in that routine tasks become automated and goods get produced where it is cheapest to do so. The analysis in Chapter 5 deals with the part of structural change, which is induced by trade, in particular import competition. The central idea is that trade with China following its accession to the WTO and Eastern Europe after the fall of the iron curtain, was an exogenous shock in manufacturing imports, which affects different regions in Germany differentially, due to varying industry structures. Studies looking at the impact of this trade exposure for Germany actually find, that Germany has profited due to increased export opportunities that retained employment in manufacturing. Nevertheless, certain regions also lost large shares of manufacturing employment because of their specialisation in industries in which China amd Eastern Europe happened to become more competitive than Germany (Dauth, Findeisen and Suedekum, 2014).

The question is whether individuals who grew up in regions with trade-induced structural change, shelter themselves in terms of the vocational occupations they enter. General skills act as an insurance to individuals for at least three reasons. First, occupations which impart general skills tend to be in more modern and service-oriented occupations and therefore not so much subject to these transitory forces. Secondly, these occupations are less likely to be replaced by trade and automation because they require human interaction or case-specific actions. Lastly, if points one and two do not hold, general skills enable the individual to adapt and apply the skills in a new occupation.

[6] 68 percent of apprentices were offered a full employment contract at their training firm upon completion of VET in 2016 (BIBB, 2016).

1.3 Empirical Methods for Causal Inference

Economists are interested in making causal claims to the relationships we uncover in order to make policy recommendations. The gold standard of causal inference is to run an experiment, in which the experimental subjects are randomly assigned to treatment and control groups. Random assignment ensures that *on average*, individuals in both groups are the same in terms of observable as well as unobservable characteristics, and that therefore any difference in outcome must stem causally from assignment to the treatment (*e.g.,* Angrist and Pischke, 2009; Schlotter, Schwerdt and Woessmann, 2011). In absence of such experimental designs with random assignment, a simple regression of an outcome on a treatment will likely introduce a bias and therefore not give the causal effect of the treatment. Take for example earnings as outcome and a university degree as treatment. If we were to regress earnings on whether an individual holds a university degree, we would overestimate the causal effect of the university degree for a couple of reasons. First, selection into tertiary education is not random and the individual who enters it may have done similarly well on the labour market in absence of a university degree. The problem here is due to omitted variables that we cannot observe and which make an individual both choose tertiary education and do well on the labour market, such as factors of ability, perseverance, and motivation. The second source of bias is reversed causality: the individual who went to university may have gone because of his or her high (family) earnings, not the other way around. In the case where a randomized experiment is not possible, the researcher must rely on "an identification strategy", which is the manner in which observational data is used to approximate a real experiment (Angrist and Pischke, 2009).

Chapters 2 and 3 and Chapters 4 and 5 can be separated along another dimension than that of individual versus external determinants of labour market outcomes, namely being experimental versus observational studies.[7] In Chapters 2 and 3, the empirical analyses are based on experiments which use surveys as a basis for the experimental design. In Chapter 2, an online experiment is administered among a representative sample of human resource managers. The participants are asked to imagine there were a vacancy in their firm and to consider the two CVs of fictitious candidates that appear side-by-side. They have to choose one of these two CVs, to "invite them to an interview at their firm" (hypothetically speaking). On the CVs, we independently randomize skill signals, such as grade-point-averages, language skills or social volunteering. This allows to obtain the causal effect of skill signals on the probability of being

[7] However, the two dimensions are certainly related. One can credibly "manipulate" individual characteristics in an experiment, in particular when another person is supposed to believe or judge them. On the other hand one may need a natural experiment for exogenous shifts in external factors.

invited for an interview. It would be very difficult to obtain such causal effects from an observational study, because different skills are usually highly correlated with each other. Arguably, many types of skill investments do not only increase one particular type of skill, but affect the development of several dimensions of skills. In addition, the acquisition of actual skills and skill signals may depend on other determinants of employability such as innate characteristics.[8]

In Chapter 3, we design a survey experiment in which we ask a large sample of university students about their attitudes towards refugees. The challenge here is that we need random exogenous variation in our participants' beliefs about how educated refugees are. The exogenous variation in beliefs stem from random provision of information, which shift individuals' beliefs, a so-called information treatment[9]. Our participants are randomly assigned into one of three groups. The control group does not receive any information on the education level of refugees. Respondents in the *High Skilled* treatment are informed about a study that finds that refugees are rather well-educated. In the *Low Skilled* treatment, we induce the opposite beliefs by informing participants about a different study that finds that refugees are rather low-educated. This survey design was feasible because of the existing uncertainty and contradictory media coverage on the level of education of the incoming refugees between 2014 and 2016. The information treatments shift our participants' beliefs about the education level of refugees in the expected directions. We exploit this shift in beliefs to analyse its causal effect on attitudes towards refugees.

Chapters 4 and 5 use observational data and identification strategies to mimic the random assignment of an experiment. Chapter 4 exploits a policy change as a natural experiment and uses a difference-in-difference estimator. There are two groups of craft occupations in that chapter, one of which gets deregulated (*i.e.* "treated" group) and the other remains as it was (*i.e.* "comparison" group). Because these two groups are not assigned to treatment and comparison randomly, we cannot simply take their difference in outcomes after the reform, because they may have been different even in the absence of the reform. The crux is to observe the two groups both before and after the reform. By taking a double-difference, *i.e.,* the difference in average outcome in the treatment group before and after treatment minus the difference in average outcome in the comparison group before and after treatment, we obtain the causal effect of the reform which is rid of both the difference between the two groups, as well as any difference which occurred to both groups over time. For this estimate to produce

[8] See Rich (2014), Neumark (2016), and Bertrand and Duflo (2016) for overviews of CV studies.

[9] See Cruces, Perez-Truglia and Tetaz (2013); Elias, Lacetera and Macis (2015); Kuziemko et al. (2015); Wiswall and Zafar (2015); Lergetporer et al. (2016); Bursztyn (2016) and **?** for further examples and overviews of survey experiments using information treatments.

the true effect, it must hold that the two groups would have developed similarly, *i.e.,* on "parallel trends" in the absence of the reform (Angrist and Pischke, 2009). Luckily in Chapter 4, we have high-quality social security data as well as microcensus data, which allows to observe workers in many years both before and after the reform, in order to ensure that the parallel trends assumption holds. We even take a further step to ensure comparability of our treatment and comparison groups and use entropy balancing, a non-parametric matching procedure which allows us to reweight observations such that their pre-reform characteristics are identical on average and in their variances (Hainmueller, 2012).

In Chapter 5 finally, I use a combination of different strategies to obtain an exogenous source of regional structural change. To construct the extent of local import exposure per worker, a so-called shift-share measure is constructed (Bartik, 1991). In a first step, I look at yearly industry imports from China and Eastern Europe in every manufacturing industry. This then gets apportioned to the region proportionally to initial employment in the industry and region, and then summed over all industries. The measure therefore gives predicted potential local import exposure per worker, given national import volumes (see also *e.g.,* Dauth, Findeisen and Suedekum, 2014). This is useful (i) because I do not observe the actual local per worker import exposure and (ii) using national industry imports and apportioning it to initial industry structures gets rid of region-specific adjustments which are endogenous to the trade shock. In a second step, I instrument imports to Germany with imports to other high-income countries. "Instrumental variables" are used when an explanatory variable is endogenous, *i.e.* is correlated with the error term. The main idea is to find another variable which is highly correlated with the endogenous regressor, but is unrelated with the outcome and the error term, meaning that the only way in which it affects the outcome is through the channel of the endogenous regressor (Wooldridge, 2008; Angrist and Pischke, 2009). One then uses the instrument to predict the endogenous variable and uses the predicted values instead of the endogenous variable, in a so-called two-stage least squares instrumental variable regression. In the context of Chapter 5, this means that trade in other high-income countries with China and Eastern Europe is highly correlated with Germany's trade with China and Eastern Europe and can be used to predict it. However, imports from China and Eastern Europe to other high income countries (such as New Zealand for example), are entirely unrelated to occupational choices and labour market outcomes of adolescents in German local labour markets. This allows to uncover the causal effect of import exposure on vocational occupation choices.

1.4 Chapter Overview

While previous sections introduced selected aspects of the four chapters containing my research, this section provides concise but comprehensive summaries of the four chapters.

Chapter 2 provides evidence on the importance of skill signals in the job application process of labour market entrants. The chapter is joint work with Marc Piopiunik, Guido Schwerdt and Ludger Woessmann. As skills of labour market entrants are usually not directly observed by employers, individuals acquire skill signals which they send out to potential future employers, most commonly on their CV. To study which signals are valued by employers, we administer an experiment among a large representative sample of German human-resource (HR) managers. The HR managers are asked to choose between two fictitious CVs, which appear on their computer screens side-by-side, and decide which candidate they would rather invite to an interview, if their firm had an opening. To obtain causal effects of different skill signals, we simultaneously and independently randomize a broad range of skill signals on the CV pairs. Because the skill signals that are effectively relevant might differ substantially between secondary-school and college graduates due to differing relevance, expectedness, and credibility of various skill signals, part of the HR managers receive applications from secondary-school graduates for an apprentice position and the other part receive applications from college graduates in business administration for a business trainee position.

We find that signals in all three studied domains – cognitive skills, social skills, and maturity – have a significant effect on being invited for a job interview. Consistent with the relevance, expectedness, and credibility of different signals, the specific signal that is effective in each domain differs between apprenticeship applicants and college graduates. GPAs prove important for both genders, with a stronger effect for college graduates than for secondary-school graduates. IT and language skills are particularly relevant for females. Social skills are highly relevant for both genders and particularly important for secondary-school graduates entering the labour market at a young age. Maturity is particularly relevant for males, especially for secondary-school graduates. Moreover, we test heterogeneities by HR manager characteristics. Notably, older HR managers value school grades less and other signals more, whereas HR managers in larger firms value college grades more. To validate our experimental set-up, we find that HR managers' choices in the experiment are consistent with self-reported hiring priorities.

Chapter 3 analyses the impact of refugees' education level on natives acceptance of refugees. The chapter is joint work with Philipp Lergetporer and Marc Piopiunik. In recent years, Europe has experienced an unprecedented influx of refugees. While natives' attitudes towards refugees are decisive for the political feasibility of asylum policies, little is known about how these attitudes are shaped by refugees' characteristics. We study whether attitudes towards refugees are affected by beliefs about the education level of refugees. To do so, we implement online survey experiments with more than 5,000 students at universities in Germany. To estimate causal effects of beliefs about education on attitudes, we exogenously shift respondents' beliefs by randomly providing information on the education level of refugees. The uncertainty regarding refugees' education level at the time, allows us to provide opposing information on the education level of refugees in Germany to our three experimental groups. The control group does not receive any information on the education level of refugees. Respondents in the *High Skilled* treatment are informed about a study that finds that refugees are rather well-educated (UNHCR, 2015). In the *Low Skilled* treatment, we induce the opposite beliefs by informing participants about a different study that finds that refugees are rather low-educated (Woessmann, 2016).

We test two economic theories which make opposing predictions about how the education level of refugees should affect natives' attitudes towards them. The *labour market competition model* predicts that natives will be most opposed to immigrants whose skills are similar to their own since these immigrants might be competitors on the labour market. This model therefore predicts that university students, the participants in our surveys, are more opposed to refugees if they believe refugees to be well-educated. The *fiscal burden model*, on the other hand, predicts that natives in general are more opposed to low-skilled immigrants because they impose larger fiscal burdens on the public budget than high-skilled immigrants. In contrast to the labour market competition model, the fiscal burden model predicts that university students are more opposed to refugees if they believe refugees to be *low*-educated (see Hainmueller and Hiscox, 2010).

We find that our information treatments strongly shift respondents' beliefs about the education level of refugees in the expected directions. Using the exogenous shift in respondents' beliefs about refugees' education as the first stage in an instrumental-variable approach, we find that beliefs about refugees' education level affect natives' concerns about labour market competition. This finding is in line with the predictions of the labour market competition model. In contrast, we find no effects on fiscal burden concerns or other concerns such as increasing crime levels. We also do not find that education beliefs affect general attitudes towards refugees, because economic

aspects are rather unimportant for forming general attitudes towards refugees. In a follow-up survey, we test that our information treatment effects are not driven by experimenter demand effects nor priming effects, and show that they are persistent across time, and are not subject to social desirability bias.

Chapter 4 looks at the effects of a labour market reform in the crafts sector. The chapter is joint work with Jens Ruhose and Philipp Lergetporer. We analyse the impact of a deregulation reform in the German crafts sector on labour market outcomes of incumbent workers. The reform abolished the requirement to hold a costly occupational license to open a new business in some craft occupation but not in others. This led to large increases in entrepreneurial activity, namely it tripled the number of businesses in the deregulated occupations within ten years. Using longitudinal social security data, we implement a matched difference-in-differences design with entropy balancing to account for observable characteristics and unobserved individual heterogeneity.

We find that the deregulation reform had negative effects on earnings of incumbent workers and that firms also adjust to competitive pressure via the employment margin. We find that the daily gross earnings of incumbent workers in deregulated occupations grew significantly less than those of workers in regulated occupations after the reform. Over the period from 2004 to 2014, workers in deregulated occupations experienced a negative average effect on their earnings of about 2.3 percent relative to workers in regulated occupations. Year-specific estimates show that the treatment effect becomes gradually larger over time to -4.3 percent in 2014. We also find that unemployment among incumbent workers increased by 0.7 percentage points more in the deregulated occupations than in the regulated occupations. Using cross-sectional census data allows us to provide evidence on the income effects of self-employed, showing no effects on the income position of self-employed individuals in deregulated compared to regulated occupations. Further analysis suggests that the reform had negative effects on overall employment and average wages of all employees in deregulated occupations. We conclude that while the reform created slight competitive pressure on incumbent firms resulting in erosion of some monopoly mark-ups and therefore decreasing earnings and employment of incumbent workers, the newly created firms were too small and insubstantial to spark positive effects on total employment, investments or innovative activity.

Chapter 5 investigates the impact of growing up in a region with structural change on individual vocational occupation choices. Structural change describes the transition of the workforce from manufacturing to service-based occupations. Local labour market exposure to import competition has been shown to speed up this process. This

chapter looks at the impact of growing up in a region exposed to import competition on young individual' choice of occupation in vocational education training and whether the occupation teaches them specific or general skills. General skills protect from future unemployment because they are adaptable and transferable. I construct a measure of occupational skill-specificity to show that manufacturing and crafts occupations are skill-specific and have skill bundles further away from the average labour market, while service and merchant occupations are more general with skill-weights closer to the average of the labour market.

I exploit the exogenous rise in trade volumes with both China following its accession to the WTO and Eastern Europe after the fall of the iron curtain as an exogenous supply shock of manufacturing good imports to Germany. Exogenous regional variation in import exposure stems from varying initial local industry structures with respect to employment shares in the different manufacturing industries. National industry trade volumes are apportioned to the local labour markets by the initial local industry employment structure in the sense of a shift-share analysis. To isolate the supply-driven component of imports from China and Eastern Europe and to shut down pull or push factors stemming from labour demand, I instrument imports (and exports) from China and Eastern Europe to Germany with imports (and exports) from China and Eastern Europe to other high-income countries.

Using longitudinal individual-level administrative social security data, the results show that individuals growing up in regions with higher import exposure surprisingly choose more skill-specific occupation groups in manufacturing and crafts, more import-intensive manufacturing industries in particular, and less general occupations in services and commerce (as merchants). Secondly, I find that increased import exposure makes adolescents less likely to enter occupations with high computer use, and more likely to enter manual occupations. Lastly, I find that individuals exposed to import competition in their adolescence who enter vocational education, are adversely affected on the labour market in terms of earnings in later life.

While the negative labour market outcomes cannot be directly causally linked to the vocational occupation choices due to issues of biased self-selection, suggestive evidence shows that general skill occupation groups in service and as merchants, shelter individuals from the adverse effects of import competition, while the negative effect seems entirely driven by entering manufacturing occupations. I show that the effects are not biased by endogenous sub-sample sorting in the sense of differential sorting into different educational tracks and are not purely labour demand driven. Women, as opposed to men, are more likely to enter service and merchant occupations when

exposed to local import competition, but are nevertheless adversely affected by import competition in terms of later labour market outcomes.

1.5 Policy Implications

While the four chapters all deal with different aspects and determinants of labour market outcomes, the importance of education is the core message that unites them.

Chapter 2 provides useful insights not only to policy makers but also to young labour market entrants, teachers, parents and employers alike, on which skill signals are important to human resource managers in Germany. The findings show that cognitive skills, social skills and maturity are important and that the particularly relevant skill in these domains are different for apprenticeship applicants and college graduates. The policy implication to be drawn from this chapter is that various skills pay off in the labour market, and that while formal education is still most important, schools and parents alike should encourage young individuals to invest in skills also outside school, such as IT skills or social volunteering and seek prior labour market experience in the form of internships.

Chapter 3 first demonstrates that humanitarian aspects are very important for shaping natives' attitudes towards refugees, which stipulates that the decision of granting persecuted asylum seekers temporary refugee status is independent of their characteristics and economic considerations. Secondly, the chapter contributes to understanding the underlying determinants that drive public attitudes, which may strongly affect the political feasibility of asylum policy. While the effects of the large refugee inflow on the labour market and on the government budget remain to be seen, our findings suggest that developments in these areas will only have limited impact on public attitudes, at least among high-skilled natives.

Chapter 4 shows the effect of a reform that removed an educational requirement to enter self-employment in the German crafts-sector, thereby lifting a substantial barrier to entry. We find that the reform led to decreased earnings and increased unemployment for incumbent workers. This implies that the increased competitive pressure did reduce some mark-ups and monopoly rent sharing. However, the reform also did not trigger employment and earnings growth for other workers in the affected occupations, as theory on competition would predict. The most likely reason for this finding is that the newly established firms remained one-man businesses with low ability to compete against incumbent firms that also did not produce new employment. This may be due to the fact that holding a *Master Crafts Certificate* still acts as a consider-

able signal of quality to consumers, and that business owners without them simply do not compete on the same market.

While decreasing entry barriers should generally foster competition, entrepreneurial activity, innovation, and employment growth, policy makers should be aware that there may be unintended consequences when the newly created businesses do not compete with incumbent firms. If this is the case, it is likely that there are further (more important) barriers in place that hold back new firms from becoming stable competitors. Thus, each deregulation reform should collect and carefully evaluate possible industrial and occupational entry barriers before the reform is implemented. While it is difficult to identify all relevant entry barriers *ex ante*, the success of each reform has to be constantly monitored and evaluated. The results also point to the importance of investigating not only short-term but also long-term effects of policies, as the reduction and redistribution of rents through deregulation may induce adjustments over longer periods.

My findings in Chapter 5 finally show that individuals tend not to choose occupations that shelter them from forces of globalisation and trade. Individuals exposed to import competition choose more skill-specific occupations than elsewhere and also enter more import impacted industries. This implies that they will likely be exposed to further import competition in the future, and any immediate effect of trade shocks on earnings may be underestimated. The results suggest that these occupational choices lead to adverse outcomes on the labour market in particular in terms of later earnings. In terms of policy implications, these findings clearly point to the importance of providing better information to young individuals choosing their vocational occupations. The results also imply that individuals predominantly enter apprenticeships in occupations which are strongly present in their local labour market. While this is partly driven by labour demand, there is clearly a place for campaigns informing adolescents on different occupations and their advantages, in particular with regards to the general and transferable skills they teach. Moreover, the paper points to the importance of regional redistribution, as regions go through different stages of development and may be adversely affected by factors outside their immediate control. Once prosperous regions may turn to losers of globalisation, and once struggling regions may rise as stars (Dauth and Suedekum, 2016), which is why all regions have an interest in participating in redistribution policies.

To conclude this introduction, the policy implication from this thesis as a whole, is that policy makers cannot stress the importance of education and skills enough, because it is what equips the labour force to navigate the labour market and its constant

changes and challenges. The thesis demonstrates that this is true for new labour market entrants, refugees and professionals alike.

2 Skills, Signals, and Employability: An Experimental Investigation*

2.1 Introduction

Cognitive and non-cognitive skills predict individuals' labour-market performance (*e.g.,* Heckman, Stixrud and Urzua, 2006). But employers cannot directly observe the skills of job applicants. Therefore, individuals make costly investments to signal their skills to potential employers. So far, however, it is not well understood to what extent specific skill signals – characteristics in which workers have invested – affect hiring decisions. When making hiring decisions, employers simultaneously consider many different and potentially highly correlated signals, and not all of these signals are typically observed by researchers. As a consequence, a more nuanced empirical investigation of the relative importance of different skill signals for employability is challenging.

In this chapter, we investigate how several skill signals affect labour-market entry in an experimental setting. We conduct a randomized survey experiment among 579 human-resource (HR) managers, exploiting our access to the ifo Personnel Manager Survey, a regular survey of HR managers representative of German firms. The experimental design gives us full control over the information set available to firms. We simultaneously randomize several skill signals contained in applicants' CVs. This set-up allows us to base our identification strategy on independent and exogenous variation in different signals within three broad skill domains: cognitive skills, social skills, and maturity – a trait potentially of particular relevance at labour-market entry. Due to differing relevance, expectedness, and credibility of various skill signals, the specific signals that are effectively relevant might differ with the educational degree of applicants. Therefore, about half of the HR managers receive applications from secondary-

* This chapter was coauthored by Marc Piopiunik, University of Munich and ifo Institute, Guido Schwerdt, University of Konstanz and Ludger Wößmann, University of Munich and ifo Institute. Financial support by the German Federal Ministry of Education and Research for the project "Exit exams as a governance instrument in the school system: The importance of school-leaving grades for the hiring decisions of firms" within the BMBF research priority "Educational Governance" (SteBis) is gratefully acknowledged.

school graduates for an apprentice position. The other half receive applications from college graduates in business administration for a business trainee position.[1]

The choice experiments confront HR managers with resumes of two fictitious job applicants, asking them to indicate the applicant whom they would rather invite for a job interview in their firm. The only information on the applicants available to HR managers are the elements of the resumes. Specifically, cognitive skills are signalled by grade-point averages (GPAs) in school for secondary-school graduates and in college for college graduates, as well as by IT skills, fluency in English, and a second foreign language. Social skills are signalled by social volunteering and team sports (as opposed to single sports). Maturity is signalled by being older within the same school cohort and length of internship.[2] We carefully selected these CV elements in a pre-study, conducting qualitative interviews with HR managers to identify the pieces of information that are typically included in resumes of real applicants in Germany. In our survey, we complement the choice experiments with a questionnaire of the HR managers.

We find that signals in all three domains –cognitive skills, social skills, and maturity– significantly affect the probability of being invited for a job interview. GPAs prove important for both genders, with a stronger effect on the probability of being invited for a job interview for college graduates than for secondary-school graduates. IT and language skills are particularly relevant for females. Social skills are highly relevant for both genders and particularly important for secondary-school graduates entering the labour market at a young age. Maturity is particularly relevant for males, especially for secondary-school graduates.

These heterogeneities by labour-market entry age and gender are consistent with varying relevance, expectedness, and credibility of the different skill signals in different contexts. Gender differences in the effects of IT skills, language skills, and maturity are generally in line with gender stereotyping. Social skills are most effectively signalled by social volunteering among secondary-school graduates but by engaging in team sports among college graduates, possibly reflecting limited credibility of volun-

[1] To avoid unrealistic situations, the resumes are adjusted to the firm of the HR manager. Most importantly, using information on the educational composition of the firms' workforce that we elicited in a pre-survey, secondary-school graduates applying for an apprentice position are presented only to HR managers in firms that currently offer apprenticeship positions. Similarly, applications of college graduates are only shown to HR managers whose firms employ college graduates.

[2] The categorization of specific skill signals into three broad skill domains is meant as a structuring device rather than a conceptually sharp distinction. Presumably, signals that are included in real-world CVs – which are the subject of this chapter – always reflect components of different domains but tend to be perceived as having a focus in one of the domains (see Section 2.2.3 for details)

teering activities of older individuals who may behave strategically. In addition, skill signals that are easily verifiable in real hiring situations such as age, internships, and GPAs (available on transcripts usually included in German applications) tend to have higher returns than skills that are costlier to verify, such as language skills and social volunteering.

Exploiting our HR manager questionnaire, we also find heterogeneities with respect to HR managers' personal characteristics. Most importantly, managing directors and older HR managers put less weight on the school GPAs of apprenticeship applicants, but instead put more weight on IT skills, social volunteering, and experience through internships. HR managers in large firms value the college GPAs of college graduates more, possibly due to a more standardized procedure of applicant selection.

Our questionnaire also asks HR managers to indicate the importance they attach to various resume attributes of actual job applicants in their firm. Corroborating our experimental set-up, we find that HR managers' self-reported hiring priorities tend to be in line with their decisions between the fictitious candidates' resumes in the choice experiments.

Overall, our results indicate that a broad range of skill signals indeed causally affect employment chances at labour-market entry. Employers value skill signals in several different domains. While situated in a specifically designed experimental setting, the results add an important dimension to our understanding of how labour markets process and use information on skills. When observational data indicate that signals such as high-school grades are associated with labour-market outcomes, it remains unclear whether employers really value grades or whether the association captures other productivity aspects that happen to be correlated with grades, whereas grades are potentially never conscientiously observed by employers. Even in the setting of a convincing natural experiment, it is hard to imagine a research design that can separately identify independent exogenous variation in different skill dimensions such as GPAs and social engagement. Exploiting randomized controlled variation in several skill signals, our experimental results indicate that employers do indeed care about signals such as high-school GPAs, social volunteering, and internships, adding to the existing knowledge on the importance of skills and signals for labour-market outcomes. The results also inform about which signals may be particularly relevant for whom. The observed effect heterogeneities by high-school vs. college background, gender of applicants, and traits of HR decision-makers suggest that different signals are regarded as relevant, expected, and credible in different situations.

Our results contribute to the large literature that has established that labour-market outcomes are associated with different types of skills. Skills may be reflected through educational degrees (*e.g.,* Heckman, Stixrud and Urzua, 2006) or more directly observed in terms of cognitive skills (*e.g.,* Hanushek and Woessmann, 2008). Social skills seem to have become increasingly important on modern labour markets that value team production (Deming, 2017). Likewise, signals of maturity, including personality traits such as conscientiousness, commitment, and perseverance, are strongly related to labour-market outcomes (Almlund et al., 2011). Despite the wide evidence that these different types of skills are associated with labour-market success, it is unclear whether *signals* of these skills that can be observed at the application stage have a causal impact on the employability at career start.

Our research design also adds to the literature on CV studies. This literature almost exclusively focuses on discrimination in the labour market, investigating effects of *innate* characteristics such as gender and race (*e.g.,* Neumark, 2016, see Section 2.2.2 for greater detail). By contrast, we are interested in the returns to skill investments that are used as signals of productivity on resumes, thus focusing on the effects of *intentionally acquired* characteristics.[3] As a recent exception, Deming et al. (2016) investigate the employability impact of for-profit online college degrees. Our study, in turn, investigates the employability impact of a broad range of acquired signals in different skill domains.[4] A related important difference between the existing CV study literature and our study is that statistical discrimination based on innate characteristics is typically considered unfair in the sense that it is based on circumstances that are beyond a person's control Roemer (1998).[5] By contrast, differential treatment of job applicants is generally considered fair to the extent that it is based on differences in signals that reflect differences in ability or effort. According to theories of job market signalling and screening (*e.g.,* Riley, 2001), these skill signals have to be acquired by individuals since actual skills are not directly observed by employers at the application stage.[6]

In contrast to most existing CV studies, HR managers in our study are fully aware that they are dealing with fictitious job applicants. A primary motivation for not being

[3] To abstract from the aspects studied in the discrimination literature, we keep gender fixed within CV pairs and use only standard German names.

[4] A general limitation of CV studies that our study has in common with this literature is that only the first stage of the application process – the interview invitation – is observed, but not actual job offers or wages.

[5] According to the non-discrimination principle, individuals who compete for positions in society should be judged only on attributes that are relevant to the performance of the duties of the respective position. Therefore, attributes such as gender or race should not be considered.

[6] Throughout the chapter, we refer to signalling simply as the revelation of otherwise unobserved information, without any claim about whether the signalling process is productive or unproductive from a welfare perspective.

transparent about the true nature of applications in conventional CV studies is concern about social desirability bias in HR managers' behaviour. We are less concerned about social desirability bias in our setting because we do not study discrimination against certain groups of applicants such as women, blacks, or foreigners. Apart from not deceiving participants, the transparency of our research design has several advantages. First, existing discrimination studies put substantial costs on the hiring systems of firms. Second, the flooding of online portals with fictitious CVs might lead to artificial results because initial hiring decisions are increasingly computerized, whereas final job offers are made by HR managers. Instead, we make use of evaluations by those HR managers who actually make the hiring decisions in their firms. Third, in contrast to studies that contact job portals, our survey-based approach enables us to collect information on decision-makers' characteristics and hiring preferences.[7] This allows both investigating effect heterogeneity by HR manager characteristics and assessing whether self-reported hiring priorities are consistent with decisions in the experimental set-up. Fourth, in our setting we have complete information on *all* applicants that an HR manager faces. In contrast, researchers sending fictitious resumes to real job openings do not have information on the characteristics of the real job applicants for the same position. The distribution of relevant resume characteristics of the other job applicants, however, likely affects job interview decisions.[8]

In what follows, Section 2.2 provides background on the role of skills and their signals on labour markets and discusses related literature. Section 2.3 describes the experimental design of our CV study. Section 2.4 reports the baseline results of the choice experiments. Section 2.5 investigates heterogeneous effects for different HR managers. Section 2.6 concludes.

2.2 Conceptual Framework on Skills, Signals, and Labour Markets, with Relation to the Literature

Based on the existing literature, we first discuss how different skills may affect labour-market outcomes. We examine how these effects depend on the extent to which skills are observed by employers and how the importance of skill signals may vary with the

[7] We also have access to rich information on the firms, including their workforce's educational composition.

[8] For example, assuming that employers value maths skills, the estimated returns to maths skills will be lower if most real applicants possess high maths skills (since this lowers the probability that a fictitious applicant with high maths skills gets invited to a job interview), compared to a situation where hardly any real applicant possesses high maths skills. Since we have complete information on the distribution of characteristics of all applicants, the magnitudes of the estimated skill signals are directly comparable with each other and not influenced by resume characteristics of unknown job applicants.

quality of the signal and the context. We then discuss how a randomized CV study can be used to elicit exogenous variation in different skill signals and how the question of costly skill signals differs from most of the existing CV study literature. Finally, we specify our main research question and several hypotheses on effect heterogeneities.

2.2.1 Skills, Signals, and Labour-Market Outcomes

The employability of individuals depends on their marketable skills in which they make a variety of investments. Formal education is one of these investments. Numerous studies show that more schooling and higher educational degrees lead to more success on the labour market (*e.g.,* Card, 1999; Heckman, Lochner and Todd, 2006). However, while the reduced-form effects of educational investments on labour-market success are well documented, it is less well understood how these effects arise. Several aspects complicate a more nuanced investigation of the underlying mechanisms.Marketable skills are not unidimensional. The labour-market impacts of different domains of skills are the subject of a growing literature. Several studies investigate the importance of cognitive skills such as achievement on standardized tests on labour-market outcomes (*e.g.,* Osborne, Gintis and Bowles, 2001; Hanushek and Woessmann, 2008). Early studies[9] as well as more recent investigations (Chetty et al., 2011; Hanushek et al., 2015, 2017*a*) document that cognitive skills are positively related to employment and earnings.

There is also abundant evidence highlighting the importance of non-cognitive skills (*e.g.,* Heckman, Stixrud and Urzua, 2006; Borghans et al., 2008; Almlund et al., 2011). While non-cognitive skills are often used as a vague term for a residual set of skills (many of which in fact contain a strong cognitive component), an increasing literature investigates different skills in the non-cognitive domain that are empirically clearly specified. One important dimension of non-cognitive skills is social skills. Deming (2017) argues that the importance of social skills on the labour market is growing, with the fastest-growing occupations requiring a substantial amount of interpersonal interactions. His results support a model of team production where workers trade tasks to exploit their comparative advantage. In this setting, social skills reduce coordination costs, allowing workers to specialize and trade more efficiently. Thus, social skills such as the willingness to cooperate make workers more productive in team production.[10] This may be particularly important in occupations with a high complementarity between cognitive and social skills (Weinberger, 2014). Recent evidence

[9] *E.g.*, Bishop (1989); Murnane, Willett and Levy (1995); Neal and Johnson (1996) and Mulligan (1999).
[10] In the context of a lab experiment, Englmaier and Gebhardt (2016) find direct evidence of a link between productivity and cooperative behaviour measured by contributions in a public goods game.

suggests that social volunteering may be used to credibly signal willingness to cooperate (Heinz and Schumacher, 2017; Baert and Vujić, 2018), but social volunteering may also be correlated with other skills valued by employers.

Another dimension of non-cognitive skills that may be particularly relevant at the labour-market entrance stage are personality traits that include maturity, conscientiousness, perseverance, and curiosity (*e.g.,* Almlund et al., 2011). For example, Heckman, Humphries and Mader (2011) find that even conditional on cognitive skills, high-school graduates outperform GED recipients in terms of labour-market outcomes and show that this difference is driven by personality. This is in line with other findings on a positive link between personality traits and labour-market outcomes.[11]

However, any observational study on the labour-market impact of skills faces the challenge that researchers do not know whether skills are actually observed by employers. In contrast to innate characteristics such as gender and race, cognitive skills, social skills, and maturity are typically not directly observable. Thus, employability might be primarily a function of signals of these skills rather than the skills themselves. Educational credentials, grades, and extracurricular activities are just a few prominent examples of such skill signals.

A large literature explores the role of costly skill signals in determining labour-market outcomes.[12] Several studies investigate the signalling value of educational credentials (*e.g.,* Tyler, Murnane and Willett, 2000; Clark and Martorell, 2014). The importance of skill signals may, however, crucially depend on other factors. For example, skill signals may be more important for labour-market entrants than for workers with substantial experience. Such a pattern would be consistent with the evidence on employer learning about the true skills of workers over time (Altonji and Pierret, 2001). Relatedly, ability appears to be observed well for college graduates, whereas it is revealed to the labour market more gradually for high-school graduates (Arcidiacono, Bayer and Hizmo, 2010).

More generally, the importance of a specific skill signal may depend on the perceived quality of the signal. For example, in Colombia college reputation is correlated with graduates' earnings, but the return to reputation is reduced if a college exit exam is available as an additional skill signal (Macleod et al., 2017). In Germany, the informa-

[11] *E.g.,* Heckman, Stixrud and Urzua (2006); Mueller and Plug (2006); Heineck and Anger (2010); Lindqvist and Vestman (2011).
[12] See Spence (1973), Arrow (1973), and Stiglitz (1975) for seminal contributions and Weiss (1995) and Riley (2001) for surveys.

tion value of high-school grades depends on whether these grades were obtained in local or centralized exams (Schwerdt and Woessmann, 2017).

In addition, the value of a skill signal may also depend on stereotypes. If members of a specific group are perceived as having certain skills while non-members are not, signals for the same skills may matter more for non-members. As a consequence, education is generally a more valuable signal of productivity for blacks than for whites (Lang and Manove, 2011). Similarly, gender stereotypes during the hiring process have been frequently documented in the sociological literature (*e.g.,* Gorman, 2005). These stereotypes can arise unconsciously, as decision-makers may be influenced by gender-specific attitudes or beliefs they are not even aware of. One well-documented example of such an implicit gender stereotype is that males are considered as having higher math skills than females Lummis and Stevenson (1990).

2.2.2 CV Studies: Innate Characteristics vs. Acquired Signals

A key conceptual problem is that skills, as well as signals thereof, are typically highly correlated across domains. Arguably, many types of educational investments do not only increase one particular type of skill, but affect the development of several dimensions of skills. In addition, the acquisition of actual skills and skill signals may depend on other determinants of employability such as innate characteristics. As a consequence, it is empirically challenging to identify the isolated effect of an increase in one particular skill signal based on observational data. The key problem is that other determinants of employability are likely not observed by the econometrician, but may be observed by the employer. To credibly estimate the relative importance of different skill signals on employability, it is therefore crucial to obtain independent exogenous variation in the different skill domains.

Randomized CV studies offer a methodological solution to this identification problem. In these studies, fictitious applications with fictitious resumes are sent to numerous employers currently offering jobs. The resumes are designed to carefully match on all individual characteristics that matter for employability. The fictitious applicants are thus identical except for the characteristic whose impact the researcher is investigating. This research design has two key advantages. First, the experimentally generated variation in resume characteristics solves the identification problem. Second, the econometrician knows exactly all the signals that the employer observes for the fictitious applicants. A common drawback of this research design, however, is that only the first stage of the application process – i.e., the invitation for a job interview – is observed, but not actual job offers and wages.

Most studies using randomized resumes are motivated by research questions addressing different types of discrimination in the labour market (see Rich (2014), Bertrand and Duflo (2016) and Neumark (2018) for recent overviews). As a consequence, nearly all existing CV studies focus on the impact of innate characteristics such as race (e.g. Bertrand and Mullainathan, 2004), gender (e.g. Booth and Leigh, 2010), age (e.g. Lahey and Oxley, 2016), immigrant status (e.g. Oreopoulos, 2011)[13], phenotype (e.g. Arceo-Gomez and Campos-Vazquez, 2014), or beauty (e.g. Ruffle and Shtudiner, 2015).[14]

A differential treatment of job applicants by firms based on these innate characteristics will generally be viewed as unfair – e.g., according to Roemer's 1998 concept of equality of opportunity. In contrast, our focus is on characteristics in which individuals can invest.

2.2.3 Main Research Question and Contextual Heterogeneity: Relevance, Expectedness, and Credibility of Skill Signals

The previous discussion implies some important open research questions that we address in this chapter. Our main question of interest is whether and which acquired signals of skills in different domains have a causal effect on employability. To address this question, we simultaneously and independently randomize several skill signals in the framework of a CV study, covering three skill domains: cognitive skills, social skills, and maturity.

To obtain a more nuanced view of how skill signals function in the labour market, we also aim to investigate how the effectiveness of different types of skill signals varies in different settings. As indicated, different skills may be viewed as relevant and expected in different contexts, and different signals may be viewed as credible. We thus set out to test how the relevance, expectedness, and credibility of different skill signals affect their effectiveness in different contexts. First, given the differential observability of skills for graduates from high school and college, we study the relevance of different skill domains for graduates of lower-secondary school who apply for an apprenticeship position as opposed to college graduates. Further aspects of skill rele-

[13] While migration status is not innate, individuals cannot simply invest in this characteristic as in cognitive and non-cognitive skills.

[14] Notable exceptions of randomized CV studies that investigate the effects of including information on a particular cognitive or non-cognitive skill on the resume include Koedel and Tyhurst (2012) for maths skills, Protsch and Solga (2015) for school grades and teacher evaluations, Humburg and van der Velden (2015) for occupation-specific field of study and professional experience, Kübler and Schmid (2015) for age and additional training, andHeinz and Schumacher (2017) andBaert and Vujić (2018) for social volunteering. In a non-randomized pre-post set-up of a CV study, Falk, Lalive and Zweimüller (2005) study the effect on job interview invitations of a training course meant to raise basic computer skills.

vance may give rise to heterogeneity by gender (if employers have stereotypes about how different genders will be employed in the workplace), firms (e.g., relevance of different skills in different sectors), and types of HR managers (e.g., more or less experienced managers). The investigation of whether the importance of skill signals depends on the characteristics of HR managers is typically not feasible when sending fictitious applications to real job openings.[15]

Second, certain groups of applicants may be generally expected to be equipped with certain skills, reducing the pay-off to respective signals. For example, employers may expect college graduates, but not secondary-school graduates, to be equipped with basic foreign language and IT skills anyway. A further example of expectedness is another form of gender stereotyping: if HR managers, for example, expect boys to be affine to computers in general, signalling good IT skills may have a higher value for girls than for boys.

Third, the value of a signal may depend on its credibility. One central aspect of credibility is the extent to which a signal can be verified, which may be easier for GPAs than for social skills. Another aspect of credibility refers to the possibility of strategic behaviour of job applicants. For example, even rather unsociable people may choose to signal social volunteering if they expect such signals to be rewarded in the labour market. In such a setting, social volunteering may lose credibility as a signal of social skills and may be replaced by other signals that are less subject to strategic behaviour.

2.3 Experimental Design

To investigate the importance of skill signals for employability, we conduct an online survey experiment with randomized CVs among German HR managers.

2.3.1 Choice Experiments in an HR Manager Survey

We conduct our online survey experiment among HR managers who participate in the ifo Personnel Manager Survey.[16] The ifo Institute is an independent economic research institute that regularly administers business surveys including Germany's main business climate index. The quarterly survey of personnel managers is generally used to construct an index of the usage of different personnel management instruments

[15] In an alternative attempt to learn about heterogeneity across screeners of resumes, Lahey and Oxley (2016) use eye-tracking technology, albeit on a sample of students in the lab rather than actual HR managers.
[16] For more information on the ifo Personnel Manager Survey, see https://www.cesifo-group.de/ifoHome/facts/Survey-Results/Personalleiterbefragung.html.

and to investigate current topics of personnel policy. We conducted our survey as a special additional survey in August 2016.[17] The firms covered by the database are a representative sample of firms in Germany.

In our survey, we confront each HR manager with two choice experiments. In each choice experiment, we ask HR managers to compare resumes of two fictitious applicants (either two secondary-school graduates or two college graduates) which are presented side by side on the same screen.[18] The HR managers are asked to choose the candidate they would rather invite for an interview in their firm.[19] We force HR managers to select exactly one of the two candidates to mimic the fact that HR managers ultimately have to make choices and to increase statistical power in the empirical analysis. HR managers complete the two choice experiments sequentially and are not able to revise their decisions.

To present realistic candidates to HR managers, we elicited information on the educational structure of the firms' workforce in a pre-survey, conducted in the context of a regular ifo Personnel Manager Survey prior to our main survey. Based on the share of employees with college degrees and the presence of apprentices in the firm, we split firms in our database into two groups. HR managers in firms with a high share of college-educated employees were shown resumes of fictitious college graduates applying for a fictitious graduate trainee position.[20] The second group of HR managers, who had apprentices in their firm at the time of our survey, were shown resumes of fictitious secondary-school graduates with an intermediate school degree applying for a fictitious apprentice position.

[17] HR managers of firms in the ifo database were first contacted by mail with the request to participate in a special additional survey. The letter informed the HR managers that the ifo Institute was to carry out a scientific study that was financed by the Germany Ministry of Education and Research. They were told that they would receive an email a few days later that included a link to the online survey and that completing the survey would take about three minutes. Additionally, the letter stated that the goal of the survey was to study hiring decisions in Germany and that all answers would be kept strictly confidential.

[18] In the instructions of the choice experiment, HR managers were asked to imagine that their firm had a vacancy – either an apprenticeship position or a traineeship position for a business university graduate – and to consider the two shown fictitious applicants for that position.

[19] Additional analyses (not shown) reveal that the resume shown on the left-hand side is chosen with the same probability as the resume shown on the right-hand side. Including an indicator for the side on which a CV is shown does not affect our estimates.

[20] HR managers are assigned to this group if their establishment either (i) does not offer apprentice positions, (ii) has a share of at least 25 percent of college-educated employees, or (iii) has a share of at least 5 percent college-educated employees and the majority of apprentices have completed the most academic high-school track (*Abitur*).

To create realistic resumes, we used real resumes to set up our fictitious resumes.[21] Prior to our main survey experiment, we conducted field interviews with HR managers responsible for selecting candidates in six firms located in Munich (also drawn from the ifo Personnel Manager Survey database) to assess whether our fictitious resumes were realistic. Importantly, all interviewed HR managers stated that our skill signals included in the fictitious resumes are relevant criteria at the first stage of the hiring process. We also discussed the values that these signals typically take on in practice, e.g., the common range of values of school grades and college grades. The expert interviews helped to set up realistic resumes that are appropriate to answer our research questions and also provided feedback on the questionnaire (see Section 2.5).

2.3.2 The Resumes

Secondary-School Graduates

The resumes shown to the HR managers are one-page CVs that contain standard information that would generally be included in job applications in Germany.[22] Appendix A2.1 describes all elements of the resumes of secondary-school graduates in detail. Figure 2.1 shows an example of a resume of a secondary-school graduate and Table A2.1 lists all possible values of all CV elements.

All fictitious secondary-school graduates obtained an intermediate school degree (*Mittlere Reife*) after 10 years of schooling.[23] Because mobility is typically low among individuals with vocational education in Germany, all secondary-school graduates were born and attended school in the state of the HR manager's firm.[24] Furthermore, at the beginning of the online survey, HR managers are asked whether their firm offers predominantly technical or commercial apprenticeships. Depending on the answer to

[21] We did not include a cover letter or a photograph on the resume, which is standard in German applications, since HR managers in our study were fully aware that they face fictitious candidates.

[22] Firms typically receive many applications for apprenticeship positions. Therefore, in the first stage, HR managers pre-select appropriate candidates based on written applications, which include a cover letter, the resume, and various documents. Subsequently, large firms may conduct written tests and ask applicants to do some trial work, followed by job interviews. About 30 to 40 percent of applicants pass the first stage and only 10 to 15 percent are eventually invited to job interviews (Protsch and Solga, 2015).

[23] The German school system tracks students (in almost all states) after four years in primary school into three secondary-school tracks that differ in academic orientation: basic school (*Hauptschule*), intermediate school (*Realschule*), and high school (*Gymnasium*). Basic school is the least academic track and lasts until grade 9 or 10. It is typically followed by an apprenticeship in a firm that includes part-time attendance in a vocational school. Intermediate school usually lasts until grade 10 and is traditionally also followed by such an apprenticeship. High school is the most academic track and lasts until grade 12 or 13. It is meant to prepare students for college, with the high-school leaving certificate (Abitur) being a precondition for attending college.

[24] Fictitious candidates are born in the capital of the respective state (there are 16 states in Germany), where they have also attended an (existing) intermediate school and completed their internship.

this question, the resumes include candidates who have completed an internship in the respective field (technical or commercial). To further increase realism, we ensure that HR managers receive female applicants only if the share of female employees in their industry is at least 20 percent.[25]

The resumes include the following specific signals of the three broad domains of skills that we focus on in this study.[26] *Cognitive skills* of secondary-school graduates are signalled by their final GPA in school, extended IT skills, English proficiency, and proficiency in French or Spanish as a second language.[27] *Social skills* are signalled by social volunteering (neighbourhood help such as youth work, elderly group, and offering German language courses)[28] and by reporting team sports (as opposed to single sports). *Maturity* is signalled by an age difference of at most four months, with the older candidate being born in the calendar year before the younger candidate (but, given the year of school graduation, from the same regular school birth cohort), and by having conducted a long internship (four weeks as opposed to two weeks).[29]

As HR managers have to select exactly one of the two candidates, we design resumes such that one resume does not dominate the other resume within a CV pair to avoid obvious choices. To this end, we introduce a negative correlation between the school

[25] The share of female employees per industry is computed using the statistics of the German Statistical Office from 2015. We distinguish between 62 industries based on the two-digit German Classification of Economic Activity, Version 2003.

[26] The assignment of the specific signals to one of the three skill domains serves to consolidate the presentation. We do not argue that each skill signal can be exclusively mapped to one domain. Instead, we categorize each skill signal according to the skill domain it most likely reflects, given all other signals. For example, GPAs likely reflect both cognitive components such as ability and non-cognitive components such as perseverance, but arguably most people would consider GPAs primarily as signals of cognitive skills. An alternative would have been to include information that more clearly reflects only one skill domain, such as Raven scores as reflecting only cognitive skills – but this information would never be reported on a CV. Similarly, engaging in volunteer activities or in team rather than single sports may reflect traits beyond sociability such as initiative or specific aspects of fitness. Facing these trade-offs between conceptual sharpness and creating a realistic choice problem among actually used signals, we opt for the latter and only include important CV elements that are typically included in real applications in Germany, as elicited in our pre-study. While this comes at the cost of having to use a slightly *ad-hoc* categorization of skill domains, we believe that the proposed categorization is overall very reasonable. At the minimum, it allows for a more structured presentation of the results for the specific skill signals on a typical CV that should be of interest per se.

[27] Grades in Germany range from 1 (very good) to 6 (failed). For the empirical analysis, we recode all grades (GPAs) such that higher values mean better grades.

[28] These social volunteering activities meet the definition of social engagement used in Heinz and Schumacher (2017). They have been shown to be related to the actual willingness of individuals to cooperate as well as to HR managers' beliefs about the willingness of workers to cooperate.

[29] While these are short periods, it is uncommon for a 16- or 17-year-old secondary-school graduate to have more labour-market experience. Importantly, a two-week internship may signal a compulsory internship during the school year, whereas a four-week internship tends to signal a voluntary labour-market experience obtained during vacations.

GPA (likely an important skill signal) and the other skill signals in the CV. This means that the CV with the better GPA gets worse other skill signals, such as lower language and IT skills (see Appendix A2.1).

Finally, as the effects of innate characteristics have already been studied extensively in the literature, we intentionally keep these characteristics fixed within the choice sets in our experimental design. That is, we keep gender fixed within CV pairs and use only fictitious candidates with German nationality and standard German-sounding names (see Appendix A2.1.

College Graduates

The set-up of resumes of college graduates is similar to those of secondary-school graduates, with only few obvious differences. Appendix A2.2 describes the elements of the resumes of college graduates in detail. Table A2.2 lists all values of all CV elements and Figure 2.2 presents a sample resume for college graduates.

All fictitious college graduates completed upper-track high school (*Gymnasium*) after 12 years of schooling and obtained a four-year Bachelor's degree in business administration at a public German university. We chose business administration since this is by far the most common college major in Germany, with 15 percent of university students being enrolled in business administration during the academic year 2015/16 (Statistisches Bundesamt, 2016). Because business administration is so pervasive, most firms likely hire college graduates with this major. This choice seems particularly appropriate for our context as we do not send applications to actual job advertisements. Instead, we ask HR managers to choose the better applicant for a hypothetical universal trainee position in their firm with a college graduate who has a business administration degree. We use five top-ranked universities and five lower-ranked universities to investigate whether the impact of college GPA varies with college quality (the category, *e.g.,* top-ranked university, is held constant within a CV pair; see Appendix A2.2).

For college graduates, *cognitive skills* are signalled by their college GPA, the most recent productivity signal obtained just before entering the labour market. Further signals of cognitive skills again include extended IT skills, English proficiency (measured as 0 = basic, 1 = very good, and 2 = fluent), and proficiency in French or Spanish as a second language. *Social skills* are signalled by volunteering activities that involve intensive interactions with other people (neighbourhood help or mobile care services) as opposed to volunteering activities that involve only limited social interactions (preservation of monuments or online IT work and translations for the United Nations). Again,

social skills are also signalled by engaging in team sports as opposed to single sports. As for secondary-school graduates, *maturity* is signalled by an age difference of at most four months within a school cohort, with the older candidate being born in the calendar year before the younger candidate (but, given the year of high-school graduation, from the same regular school birth cohort). Maturity is additionally signalled by having completed a long internship (three or five months as opposed to only one month). In the context of college graduates, we interpret high-school GPA as another signal of maturity as, conditional on college GPA, it is more likely to be perceived as a signal of effort during adolescence rather than a signal for cognitive skills.

In contrast to secondary-school graduates who apply for an apprenticeship position in their region, college graduates are geographically more mobile. While the fictitious college graduates attended a high school in their city of birth (using schools that actually exist), they are completely mobile with respect to both the college location and the location of the HR manager's firm.

Each element on the resume is randomized independently of all other CV elements, except for one restriction on GPAs: while the first three GPAs within a CV pair (there are two high-school GPAs and two college GPAs) are randomized independently of each other, the fourth GPA, randomized last, is restricted such that no resume contains both a better high-school GPA and a better college GPA. We impose this restriction since HR managers may select the resume that dominates the other resume with two better GPAs. Finally, as for the resumes of the secondary-school graduates, we keep gender constant within CV pairs and use only candidates with German nationality and German-sounding names.

2.3.3 Descriptive Statistics

We sent the online survey to HR managers in 1,496 firms (one HR manager per firm), of whom 579 HR managers participated. 307 respondents participated in the secondary-school graduate sample and 272 in the college-graduate sample. Given that each HR manager was exposed to two pairs of resumes, we have a total of 1,158 decisions and 2,316 resumes. Table 2.1 provides summary statistics of the resume characteristics for the secondary- school-graduate sample and Table 2.2 for the college-graduate sample. By construction, the mean of our outcome variable – the job interview invitation – is 0.5. The average school GPA (GPAs are recoded as "4 minus actual grade" such that higher values mean better grades) is 1.46 for secondary-school graduates and 1.67 for college graduates; average college GPA is 1.67. There are slightly more male applicants in the secondary-school graduate sample (56.8 percent), which is due to

the fact that female applicants are only presented to HR managers in industries with more than 20 percent of female employees.[30]

The distribution of firms in the ifo Personnel Manager Survey is representative for firms in Germany. As shown in Table A2.3 in the appendix, HR managers (and their firms) who participate in our study do not differ significantly from non-respondents of the ifo Personnel Manager Survey database in terms of location, industry, number of employees, and share of females in the industry.

2.3.4 Empirical Model

The randomization of the different skill signals provides us with identification of their causal effects on being invited for a job interview. We estimate the effects in a first-differenced model, treating a resume pair as the unit of observation. Accordingly, the dependent variable equals 1 if the resume on the left-hand side was selected and equals -1 if the resume on the right-hand side was selected. Similarly, all explanatory variables are constructed as first differences, i.e., the characteristic of the left-hand side resume minus the characteristic of the right-hand side resume. We hence estimate the following OLS specification:

$$\Delta y_{ij} = \beta_0 + \beta_1' \Delta S_{ij} + \epsilon_{ij}, \tag{2.1}$$

where Δy_{ij} is the outcome for CV pair i (i = 1,2) of HR manager j. ΔS_{ij} is a vector of CV-pair-specific differences in skill signals, and ϵ_{ij} is an error term. In some specifications, we additionally include industry fixed effects and even HR manager fixed effects, thus exploiting only variation in the choices within HR managers across the two CV pairs. Our parameters of interest that capture the impact of the skill signals are given by the vector β_1. Throughout, we cluster standard errors at the level of the HR manager.

Note that the magnitudes of the estimated effects of the skill signals do not have a straightforward interpretation in our setting because HR managers are forced to choose exactly one of the two applicants in each CV pair. This likely overemphasises the importance of the skill signals since HR managers might choose both applicants or none when facing real applications, at least in the first stage of the application process. Therefore, we prefer to interpret relative effect sizes, comparing the importance of two different skill signals.

[30] In contrast, the lower share of male applicants in the college-graduate sample arises due to chance in the randomization procedure.

To investigate heterogeneities in the effects of skill signals by HR managers' characteristics, we estimate models with interaction terms between the two:

$$\Delta y_{ij} = \beta_0 + \beta_1' \Delta S_{ij} + \beta_2' C_j \Delta S_{ij} + \epsilon_{ij}, \tag{2.2}$$

where C_j is a characteristic of HR manager j. In particular, we explore two dimensions of potential heterogeneity in the valuation of skill signals by HR managers. First, we study whether skill signal effects vary with HR managers' characteristics such as their age, gender, position and responsibility in the firm, and the size of their firm. Second, we test the consistency between HR managers' observed choices and the preferences that they express for a particular skill signal in the subsequent questionnaire.

2.4 The Impact of Skill Signals on Job-Interview Invitations

We start by presenting the baseline results of the impact of signals of cognitive skills, social skills, and maturity on job-interview invitations for secondary-school graduates and college graduates. In the next section, we turn to effect heterogeneities for different HR managers.

2.4.1 Baseline Results for Secondary-School Graduates

Table 2.3 reports the baseline results of the choice experiment in the secondary-school graduate sample. The baseline specification indicates significant effects of skill signals in each of the three domains – cognitive skills, social skills, and maturity (column 1). Results hardly change when industry fixed effects are added to the first-differenced model (column 2). Signals of cognitive skills strongly affect the invitation decision. In the given setting, an improvement in school GPA by one grade level increases the probability of being invited for a job interview, *ceteris paribus*, by 22 percentage points. The point estimates do not differ significantly between female and male applicants (columns 3 and 4), although the coefficient is slightly smaller and less precisely estimated in the female sample. We tested for non-linearity in the effects of school GPA by adding two interaction terms: school GPA interacted with an indicator for whether both school GPAs in the CV pair are equal to or better than 2.3 (a good GPA) and school GPA interacted with an indicator for whether both school GPAs in the CV pair are equal to or worse than 2.7 (a mediocre GPA). The coefficients on both inter-

action terms are insignificant (not shown), indicating that the impact of school GPA is rather linear.[31]

Extended IT skills also improve the odds of being invited to a job interview. This effect is more pronounced for female applicants, which might reflect implicit gender stereotypes among HR managers that males in general have reasonable IT skills (similar to maths skills, *e.g.* Lummis and Stevenson (1990)), so that returns to these skills are higher for females. Thus, HR managers may subconsciously not place a high value on IT skills signalled by males, but may reward female applicants with this signal because it is less expected among this group.

Foreign language skills, another dimension of cognitive skills, seem to be less important in case of apprenticeship applications. The effect of being fluent (as opposed to basic) in English is marginally significant, whereas having a second foreign language (either French or Spanish) does not have a significant effect.

Signalling social skills by social volunteering has a considerable impact in the hiring process. Specifically, applicants who report doing neighbourhood help such as youth work, elderly group, and offering German language courses have a 37 percentage points higher probability of being invited for a job interview than identical applicants who have not volunteered in our setting. The effect size is almost equivalent to improving school GPA by two grade levels and it is similar for females and males. This indicates strong importance of signalling social skills and commitment for young applicants who enter the labour market directly out of school.

A potential second signal of social skills is whether an applicant participates in team sports such as football (soccer) and basketball as opposed to single sports such as swimming and cycling. This signal, however, does not affect interview invitations among applicants for apprenticeship positions. This may reflect that the social skill component here is dominated by social volunteering and that 17-year-olds commonly play team sports, which may not make it a very good predictor for actual social skills at this age.

Concerning maturity, HR managers significantly prefer older applicants for apprenticeship positions. While all fictitious applicants are 17 years old and born within the same regular birth cohort (given the school graduation year), applicants born in the earlier calendar year are more likely to get chosen. This effect is entirely driven by male applicants, the only significant gender difference in the skill-signal effects for

[31] Similarly, we do not find evidence of non-linearity in the form of an additional effect of having a grade that starts with 1 (indicating top grades in Germany) when added to the linear grade effect (not shown).

apprenticeship positions. Boys may generally be perceived as more immature than girls during adolescence (another form of gender stereotype), so that being slightly older is a relevant signal of maturity for boys. By contrast, we do not find a significant effect of having completed a longer internship – four weeks rather than two weeks – in the secondary-school graduate sample.

As each HR manager receives two pairs of resumes and thus makes two choices, we can additionally restrict the analysis to exploit only variation in decisions within HR managers. With HR manager fixed effects taking out most of the variation, estimates become considerably more imprecise (with standard errors tending to double) and only the effect of social volunteering retains statistical significance at conventional levels (column 5). However, point estimates remain very similar to the baseline model.

2.4.2 Baseline Results for College Graduates

Table 2.4 presents baseline results of the choice experiment in the college graduate sample. Again, we find significant effects of skill signals in each of the three skill domains. However, the specific signals that affect job-interview invitations for college graduates partly differ from the specific signals relevant for secondary-school graduates.[32]

Results show that college grades, one signal of cognitive skills, are an important determinant in the first stage of the hiring decision of HR managers. A better college GPA significantly increases the probability of being invited to a job interview for both females and males. In the given setting, a better college GPA by one grade level increases the likelihood of a job-interview invitation by 38 percentage points. Again, we do not find evidence of non-linearity in the GPA effects among college graduates (not shown).

Results for other cognitive-skill signals are mixed. Extended IT skills and English proficiency do not affect the interview decision in the college graduate sample. One possible explanation is that firms expect that German college graduates have reasonably decent IT skills and English proficiency anyway By contrast, high proficiency in a second foreign language (French or Spanish), which is less common, does improve the probability of an interview invitation. This effect is entirely driven by female college graduates, whereas no such effect is observed for males, possibly reflecting the types

[32] We replicated all results from Tables 2.3 and 2.4 by weighting HR managers with the (log) firm size to account for the relative over-representation of small firms in the German economy. These results are very similar to the unweighted results (not shown).

of occupations or tasks that HR managers expect females to perform. The effect is very similar for whether the second foreign language is French or Spanish (not shown).

Signals of social skills also matter for the employability of college graduates, although to a substantially lesser extent than the signals of cognitive skills. Among females, volunteering work that is characterized by intensive social interactions (such as neighbourhood help or mobile care services) is an advantage over volunteering work that involves less social interactions (such as monument preservation or online office work for the UN). No such effect is found for males. Note that the treatment here is different from the secondary-school graduate sample, where applicants do or do not report volunteering work; by contrast, all college graduates report some sort of volunteering, only that its intensity of social interactions differs.

Interestingly, participation in team sports such as football and basketball, as opposed to single sports such as swimming and cycling, increases the probability of a job interview in the college graduate sample. This contrasts with the lack of an effect in the secondary-school sample. Potentially, at the age of 17, the type of sport that an applicant does may be conceived as being primarily determined by family and friends rather than being a personal choice, so that it is a poor signal of applicants' social skills. By contrast, at the age of 24 in the college graduate sample, the type of sports that a person decides to (continue to) participate in may be more likely perceived as a personal choice and thus a better signal of social skills. In addition, HR managers may be wary of strategic behaviour among applicants who have reached the stage of finishing college. This could account for the fact that reporting social volunteering work has a much smaller effect in the college sample than in the secondary-school sample (besides reflecting a somewhat weaker variation in the signal), whereas it is the other way around for team sports. Knowing that firms value social volunteering Baert and Vujić (2018), even persons with limited social skills may do some voluntary social work only to have the signal. With limited credibility of volunteering as a signal of actual social skills, HR managers may revert to the type of sports as an alternative signal that is less obvious and may thus be less subject to strategic manipulation.

Signals of maturity also seem to play some role in the hiring of college graduates, but again to a lesser extent than cognitive skills. In particular, a longer internship – three or five months as opposed to one month – increases the probability of being invited to a job interview.[33] In contrast to secondary-school graduates, we do not find an advantage for applicants who are older within their school cohort, presumably because small age differences are less relevant for applicants aged 24 than for

[33] There are two separate treatments, either three months of internship or five months of internship. We do not find a significant difference between the two treatment effects, so we combine them into one indicator.

applicants during their adolescence (aged 17 in the secondary-school graduate sample). For male college graduate applicants, we find a significant effect of a better high-school GPA. Holding constant college GPA, we interpret this as a signal that the applicant focused on school work already during adolescence.[34] No such effect is found for female college graduate applicants. This may reflect the same gender stereotype as in the secondary-school graduate sample: boys are perceived as more immature than girls during adolescence, and therefore have higher returns to signals of maturity.

While less precisely estimated, the order of magnitude of the point estimates and the pattern of results are again confirmed in the specification with HR manager fixed effects (column 5). In the college-graduate sample, the effects of college GPA, team sports, and internship duration retain statistical significance at conventional levels even though standard errors with HR manager fixed effects roughly double.

Although the set-up of our experiment was not designed for deeper investigation of the importance of complementarities among different skills or different packages of skills, we also experimented with adding interaction terms between different skill signals to our baseline model (not shown). There are virtually no significant interaction effects among the different randomized skill signals, possibly due to limited statistical power in these models. In particular, we do not find significant interactions between GPAs and social skills. Similarly, there is no consistent pattern of effect heterogeneities by the four high-school types used on the college graduate resumes or by whether the college is top-ranked or not.

In sum, signals in all three domains – cognitive skills, social skills, and maturity – have a causal impact on the decisions of HR managers whom to invite for a job interview among both secondary-school and college graduates. In general, cognitive skills as signalled by school GPA for secondary-school graduates and college GPA for college graduates play a consistently important role. Among college graduates, cognitive skills tend to be relatively more relevant than social skills, whereas social skills seem to be more relevant for young school graduates applying for an apprenticeship. Signals of maturity also tend to be more important for young job applicants in their adolescence, with effects being restricted to boys. In contrast, IT skills tend to be more important for female secondary-school graduates, possibly because of common gender stereotypes that boys have an affinity to computers. Overall, the observed effect heterogeneities across entry ages and genders appear consistent with differences in the relevance, expectedness, and credibility of specific signals in different settings.

[34] Additionally controlling for whether the GPA has improved from school to college does not change the coefficient on high-school GPA and slightly increases the coefficient on college GPA (not shown).

2.5 Heterogeneous Effects for Different HR Managers

In this section, we turn to effect heterogeneities with respect to HR manager characteristics. Furthermore, we investigate whether decisions in the choice experiment are consistent with HR managers' answers to survey questions regarding the importance of various skill signals of applicants in their firm.

2.5.1 Heterogeneity by HR Manager Characteristics

In contrast to most existing CV studies, we have information about the characteristics of the HR managers who make the decisions in our setting. After the choice experiments, we provided the HR managers with a short survey questionnaire that included questions on their personal characteristics and on the importance they assign to different skill signals in actual applications to their firm (see Appendix A2.3). The information on HR managers' personal characteristics includes age, gender, educational attainment, whether they are responsible for hiring decisions in their firm, whether they are the managing director of the firm (more likely in smaller firms), and how many job interviews they have conducted during the past 12 months. This information allows us to investigate whether the effects of the different skill signals differ across different types of HR managers.

Table 2.5 provides summary statistics of the characteristics of HR managers and firms, separately for the secondary-school graduate and college graduate samples. In both samples, HR managers are on average 50 years old, and about two thirds are male. HR managers in the secondary-school graduate sample are more likely to be managing directors of their firms, presumably because apprenticeship-offering firms tend to be smaller than those in the college sample. Most HR managers (87-88 percent) are responsible for the actual final hiring decisions in their firm. HR managers are rather similarly divided between the three types of professional degrees – vocational education, university of applied sciences, and university – in the two samples. Firms in the secondary-school graduate sample, all of which employ apprentices, are more likely to be in the manufacturing sector, whereas firms in the college graduate sample are more likely to be in the real estate sector. Furthermore, firms in the secondary-school graduate sample are more likely to be in industries where the share of women is below 20 percent. This is likely due to the fact that many apprenticeship positions are technical jobs.

Tables 2.6 and 2.7 report estimated coefficients on interaction terms between the various skill signals (indicated in the first column) and selected characteristics of the HR managers and firms (indicated in the column headers) for the secondary-school grad-

uate and the college graduate sample, respectively. Each cell stems from a separate regression.

For apprenticeship positions, we find substantial effect heterogeneity with respect to HR manager characteristics (Table 2.6). Older HR managers (split at median age 51) put less weight on school grades and more weight on IT skills. Older managers also appreciate social volunteering more, but consider team sports to be less important for the decision whom to invite for a job interview. These differences between young and old HR managers might indicate that older HR managers have gained the experience that school grades predict the workplace performance of apprentices less well than signals of specific abilities such as IT skills. Similarly, experienced HR managers might have experienced that social volunteering is indeed a good signal for social skills that are important on the labour market. Interestingly, the focus of HR managers who are also the managing directors of their firm goes in the same direction as that of older HR managers, partly reflecting that the majority of managing directors (59.5 percent) are also in the subgroup of older HR managers. Furthermore, managing directors, as well as HR managers who are responsible for hiring decisions in their firm, place more weight on work experience through long internships.

We also find substantial and intuitive differences between apprenticeship positions in the technical sector and in the commercial area. Concerning signals of cognitive skills, the priorities of HR managers in the technical sector are more strongly focused on school GPA and less on specific skills in IT and second foreign languages, which might be particularly relevant for apprenticeship positions in the commercial area. Similarly, social volunteering is more important in the commercial area, likely reflecting higher relevance of social skills in commercial jobs than in technical jobs. Longer internship duration is also more important for HR managers in the commercial area than in the technical sector. By contrast, effects of the different skill signals in the secondary-school graduate sample hardly differ between female and male HR manager, and they do not vary significantly with firm size.

Overall, there is less effect heterogeneity with respect to HR manager characteristics in the college graduate sample (Table 2.7). Interestingly, HR managers in larger firms place more weight on college GPA, possibly reflecting a more standardized and automated screening process in larger firms with a particular focus on formal signals or with applicants having to pass a specific GPA threshold. Easy verifiability of signals may thus be particularly important for large firms in the first hiring stage. Older HR managers, males, and those with hiring responsibility place more weight on proficiency in a second foreign language. Internship duration seems less important for managing directors and HR managers with hiring responsibility. HR managers who ob-

tained an apprenticeship degree care less about high-school GPA than HR managers with a college degree; HR managers with a technical college degree put less weight on college GPA and more weight on social skills.

2.5.2 Consistency with Stated Priorities in Survey Questions

At the end of the questionnaire, HR managers also indicate their priorities for various skill signals of actual applicants applying for jobs in their firm. Table 2.8 shows that 67 percent of HR managers in the secondary-school graduate sample report that school GPAs are either "rather important" or "very important" (as opposed to "rather unimportant" or "very unimportant"). An even larger share of HR managers states that school grades in specific main subjects are important: 89 percent in maths and 81 percent in German. IT skills are considered important by 86 percent of HR managers, language skills by 66 percent, and professional experience through internships by 74 percent. In the college graduate sample, 47 percent of HR managers view high-school GPA as important and 81 percent college GPA, the more recently acquired skill signal. The shares of HR managers who view IT skills (96 percent), language skills (83 percent), and professional experience through internships (94 percent) as important are all higher in the college graduate sample than in the secondary-school graduate sample. The only dimension without a significant difference between the two samples is hobbies (47 to 48 percent).

The priorities reported by HR managers in the questionnaire allow us to investigate whether answers to survey questions are consistent with choices between the fictitious applicants in the choice experiment. This yields insights into whether HR managers' answers in survey questionnaires are in line with their decisions when comparing entire resumes of applicants. To investigate this question, we add to our baseline model an interaction term between a skill signal in our fictitious resumes and the degree of importance that the HR manager assigns to that specific skill signal in the questionnaire (using the original four-point scale). We estimate models with interaction terms separately for each skill signal that is included both in the questionnaire and in the fictitious resumes.

For HR managers in the secondary-school graduate sample, all interaction terms are positive and statistically significant (Table 2.9). This implies that HR managers who report in the questionnaire that a certain skill signal is important do indeed put more weight on that signal when choosing between resumes. This is the case for school GPA, IT skills, language proficiency (English and second foreign language), and internship duration.

Similar positive interactions are found for HR managers in the college graduate sample, with the interactions with college GPA, IT skills, and English language proficiency reaching statistical significance at conventional levels (Table 2.10). The results again indicate that choices are consistent with self-reported priorities in the questionnaire. Interestingly, statistical significance is reached for the three signals of cognitive skills, which is the most important domain of signals among college graduates, whereas the signals of maturity (high-school GPA and internship duration), which are in general less important for decisions among college graduates, do not capture statistical significance.

2.6 Conclusion

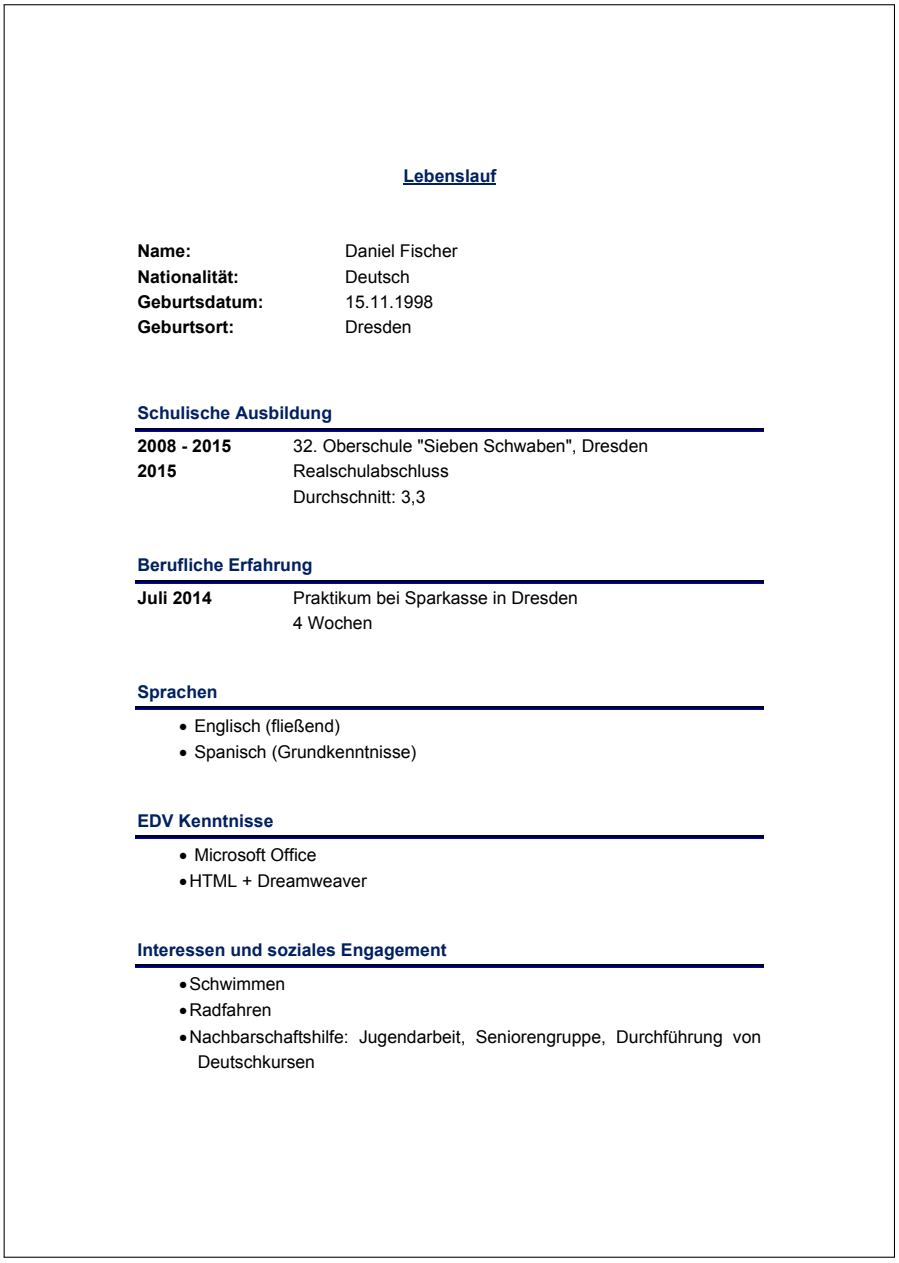
We conduct a randomized CV study among HR managers to investigate how acquired signals of a broad range of cognitive skills, social skills, and maturity are valued by employers. We find evidence that signals in each of the three domains increase the probability of being invited for a job interview. Given our experimental design, these are separate and independent effects of signals in the different skill domains, with little evidence of strong complementarities between the domains. The results indicate at least three conclusions on how labour markets function. First, skills in all three domains matter on the labour market. Second, applicants can effectively signal these skills to employers before career start with information contained on their CVs. Third, associations of labour-market outcomes with skill indicators such as school or college grades in observational data do in fact have a causal interpretation in the sense that employers observe and react to them during the application stage. Furthermore, the importance of the specific signals differs depending on the respective relevance, stereotypical expectedness, and credibility of the signal in different contexts. Important effect heterogeneities exist between secondary-school graduates applying for an apprentice position and college graduates. While signals of cognitive skills (particularly college GPA) seem to be more important than signals of social skills and maturity among college graduates, this is not true for secondary-school graduates. School GPA of secondary-school graduates and college GPA of college graduates – signals of cognitive skills – are important for both genders, but other signals matter more for one gender than the other: IT and second language skills – and, to a lesser extent, social skills – are particularly relevant for females, whereas signals of maturity are particularly relevant for males. These differences might reflect gender stereotypes in relevance (with stronger effects for the gender which is expected to perform the respective tasks) and in expectedness (with stronger effects for the gender for which the skill is less expected). Social volunteering is a strong signal of social skills among secondary-school

graduates. Among college graduates, however, social skills are more effectively signalled by engaging in team sports, consistent with reduced credibility of social volunteering as a signal of actual social skills due to potential strategic behaviour at older ages.

We also find differences in the impact of skill signals across different groups of HR managers. For secondary-school graduates, older HR managers and managing directors put less weight on school GPA and more weight on IT skills, social volunteering, and internship duration. Among college graduates, HR managers in larger firms place more weight on college GPA than those in smaller firms, which might reflect more standardized procedures of hiring applicants in larger firms that attach more importance to easily verifiable skill signals. Using a questionnaire that asks HR managers to indicate their priorities of skill signals of actual applicants applying for jobs in their firm, we find that the decisions in the choice experiments are consistent with HR managers' self-reported hiring priorities. Together, the effect heterogeneities by entry age, gender, and HR managers reveal important aspects about how signals of skills are processed and utilized in the labour market.

Figures and Tables

Figure 2.1 : Example CV of a Secondary-School Graduate



Lebenslauf

Name:	Daniel Fischer
Nationalität:	Deutsch
Geburtsdatum:	15.11.1998
Geburtsort:	Dresden

Schulische Ausbildung

2008 - 2015	32. Oberschule "Sieben Schwaben", Dresden
2015	Realschulabschluss Durchschnitt: 3,3

Berufliche Erfahrung

Juli 2014	Praktikum bei Sparkasse in Dresden 4 Wochen

Sprachen

- Englisch (fließend)
- Spanisch (Grundkenntnisse)

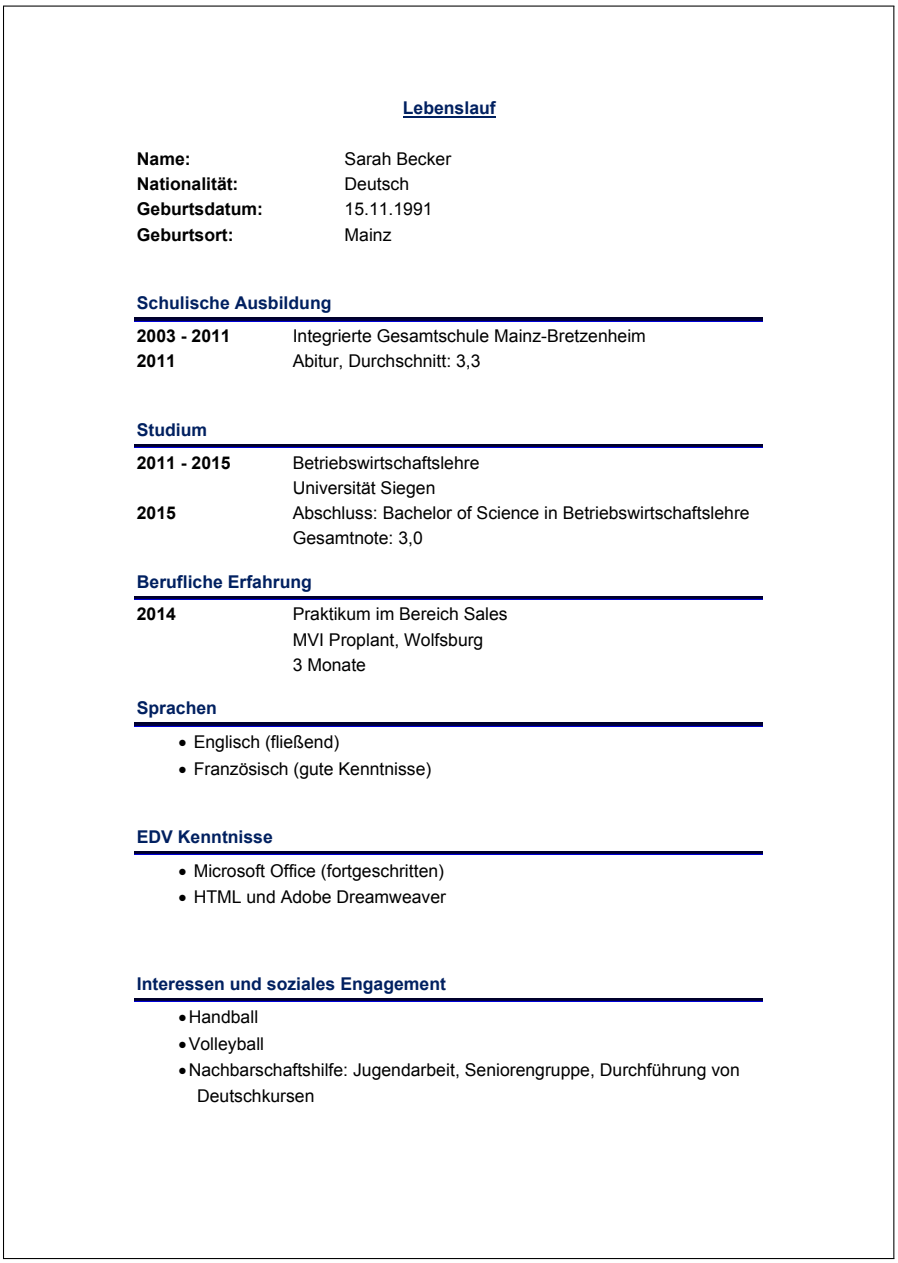
EDV Kenntnisse

- Microsoft Office
- HTML + Dreamweaver

Interessen und soziales Engagement

- Schwimmen
- Radfahren
- Nachbarschaftshilfe: Jugendarbeit, Seniorengruppe, Durchführung von Deutschkursen

Figure 2.2 : Example CV of a College Graduate



Lebenslauf

Name:	Sarah Becker
Nationalität:	Deutsch
Geburtsdatum:	15.11.1991
Geburtsort:	Mainz

Schulische Ausbildung

2003 - 2011	Integrierte Gesamtschule Mainz-Bretzenheim
2011	Abitur, Durchschnitt: 3,3

Studium

2011 - 2015	Betriebswirtschaftslehre Universität Siegen
2015	Abschluss: Bachelor of Science in Betriebswirtschaftslehre Gesamtnote: 3,0

Berufliche Erfahrung

2014	Praktikum im Bereich Sales MVI Proplant, Wolfsburg 3 Monate

Sprachen

- Englisch (fließend)
- Französisch (gute Kenntnisse)

EDV Kenntnisse

- Microsoft Office (fortgeschritten)
- HTML und Adobe Dreamweaver

Interessen und soziales Engagement

- Handball
- Volleyball
- Nachbarschaftshilfe: Jugendarbeit, Seniorengruppe, Durchführung von Deutschkursen

Table 2.1 : Summary Statistics: Secondary-School Graduate CVs

	Mean	Std. Dev.
Job interview invitation	0.500	
Cognitive Skills		
School GPA	1.460	0.628
Extended IT skills	0.491	
Fluent English	0.516	
French as 2nd language	0.237	
Spanish as 2nd language	0.265	
Social Skills		
Social volunteering	0.458	
Team sports	0.500	
Maturity		
Age within school cohort	0.416	
Long internship	0.514	
Non-varying Characteristics		
Male	0.568	
Technical internship	0.477	
State		
Baden-Wuerttemberg	0.160	
Bavaria	0.254	
Berlin	0.003	
Brandenburg	0.023	
Bremen	0.007	
Hamburg	0.020	
Hesse	0.072	
Lower Saxony	0.088	
Mecklenburg-Vorpommern	0.013	
North Rhine-Westphalia	0.178	
Rhineland-Palatinate	0.024	
Saarland	0.007	
Saxony	0.088	
Saxony-Anhalt	0.010	
Schleswig-Holstein	0.020	
Thuringia	0.036	
N (CVs)	1228	

Note: Means of CV elements of secondary-school graduates. All variables except school GPA are binary.

Table 2.2 : Summary Statistics: College Graduate CVs

	Mean	Std. Dev.
Job interview invitation	0.500	
Cognitive Skills		
College GPA	1.665	0.683
Extended IT skills	0.500	
Very good English	0.367	
Fluent English	0.318	
French as 2nd language	0.267	
Spanish as 2nd language	0.245	
Social Skills		
Social volunteering	0.500	
Team sports	0.500	
Maturity		
High-school GPA	1.677	0.674
Age within school cohort	0.380	
3-months internship	0.275	
5-months internship	0.330	
Non-varying Characteristics		
Male	0.441	
Catholic school	0.360	
Comprehensive secondary	0.353	
Top-ranked university	0.553	
Internship in sales	0.232	
Internship in controlling	0.395	
College		
University of Munich	0.106	
RWTH Aachen	0.092	
University of Frankfurt	0.110	
University of Cologne	0.132	
University Leuphana Lueneburg	0.084	
University of Mannheim	0.113	
University of Siegen	0.098	
University of Trier	0.107	
University of Bremen	0.084	
High-school State		
Baden-Wuerttemberg	0.066	
Bavaria	0.202	
Hesse	0.132	
Lower Saxony	0.153	
North Rhine-Westphalia	0.172	
Rhineland-Palatinate	0.128	
Thuringia	0.147	
N (CVs)		1088

Note: Means of CV elements of college graduates. All variables except GPAs are binary.

Table 2.3 : Skill Signals and Job-Interview Invitation: Baseline Results for Secondary-School Graduates

	(1) All	(2) All	(3) Female	(4) Male	(5) All
Cognitive Skills					
School GPA	.22***	.21***	.18	0.24**	0.22
	(0.08)	(0.08)	(0.14)	(0.10)	(0.16)
Extended IT skills	0.17***	0.16***	0.22**	0.14	0.20
	(0.06)	(0.06)	(0.09)	(0.09)	(0.13)
Fluent English	0.12*	0.12*	0.14	0.09	0.01
	(0.06)	(0.07)	(0.10)	(0.09)	(0.13)
2nd foreign language	0.04	0.05	−0.05	0.10	−0.02
	(0.06)	(0.06)	(0.10)	(0.08)	(0.14)
Social Skills					
Social volunteering	0.37***	0.36***	0.38***	0.36***	0.50***
	(0.07)	(0.07)	(0.10)	(0.08)	(0.14)
Team sports	0.01	0.02	0.03	−0.01	0.09
	(0.04)	(0.04)	(0.06)	(0.05)	(0.08)
Maturity					
Age within school cohort	0.13***	0.14***	−0.00	0.24***	0.16
	(0.05)	(0.05)	(0.08)	(0.07)	(0.10)
Long internship	0.04	0.04	0.03	0.05	0.09
	(0.06)	(0.06)	(0.09)	(0.09)	(0.13)
Industry FE	No	Yes	Yes	Yes	
HR manager FE	No	No	No	No	Yes
R^2	0.119	0.125	0.175	0.130	0.493
N (CV pairs)	614	614	264	350	614

Note: First-differenced model with CV pair as unit of observation. Dependent variable: invitation for job interview. Standard errors clustered at HR-manager level in parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Table 2.4 : Skill Signals and Job-Interview Invitation: Baseline Results for College Graduates

	(1) All	(2) All	(3) Female	(4) Male	(5) All
Cognitive Skills					
College GPA	0.38***	0.39***	0.33***	0.44***	0.28**
	(0.07)	(0.07)	(0.11)	(0.09)	(0.14)
Extended IT skills	–0.03	–0.02	0.02	–0.00	–0.02
	(0.06)	(0.06)	(0.08)	(0.09)	(0.13)
English level	–0.03	–0.03	–0.07	–0.01	–0.05
	(0.04)	(0.04)	(0.05)	(0.06)	(0.07)
2nd foreign language	0.11*	0.12**	0.25***	–0.04	0.05
	(0.06)	(0.06)	(0.08)	(0.08)	(0.12)
Social Skills					
Social volunteering	0.07	0.06	0.10*	–0.00	–0.00
	(0.05)	(0.05)	(0.06)	(0.07)	(0.10)
Team sports	0.10**	0.10**	0.11*	0.06	0.16*
	(0.04)	(0.04)	(0.06)	(0.07)	(0.09)
Maturity					
High-school GPA	0.05	0.06	–0.02	0.14*	–0.03
	(0.05)	(0.05)	(0.07)	(0.08)	(0.11)
Age within school cohort	0.00	0.01	0.12	–0.08	0.03
	(0.06)	(0.06)	(0.08)	(0.08)	(0.13)
Long internship	0.12***	0.13***	0.13***	0.17***	0.15**
	(0.03)	(0.03)	(0.05)	(0.05)	(0.06)
Industry FE	No	Yes	Yes	Yes	
HR manager FE	No	No	No	No	Yes
R^2	0.139	0.150	0.188	0.182	0.564
N (CV pairs)	544	544	304	240	544

Note: First-differenced model with CV pair as unit of observation. Dependent variable: invitation for job interview. Long internship refers to internship of three or five months (instead of only one month). Standard errors clustered at HR-manager level in parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Table 2.5 : Summary Statistics: HR Manager and Firm Characteristics

	Secondary-school graduates		College graduates	
	Mean	Std. Dev.	Mean	Std. Dev.
HR Manager Characteristics				
Age	49.944	10.0	49.664	9.8
Male	0.658		0.651	
Managing director	0.437		0.288	
Hiring responsibility	0.875		0.871	
Professional education level				
Vocational education degree	0.309		0.217	
University of applied sciences degree	0.250		0.258	
University degree	0.411		0.472	
None/Other	0.026		0.051	
Firm Characteristics				
Number of employees	331	1286.7	442	1817.9
Industry share female employees $\leq$ 20%	0.239		0.118	
Industry				
Manufacturing	0.534		0.360	
Trade, maintenance and reparations	0.182		0.136	
Hospitality	0.032		0.025	
Transport and communication	0.072		0.074	
Real estate	0.162		0.341	
Other public services	0.016		0.062	
N (HR managers)	307		272	

Note: Means (and standard deviations for continuous variables) reported. HR manager characteristics come from the survey questionnaire and firm characteristics come from the ifo Personnel Manager Survey Database. "Industry share female employees $\leq$ 20%" refers to the share of firms in industries with less than 20 % female employment. Indicated industries refer to the 1-digit German Classification of Economic Activity, Version 2003.

Table 2.6 : Interactions of Skill Signals with HR Manager and Firm Characteristics: Secondary-School Graduates

	(1) HR age above median	(2) Male	(3) Managing director	(4) Hiring responsibility	(5) Technical sector	(6) Firm size $\geq$ 250 employees
Cognitive Skills						
School GPA	-0.22**	0.04	-0.33***	-0.24	0.16*	0.03
	(0.10)	(0.11)	(0.09)	(0.16)	(0.10)	(0.12)
Extended IT skills	0.19*	0.16	0.26***	0.21	-0.21**	-0.01
	(0.10)	(0.10)	(0.10)	(0.15)	(0.10)	(0.12)
Fluent English	0.09	-0.08	0.10	0.12	-0.12	0.11
	(0.10)	(0.10)	(0.10)	(0.15)	(0.10)	(0.13)
2nd foreign language	0.10	0.19*	0.24**	0.21	-0.22**	-0.02
	(0.10)	(0.11)	(0.10)	(0.14)	(0.10)	(0.12)
Social Skills						
Social volunteering	0.22**	0.06	0.24**	0.20	-0.19*	-0.10
	(0.11)	(0.12)	(0.11)	(0.18)	(0.11)	(0.13)
Team sports	-0.16**	0.08	-0.05	0.12	0.02	0.14
	(0.08)	(0.08)	(0.08)	(0.12)	(0.08)	(0.09)
Maturity						
Age within school cohort	-0.01	0.03	-0.02	0.07	-0.14	0.03
	(0.10)	(0.11)	(0.10)	(0.16)	(0.10)	(0.12)
Long internship	0.11	0.03	0.21**	0.27*	-0.23**	0.12
	(0.10)	(0.11)	(0.10)	(0.16)	(0.10)	(0.13)

Note: Each cell stems from a separate regression. The reported coefficients are the interaction terms between the two elements indicated in the header and first column. First-differenced model with CV pair as unit of observation. Dependent variable: invitation for job interview. Controls for other CV elements as in Table 2.3 included throughout. All regressions include industry fixed effects. Standard errors clustered at HR-manager level in parentheses. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 2.7 : Interactions of Skill Signals with HR Manager and Firm Characteristics: College Graduates

	(1) HR age above median	(2) Male	(3) Managing director	(4) Hiring responsibility	(5) Vocational degree	(6) Technical college degree	(7) Firm size $\geq$ 250 employees
Cognitive Skills							
College GPA	-0.06	-0.13	-0.17	-0.05	0.18	-0.31***	0.23**
	(0.11)	(0.10)	(0.13)	(0.12)	(0.11)	(0.11)	(0.10)
Extended IT skills	0.16	0.08	-0.03	0.18	0.15	0.01	0.19
	(0.12)	(0.13)	(0.13)	(0.18)	(0.15)	(0.14)	(0.14)
English level	-0.03	-0.07	-0.14*	0.04	0.02	0.06	0.08
	(0.07)	(0.07)	(0.08)	(0.10)	(0.08)	(0.08)	(0.08)
2nd foreign language	0.25**	0.26**	0.20	0.29**	-0.20	-0.16	-0.31**
	(0.12)	(0.12)	(0.13)	(0.14)	(0.14)	(0.14)	(0.13)
Social Skills							
Social volunteering	-0.05	0.04	-0.13	0.16	-0.11	0.23**	-0.11
	(0.09)	(0.09)	(0.10)	(0.11)	(0.10)	(0.10)	(0.10)
Team sports	0.10	0.00	-0.02	-0.07	0.00	0.27***	0.06
	(0.08)	(0.09)	(0.09)	(0.12)	(0.10)	(0.09)	(0.10)
Maturity							
High-school GPA	0.05	0.11	-0.06	-0.11	-0.21**	0.14	00.00
	(0.08)	(0.08)	(0.1)	(0.1)	(0.09)	(0.10)	(0.11)
Age within school cohort	0.01	0.00	0.01	0.03	-0.10	0.04	0.02
	(0.11)	(0.11)	(0.14)	(0.14)	(0.12)	(0.13)	(0.13)
Long internship	-0.06	-0.07	-0.16**	-0.14*	0.08	-0.05	0.01
	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(.07)

Note: Each cell stems from a separate regression. The reported coefficients are the interaction terms between the two elements indicated in the header and first column. First-differenced model with CV pair as unit of observation. Dependent variable: invitation for job interview. Long internship refers to internship of three or five months (instead of only one month). Controls for other CV elements as in Table 2.4 included throughout. All regressions include industry fixed effects. Columns 5 and 6 refer to the highest education level of the HR manager. Standard errors clustered at HR-manager level in parentheses. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 2.8 : HR Managers' Stated Priorities: Survey Results

	HR manager sample		Difference (1)-(2)	p-value
	Secondary-school graduates	College graduates		
	(1)	(2)	(3)	(4)
School GPA	0.668	0.469	0.199	0.000
College GPA	n/a	0.808	n/a	n/a
German grade	0.806	n/a	n/a	n/a
Math grade	0.885	n/a	n/a	n/a
IT skills	0.858	0.956	-0.098	0.000
Language skills	0.657	0.828	-0.172	0.000
Professional experience through internships	.736	.941	-.205	.000
Hobbies	.483	.472	.011	.593
N (HR managers)	307	272		

Note: Shares of HR managers stating to find the indicated characteristic "very important" or "rather important". p-values in column (4) stem from a two-sided t-test.

Table 2.9 : Interactions of Skill Signals with HR Manager Priorities: Secondary-School Graduates

	(1) School GPA	(2) Extended IT skills	(3) Fluent English	(4) 2nd foreign language	(5) Long internship
CV element	0.21***	0.16**	0.13**	0.06	0.04
	(0.07)	(0.06)	(0.07)	(0.06)	(0.06)
Interaction with HR priority	0.36***	0.27***	0.32***	0.27***	0.18**
	(0.07)	(0.08)	(0.06)	(0.06)	(0.08)
Controls for other CV elements	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
R^2	0.171	0.152	0.175	0.163	0.143
N (CV pairs)	601	605	605	605	605

Note: First-differenced model with CV pair as unit of observation. Dependent variable: invitation for job interview. Controls for other CV elements as in Table 2.3 included throughout. HR priority refers to importance given to the respective CV element (on a 4-point scale) by the HR manager in the questionnaire (1=very unimportant, 2=rather unimportant, 3=rather important, 4=very important). In columns 3 and 4, this importance refers to "language skills", in column 5 to "professional experience through internships". Standard errors clustered at HR-manager level in parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Table 2.10 : Interactions of Skill Signals with HR Manager Priorities: College Graduates

	(1) College GPA	(2) Extended IT skills	(3) English level	(4) 2nd foreign language	(5) High-school GPA	(6) Long internship
CV element	0.43***	0.02	0.05	0.12**	0.08	0.26***
	(0.07)	(0.06)	(0.07)	(0.06)	(0.05)	(0.06)
Interaction with HR priority	0.27***	0.23**	0.18**	0.12	0.08	0.02
	(0.08)	(0.12)	(0.09)	(0.08)	(0.07)	(0.10)
Controls for other CV elements	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.168	0.163	0.171	0.168	0.156	0.154
N (CV pairs)	542	542	536	536	542	542

Note: First-differenced model with CV pair as unit of observation. Dependent variable: invitation for job interview. Long internship refers to internship of three or five months (instead of only one month). Controls for other CV elements as in Table 2.10 included throughout. HR priority refers to importance given to the respective CV element (on a 4-point scale) by the HR manager in the questionnaire (1=very unimportant, 2=rather unimportant, 3=rather important, 4=very important). In columns 3 and 4, this importance refers to "language skills", in column 6 to "professional experience through internships". Standard errors clustered at HR-manager level in parentheses. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Appendix

Appendix A2.1 Resumes of Secondary-School Graduates

Each secondary-school graduate applying for an apprenticeship position is represented by a one-page CV. Figure 2.1 shows an example CV of a secondary-school graduate. Table A2.1 lists all possible values of all CV elements that are randomly attributed to the secondary-school-graduate resumes. Elements marked with a star – gender and apprenticeship area (commercial or technical) – do not vary within a CV pair shown to an HR manager. The other elements – name, date of birth, school GPA, internship duration, English proficiency, 2nd foreign language, IT skills, voluntary work, and sports – vary randomly within CV pairs. We use five different first names for each gender and five different last names. The last names are the most common family names in Germany, while the first names are among the ten most common names of boys and girls of the birth cohort of the fictitious candidates (1998/1999). Table A2.4 indicates that HR managers do not strongly prefer specific first names or specific family names. Invitation rates for the job interview differ statistically significantly from 0.5 (at the 5 percent level) in only two of 30 cases. Candidates are born within a four-month period between 15 November 1998 and 10 March 1999 in the capital of the respective state in which the firm is located. Grades in Germany, both in school and in college, range from 1 (very good) to 6 (failed). A grade of 4 (adequate) is typically the passing grade and GPAs typically involve decimal places. The school GPAs in our resumes range from 1.3 to 3.3, that is, between very good and satisfactory. Each resume includes two sports disciplines, either two team sports or two single sports. The secondary-school graduates do their internships either in a "technical" or "commercial" job at a local craft or retail company. Technical internships differ across gender and involve, e.g., carpenter for males and hairdresser for females. Commercial internships are the same for both gender and include, e.g., positions in banks or supermarkets. To obtain a negative correlation between the school GPA and the other skill signals on the resume, we construct a point index for the other skill signals, with each skill signal receiving 1 point if the signal is positive (and 0 otherwise). In particular, the following signals receive 1 point: fluent English proficiency (vs. basic), basic French or Spanish proficiency (vs. no second foreign language), extended IT skills (Microsoft Office plus HTML plus Dreamweaver vs. only Microsoft Office), social volunteering (vs. no volunteering), and four weeks of internship (vs. two weeks). Within each CV pair, the resume with the worse school GPA includes other skill signals with an index that is 2, 3, or 4 points higher.

Appendix A2.2 Resumes of College Graduates

Each college graduate applying for a business trainee position is represented by a one-page CV. Figure 2.2 shows an example CV of a college graduate. Table A2.2 lists all possible values of all CV elements that are randomly attributed to the college graduate resumes. Elements marked with a star – gender, type of secondary school, college type, and business area of the internship – do not vary within a CV pair shown to an HR manager. The other elements – name, date of birth, place of birth, high school GPA, college GPA, internship firm and duration, English proficiency, 2nd foreign language, IT skills, volunteering, and sports – vary randomly within CV pairs. College graduate resumes have the same five first names and five last names as the secondary-school graduates. The first names are again among the ten most common names in the year the candidates were born. Candidates are born within a four-month period between 15 November 1991 and 10 March 1992 in the capital in one of six states. The applicants went to one of three high school types, which are held constant within CV pairs: a regular high school (Gymnasium), a private catholic school, or a comprehensive school. All schools in the fictitious resumes actually exist in the respective city of birth. All candidates obtained their high school leaving certificate (Abitur) in 2011. Resumes include the high school GPA (Abiturnote). All applicants have obtained a four-year Bachelor's degree in business administration at a public German university. The degrees are from universities ranked either in the top five or bottom five in the category undergraduate business degree of the CHE University Ranking 2015. Note that there are no tuition fees and typically no entrance exams for public colleges in Germany. Resumes include the college GPA. High school GPA and college GPA range from 1.3 to 3.3, that is, between very good and satisfactory. We do not use the best and worst possible grades to avoid very large (and quite uncommon) performance changes from high school to college. To avoid that one CV in a pair dominates on both GPAs, we randomize three of the four GPAs in each CV pair independently, but restrict the fourth GPA in a way that ensures that no CV contains better GPAs at both levels. The area of the internship (sales, accounting, or controlling) is held constant within CV pairs in order not to give one candidate a particular advantage in case that the HR manager's firm happens to specialize in that area. Fictitious candidates have completed their internship at one of four existing mid-size firms that offer student internships on an online job portal. All candidates have English language skills, but the level of proficiency varies between basic, very good, and fluent. Candidates may or may not have a second foreign language, either French or Spanish (basic level). All candidates are proficient in Microsoft Office, whereas some candidates have additional IT skills in both HTML and Dreamweaver.

Appendix A2.3 Questionnaire

HR managers are given the following questionnaire (translated from the original German version) after they have selected their preferred fictitious applicants.

1. How old are you? (drop down menu, 18-100 years)
2. You are: (male, female)
3. Which professional qualification do you have? (vocational degree, university of applied sciences degree, university degree, no professional degree, other degree)
4. Are you responsible for hiring decisions in your establishment? (yes, no)
5. Are you the managing director of the firm? (yes, no)
6. How many job interviews have you approximately conducted during the previous 12 months? (0, 1-9, 10-50, more than 50)
7. How important are for you the following characteristics of applicants in your firm? (very important, rather important, rather unimportant, very unimportant)

   All HR managers

   - IT skills
   - Language skills
   - Professional experience through internships
   - Hobbies

   Only HR managers in secondary-school-graduate sample

   - GPA of school-leaving degree
   - German grade
   - Math grade

   Only HR managers in college-graduate sample

   - High-school GPA (Abiturnote)
   - College GPA

Appendix A2.4 Appendix Tables

Table A2.1 : Values of All CV Elements of Secondary-School Graduates

Gender*	Female	Male			
Male first name	Alexander	Christian	Patrick	Daniel	Tobias
Female first name	Sarah	Laura	Anna	Katharina	Julia
Last name	Becker	Fischer	Mayer	Schneider	Weber
Date of birth	15-Nov-98	2-Dec-98	22-Jan-99	12-Feb-99	10-Mar-99
School GPA	1.3; 1.7; 2.0; 2.3; 2.7; 3.0;3.3				
Apprentice area*	commercial	technical			
Commercial internship	at Sparkasse	at carhouse	in bookstore	in hotel	in supermarket
Technical internship f	at hairdresser salon	at cosmetic studio	at bakery	at jeweller	at photographic shop
Technical internship m	with floortiler craftsman	at bakery	at locksmith	with carpenter	at painter
Internship length	4 weeks	2 weeks			
English	fluent	basic			
2nd foreign language	French	Spanish			
IT skills	Microsoft Office	Microsoft Office, HTML and Dreamweaver			
Sports	single	team			
Single sports	swimming	cycling	running		
Team sports	handball	volleyball	basketball	football	
Social volunteering		Neighbourhood help: youth and senior group, German language classes (social)			

Note: This table shows all values of all CV elements that were randomized. * denotes elements that are fixed within CV pairs. School GPAs range from 1.3 (very good) to 3.3 (satisfactory); for the analysis grades are converted to points (4-grade). Technical internships vary for male and female candidates to ensure credibility. Each resume contains either two single sports or two team sports. In half of the resumes, there is no social volunteering.

Table A2.2 : Values of All CV Elements of College Graduates

Gender*	Female	Male					
Male first name	Alexander	Christian	Patrick	Daniel	Tobias		
Female first name	Sarah	Laura	Anna	Katharina	Julia		
Last name	Becker	Fischer	Mayer	Schneider	Weber		
Date of birth	15-Nov-91	2-Dec-91	22-Jan-92	12-Feb-92	10-Mar-92		
Place of birth	Wiesbaden	Erfurt	Mainz	Hannover	Düsseldorf	München	Stuttgart
Type of secondary school*	catholic	integrated comprehensive	neutral				
Catholic	Bishop-Naumann-School Königstein	Edith-Stein-School Erfurt	Theresianum, Mainz	Gymnasium St.-Ursula-School Hannover	Suitbertus-Gymnasium, Düsseldorf	Maria-Ward-Gymnasium München	Sankt-Meinrad-Gymnasium
Integrated Comp.	Helene Lange Comp. secondary Wiesbaden	Comp. secondary Erfurt	Comp. secondary Mainz-Bretzenheim	Comp. secondary Linden, Hannover	Heinrich-Heine Comp. secondary, Düsseldorf	Willy-Brandt Comp. secondary München	Elise von König School
Neutral	Gymnasium Leibniz School Wiesbaden	Albert-Schweizer Gymnasium Erfurt	Gymnasium Mainz-Oberstadt	Gymmasium Schiller School Hannover	Max-Planck Gymnasium Düsseldorf	Oskar-von-Miller Gymnasium München	Karl-Gymnasium Stuttgart
High-school GPA	1.3	1.7	2.0	2.3	2.7	3.0	3.3
College type*	Top-ranked	Non top-ranked					
Top-ranked universities	University of Mannheim	RWTH Aachen	University of Munich	University of Cologne	University of Frankfurt		
Non top-ranked universities	University of Trier	University of Greifswald	University of Siegen	University Leuphana Lüneburg	University of Bremen		
College GPA	1.3	1.7	2.0	2.3	2.7	3.0	3.3
Internship firm	Windmöller & Hölscher, Lengerich	Amann Group, Bönnigheim	FACT, Münster	MVI Proplant, Wolfsburg	Astaro GmbH & Co. KG, Karlsruhe		
Internship business area*	Accounting	Controlling	Sales				
Internship length	1 month	3 months	5 months				
English	fluent	very good	basic				
2nd foreign language	Spanish (basic)	French (basic)					
IT skills	Microsoft Office	Microsoft Office, HTML and Dreamweaver					
Sports	single	team					
Single sports	swimming	cycling	running				
Team sports	handball	volleyall	basketball	football			
Volunteering: "social"	neighbourhood help: youth and senior group, German language classes				mobile care services: senior and domestic care		
Volunteering: "non-social"	volunteering preservation of monuments work				online volunteering at UN: IT work and translations		

Note: This table shows all values of all CV elements that were randomized. * denotes elements that are fixed within CV pairs. School and college GPAs range from 1.3 (very good) to 3.3 (satisfactory); for the analysis grades are converted to points (4-grade). Top-ranked universities according to the "2015 CHE Hochschulranking" in Undergraduate Business Administration. Each resume contains either two single sports or two team sports. In half of the resumes, there is socially-interactive volunteering, in the other half, there is non-socially-interactive volunteering.

Table A2.3 : Sample Representativeness: Comparing Respondents and Non-Respondents from the ifo Personnel Manager Survey Database

	Respondents	Non-respondents	p-value
Assigned sample (secondary school=1 / college=2)	1.472	1.469	0.932
Share female ≤ 20	0.185	0.187	0.930
Industry			0.680
Employees	383.02	424.22	0.725
State			0.578
Baden-Württemberg	0.145	0.150	
Bavaria	0.242	0.195	
Berlin	0.009	0.023	
Brandenburg	0.019	0.023	
Bremen	0.007	0.011	
Hamburg	0.021	0.024	
Hesse	0.073	0.079	
Mecklenburg-West Pomerania	0.019	0.013	
Lower Saxony	0.083	0.095	
North Rhine-Westphalia	0.181	0.178	
Rhineland-Palatinate	0.043	0.040	
Saarland	0.012	0.009	
Saxony	0.074	0.066	
Saxony Anhalt	0.021	0.028	
Schleswig-Holstein	0.026	0.032	
Thuringia	0.026	0.036	
N (HR managers)	579	927	Total: 1506

Note: Means and p-values from two-sided t-tests comparing respondents and non-respondents from the ifo Personnel Manager Survey Database. For “industry” and “state” distributions, both groups are compared using a two-sample Kolmogorov-Smirnov test (means are not reported for these categorical variables). Means for the German states represent the shares of firms from the respective state.

Table A2.4 : Invitation Rate by First and Last Names

	Secondary-school graduate sample			College-graduate sample		
Male first names	mean	p-value	N (CVs)	mean	p-value	N (CVs)
Alexander	0.408	0.039	125	0.558	0.241	104
Christian	0.487	0.750	156	0.439	0.272	82
Daniel	0.561	0.227	98	0.582	0.106	98
Patrick	0.496	0.931	133	0.461	0.431	102
Tobias	0.522	0.559	186	0.447	0.305	94
Female first names						
Anna	0.455	0.397	88	0.531	0.454	143
Julia	0.554	0.239	121	0.463	0.419	123
Katharina	0.480	0.657	125	0.510	0.844	102
Laura	0.495	0.918	93	0.547	0.272	139
Sarah	0.495	0.922	103	0.426	0.136	101
Last names						
Becker	0.480	0.525	246	0.463	0.275	214
Fischer	0.482	0.554	284	0.559	0.091	202
Mayer	0.538	0.217	262	0.589	0.005	241
Schneider	0.474	0.428	228	0.403	0.003	226
Weber	0.505	0.890	208	0.483	0.626	205

Note: Mean represents the mean of the outcome of being selected for an interview. P-values stem from two-sided tests whether the mean of the outcome equals 0.5.

3 Does the Education Level of Refugees Affect Natives' Attitudes?*

3.1 Introduction

In 2014 and 2015, Europe experienced an unprecedented influx of refugees.[1] In 2015 alone, more than 1.5 million individuals applied for asylum in Europe, with Germany registering the highest number of some 440,000 applications (Eurostat, 2016).[2] These refugee movements were exceptional not only in terms of magnitude, but also in terms of refugees' origin countries: As Syria, Afghanistan, and Iraq constitute the main source countries, these refugees are perceived as culturally more distinct than those seeking asylum during previous refugee waves, such as refugees from the Balkan countries in the 1990s (see Dustmann et al., 2017). Against this background, European politicians face a challenge when implementing and enforcing asylum policies. On the one hand, these policies have to comply with international commitments, such as the 1951 Geneva Convention for Refugees or the Dublin Convention.[3] On the other hand, it is crucial that refugee policies are supported by domestic voters in order to successfully implement these policies and to preserve solidarity with refugees. The fact that public support for anti-immigration parties increased markedly in several European countries during the refugee crisis suggests that voters' scepticism towards refugees and national asylum policies have not been fully appreciated by policy makers.[4] De-

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1 Throughout the chapter, we use the term "refugee" as a collective term for all persons who seek refuge in another country, independent of their legal status. We thereby follow the public discourse in Germany, in which the migration inflow from 2014 onward has generally been referred to as "Flüchtlingskrise" (refugee crisis) by politicians, the media, and the general public.

2 The Federal Ministry of Internal Affairs registered a total of more than 1.1 million refugees entering Germany in 2015 (Bundesministerium des Inneren, 2016).

3 The Geneva Convention broadly defines the rights of refugees and the obligations of hosting countries. The Dublin Convention, which came into force in 1997/98, established the principle that the EU member state through which an asylum seeker first enters the EU is responsible for processing the asylum claim (see Dustmann et al., 2017).

4 Electoral outcomes that have largely been attributed to voters' rising anti-immigration sentiments include the "Brexit" referendum in the United Kingdom (Bansak, Hainmueller and Hangartner, 2016) and the success of the right-wing populist party "Alternative für Deutschland" (AfD) in Germany. The AfD won significant vote shares in several state elections, including the 2016 state election in Mecklenburg-West Pomerania in which it outperformed Chancellor Merkel's "Christlich Demokratische Union" (CDU) in Merkel's home state (21 percent versus 19 percent). In the German federal election in September 2017, the AfD received 13 percent of the votes, which made it the third-largest party in the German Bundestag.

spite the importance of public attitudes towards refugees, little is known about the determinants of these attitudes and whether they depend on the characteristics of refugees.

In this chapter, we study whether attitudes towards refugees are affected by beliefs about refugees' education level. To do so, we implemented online survey experiments with more than 5,000 students at universities in Germany. To estimate a causal effect of education beliefs on attitudes, we exogenously shifted respondents' beliefs by randomly providing information on refugees' education level.

The focus on refugees' education level, one specific characteristic of refugees, allows us to test two economic theories on how immigrants' skill level shapes natives' attitudes towards them (see Hainmueller and Hiscox, 2010) in the context of the European refugee crisis: The *labour market competition model* predicts that natives will be most opposed to immigrants whose skills are similar to their own since these immigrants might be competitors on the labour market (see also Haaland and Roth, 2017). This model therefore predicts that university students, the participants in our surveys, are more opposed to refugees if they believe refugees to be well-educated. The *fiscal burden model*, on the other hand, predicts that natives in general are more opposed to low-skilled immigrants because they impose larger fiscal burdens on the public than high-skilled immigrants. In contrast to the labour market competition model, the fiscal burden model thus predicts that university students are more opposed to refugees if they believe refugees to be *low*-educated.[5] Besides testing these two economic theories, our focus on refugees' education level (instead of other refugee characteristics) has also been shaped by the political debates at the time we conducted our survey, which was after the large refugee influx from 2015 slacked off. At that time, the public debate had started to focus on how to integrate the large number of refugees; obviously, the education level of refugees was central in this debate.

In the context of this study, university students are an interesting and highly relevant focus group for at least two reasons. First, in contrast to low-skilled natives, the two economic theories make opposing predictions for the effect of education beliefs on the attitudes of university students, which allows us to test the relevance of these two models. Second, university students constitute an important part of the electorate be-

[5] While refugees typically do not migrate for economic reasons, they often stay in the host countries for longer periods, making labour market integration an important challenge. Since labour market integration is considered an important step for the general integration into the host country, refugees in Germany are entitled to work once their asylum has been granted. Since many individuals applied for asylum in Germany, this implies a considerable number of refugees entering the labour market. In June 2017, for example, 10 percent of all unemployed persons seeking work in Germany were refugees (Degler, Liebig and Senner, 2017).

cause their voter turnout is traditionally higher than that of other voter groups (*e.g.,* Schäfer, Vehrkamp and Gagné, 2013). To put our findings into perspective, we provide complementary evidence from the ifo Education Survey 2016, an opinion survey representative of the German adult population, on differences in beliefs about refugees' education level between university students and other groups of the population (see Section 3.5).

For implementing the information treatment, we exploit the fact that, at the time of our survey, the information on refugees' education level[6] discussed in German media seemed to contradict itself. Due to the uncertainty regarding refugees' education level, we were able to provide opposing, equally credible, information on the education level of refugees in Germany. In particular, in our main survey, we randomly assigned survey participants to one of three experimental groups: The control group did not receive any information on the education level of refugees. Respondents in the *High Skilled* treatment were informed about a study that finds that refugees are rather well-educated (see UNHCR, 2015).[7] In the *Low Skilled* treatment, we induced the opposite beliefs by informing participants about a different study that finds that refugees are rather low-educated (see Woessmann, 2016). To assess the robustness and replicability of our main results, we conducted a follow-up survey experiment in 2017, using a new sample of more than 500 university students and a different information treatment, which relied on newly available evidence on the education level of refugees.

We find that the information treatments strongly shift respondents' beliefs about refugees' education level in the expected directions. In the follow-up survey, using an alternative information treatment, we replicate these effects and show that the shift in beliefs persists until one week later. Using the exogenous shift in respondents' beliefs about refugees' education as the first stage in an instrumental-variable approach, we find that beliefs about refugees' education level affect natives' concerns about labour market competition. This finding is in line with the predictions of the labour market

[6] We use the singular form education level to imply the average education level of refugees in Germany. Of course, the education level may vary considerably across individuals.

[7] During 2015, information that refugees are rather well-educated was widespread in German media. For example, newspaper articles discussed the contended high level of education of refugees: https://www.stern.de/politik/deutschland/so-alt-und-gebildet-sind-asylbewerber-in-deutschland-6473632.htmlb [accessed December 1, 2017]; https://www.welt.de/politik/ausland/article149755032/Syrische-Fluechtlinge-ueberdurchschnittlich-gebildet.html [accessed December 1, 2017]. Relatedly, media reports suggested that many refugees were academics, such as doctors or engineers (*e.g.,* https://www.taz.de/!5021964/ [accessed December 1, 2017]). See Section 3.2 for a detailed discussion on refugees' education level and media reports thereof.

competition model. In contrast, we find no effects on fiscal burden concerns or other concerns such as increasing crime levels.[8]

Despite a strong correlation between beliefs about refugees' education level and attitudes, we do not find any evidence that education beliefs affect attitudes toward refugees in a causal way. These precisely estimated zero effects suggest that economic aspects, such as labour market competition concerns, are rather unimportant for shaping attitudes toward refugees in our sample. To empirically explore the (missing) link between labour market competition concerns and attitudes, we investigate the importance respondents attribute to various aspects when forming their attitudes toward refugees. Two clear patterns emerge: First, providing information about refugees' education level only increases the importance of economic aspects, but not the importance of other aspects, such as humanitarian aspects. Second, when respondents form their attitudes toward refugees, economic aspects are relatively unimportant. This result is consistent with the existing literature on attitude formation toward immigrants, which suggests that non-economic aspects are more important than economic aspects (*e.g.,* Card, Dustmann and Preston, 2012; Dustmann et al., 2017; Hainmueller and Hiscox, 2010).

Several robustness checks indicate that our results are not driven by different types of biases in respondents' answering behaviour. In particular, in the follow-up survey, we used the item count technique (ICT) to assess whether survey answers are biased by respondents' desire to provide socially desirable answers (see *e.g.,* Coffman, Coffman and Ericson, 2017). We find little evidence of social desirability bias. Furthermore, the persistence of treatment effects on beliefs about refugees' education level, as well as the pattern of heterogeneous treatment effects by respondents' *baseline* beliefs, suggest that our information treatment effects are not driven by experimenter demand effects or priming effects.

This chapter contributes to several strands of economic research. It is related to the literature on attitudes toward immigration (*e.g.,* Card, Dustmann and Preston, 2012; Dustmann and Preston, 2007; Facchini and Mayda, 2008; O'Rourke and Sinnott, 2006), in particular to those studies that use survey experiments. For example, Grigorieff, Roth and Ubfal (2016) show that randomly provided information about immigration, such as the share of immigrants in the population and immigrants' unemployment or incarceration rates, yields more favourable attitudes toward immigrants, but does not affect policy preferences. Similarly, Alesina, Stantcheva and Teso (2018) study how information about the true share, the origin, and the work ethic of immigrants affects

[8] We also present reduced-form estimates, which corroborate our instrumental-variable results.

natives' preferences for redistribution. They find a negative effect of priming people to think about immigrants on demand for redistribution, which dominates the effects of their information treatments. Hainmueller and Hiscox (2010) study experimentally how concerns about labour market competition and about the fiscal burden on public services shape attitudes toward high- and low-skilled migration. They find no support for the labour market competition model or the fiscal burden model in their data. Haaland and Roth (2017) investigate whether beliefs about labour market impacts of immigration affect the support for immigration. They find that respondents report more support for immigration when being provided (research-based) evidence that immigration has no adverse effects on natives' wages. To our knowledge, ours is the first study that studies the relevance of the labour market competition model and the fiscal burden model in the context of the European refugee crisis.

While the literature on natives' attitudes towards immigration is well developed, evidence on what determines attitudes towards refugees is scarce.[9] The study most closely related to ours is the survey experiment by Bansak, Hainmueller and Hangartner (2016). The authors asked 18,000 eligible voters in 15 European countries to evaluate different profiles of refugees that varied experimentally across nine broad domains. They find that refugees are more likely to be accepted if they worked in higher-skilled occupations in their home country, have more consistent asylum testimonies and higher vulnerability, and are Christians (rather than Muslims). In a related survey experiment, Bansak, Hainmueller and Hangartner (2017) show that European citizens support a proportional allocation of asylum seekers across countries.[10] To our knowledge, we are the first to provide an in-depth analysis of the causal effect of refugees' education level on natives' attitudes. More generally, this chapter contributes to the growing literature that studies the causal effects of information provision on survey respondents' attitudes and preferences in various domains.[11]

The rest of the chapter is structured as follows. In Section 3.2, we describe the labour market competition model and the fiscal burden model. We discuss the challenges of measuring refugees' education level and present the studies that we used for our information treatments. In Section 3.3, we describe our opinion surveys and the experimental design. In Section 3.4, we present the results, including evidence that respon-

[9] Also note that most surveys cited above were conducted before the massive refugee influx in 2014/2015.
[10] Further recent studies on natives' attitudes in the context of the European refugee crisis include Steinmayr (2016), who investigates how the exposure to refugees affects voting behaviour in Austria, and Jeworrek, Leisen and Mertins (2017), who study whether telling survey respondents about the possibility that refugees support the local population with volunteering activities affects natives' support for integrating refugees.
[11] See, for instance, Cruces, Perez-Truglia and Tetaz (2013); Elias, Lacetera and Macis (2015); Kuziemko et al. (2015); Wiswall and Zafar (2015); Lergetporer et al. (2016); Bursztyn (2016) and **?**.

dents' answers are not driven by different types of biases such as social desirability bias. Section 3.5 discusses our findings and concludes.

3.2 Theoretical Framework and Evidence on Refugees' Education Level

While refugees typically do not intend to stay permanently, their integration in the host country is nevertheless an important issue since many refugees have only few prospects of returning to their country in the near future (Woessmann, 2016). The success of refugees' integration critically depends on their successful integration into the labour market (Degler, Liebig and Senner, 2017), which is also economically desirable since working refugees typically do not depend on government aid. For these reasons, refugees in Germany are allowed to work once asylum has been granted.[12] In general, policy makers may be more likely to implement successful integration policies when they possess accurate information on the skill level of refugees and when natives have positive attitudes towards refugees.

Economic theories on natives' attitudes towards immigrants

The increasing success of anti-immigration parties in Europe, including the AfD in Germany, during recent years suggests widespread hostile attitudes towards immigration and/or refugees. Thus, natives' attitudes towards immigration might be a key obstacle to the implementation of integration policies as well as for accepting new immigrants and refugees. Economic models on attitudes towards immigration emphasize the importance of migrants' education level and natives' beliefs thereof.

Hainmueller and Hiscox (2010) discuss two competing theories on how the skill level of immigrants affects natives' attitudes toward them. According to the labour market competition model, natives are most opposed to immigrants with a skill level similar to their own because they expect these immigrants to compete for the same types of jobs (*e.g.,* Mayda, 2006; Scheve and Slaughter, 2001). Since our sample of university students will fall in the upper tail of the skill distribution of workers,[13] the labour

[12] Furthermore, it has been argued that refugees would alleviate the shortage of skilled workers. For example, in September 2015, Dieter Zetsche (Chairman of Daimler), comparing refugees to guest workers who came to Germany in the 1950s and 1960s, claimed that refugees could help to create a new "German economic miracle" (Die Zeit, August 18, 2016, http://www.zeit.de/2016/35/fluechtling-arbeitsmarkt-buerokratie-unternehmen-versprechen [accessed December 1, 2017]).

[13] Only 21 percent of the German population holds a university degree (Brücker, Rother and Schupp, 2016). Note that the share of university-educated adults is lower in Germany compared to other OECD countries because of the extensive dual vocational education system in Germany.

market competition model predicts that our survey participants have more negative attitudes toward refugees when they believe that refugees are highly educated (and thus potential competitors on the labour market). In contrast, the fiscal burden model predicts that respondents are more opposed to low-skilled immigration because low-skilled immigrants (by assumption) impose net burdens on public finance whereas high-skilled immigrants are net contributors.[14]

This study tests these two competing theories in the context of the European refugee crisis. In particular, we test whether shifting respondents' beliefs about refugees' education level upward (*i.e.*, toward a higher education level) (*i*) increases concerns about competition on the labour market (hypothesis 1); (*ii*) decreases concerns that refugees impose fiscal burdens on public services (hypothesis 2); and (*iii*) affects general attitudes toward refugees (hypothesis 3). Of course, beliefs about refugees' education may affect general attitudes not only because of labour market competition concerns and fiscal concerns (*e.g.,* Bauer, Lofstrom and Zimmermann, 2000; Dustmann and Preston, 2007). Therefore, we also assess the relevance of alternative concerns such as increasing crime levels.

The education level of refugees in Germany

The successful integration of refugees into the labour market of the host country may substantially depend on their skills.[15] A major challenge in this context is the large degree of uncertainty regarding refugees' formal education, which provides information on their professional skills. The large inflow of refugees during the years 2014 and 2015 posed an enormous administrative challenge to register arriving refugees and an even larger challenge to document their educational degrees. Particular problems arise due to missing verifiable credentials, such as graduation certificates, and because educational degrees from the refugees' home countries are often hardly comparable with German educational degrees (see Brücker, Rother and Schupp, 2016; Woessmann, 2016).

[14] In particular, the model predicts that richer (poorer) natives are more opposed to low-skilled immigration if the government balances its budget by changing tax rates (by changing per capita transfers) in response to increased spending on immigrants. Therefore, we measured respondents' concerns about (i) the need for tax increases and about (ii) lower levels of government benefits because of government spending on refugees (see Section 3.3.1). While we refrain from making assumptions about how the German government might finance spending increases on refugees, note that university graduates in Germany will on average have relatively high future earnings (OECD, 2016). This implies that they should be more concerned about tax increases than about cuts in government transfers if they believe that refugees' education level is low.

[15] Note that, from a legal perspective, granting prosecuted individuals temporary refugee status is a humanitarian act that is independent of economic considerations and independent of the asylum seeker's education level (Dustmann et al., 2017).

As a consequence of these difficulties, studies that aim at quantifying the education or skill level of refugees have produced seemingly contradictory findings. One of the first assessments of refugees' education level is the UNHCR study on Syrian refugees (UNHCR, 2015). The study draws a positive picture of refugees' education level since it finds that 43 percent of adult Syrian refugees report to have some university education and an additional 43 percent report to have completed secondary education (UNHCR, 2015).[16] These data were collected by UNHCR border protection teams who conducted interviews among a non-random sample of Syrian asylum seekers in various locations in Greece.[17] Since the majority of interviewees (50 percent) intended to request asylum in Germany, the findings of this study have been interpreted as a proxy for the education level of asylum seekers in Germany (von Redetzky and Stoewe, 2016).[18]

In contrast to the UNHCR study, Woessmann (2016) draws a negative picture of refugees' education level. Comparing multiple data sources (*e.g.,* the German Microcensus and the IAB-SOEP Migration Sample), the author finds that only about 10 percent of asylum seekers in Germany have a university degree and two-thirds do not have any type of professional qualification. Moreover, using data from the Trends in International Mathematics and Science Study (TIMSS) in 2011 (before the Syrian civil war started), Woessmann (2016) finds that 65 percent of Syrian 8th-grade students fail to achieve the most basic proficiency level in mathematics and in science. Compared to German 8th-grade students, the achievement gap amounts to 4-5 years of schooling.[19]

We used these two studies for the two information treatments in our main survey to exogenously shift respondents' beliefs about refugees' education level. The fact that the two studies reach contradicting conclusions allows us to implement symmetric information treatments: One treatment tends to shift beliefs about refugees' education level upward, whereas the other treatment tends to shift beliefs downward. These two studies, UNHCR (2015) and Woessmann (2016), received considerable media at-

[16] The UNHCR interprets its findings on the education level of Syrian refugees as follows: "Overall, the profile is of a highly-skilled population on the move." (UNHCR, 8 December 2015, http://www.unhcr.org/news/latest/2015/12/5666ddda6/unhcr-says-syrians-arriving-greece-students.html [accessed 1 December, 2017]).

[17] These Syrian asylum seekers arrived in Greece between April and September 2015. The authors of the study note that the interviews were voluntary and interviewees were not required to verify their statements with credentials.

[18] See Buber-Ennser et al. (2016) for a similar interview study with asylum seekers in Austria.

[19] The TIMSS results should be viewed as an approximation of the skill level of refugees in Germany. First, while Syria is the most relevant source country of refugees in Germany, refugees also come from other countries. Second, regarding the skill level, it is unclear to what extent Syrian refugees in Germany are a selected group of Syrians.

tention and were, to our knowledge, the most convincing academic assessments of refugees' education level at the time of our main survey. More recent evidence on refugees' education level from the IAB-BAMF-SOEP Survey of Refugees in Germany was released only in late 2016, after our main survey had been conducted. This study finds that 32 percent of asylum seekers in Germany aged 18 years and older have a high school degree and 13 percent hold a university degree (see Brücker, Rother and Schupp, 2016). We used this alternative, and more recent, information on refugees' education level in the follow-up survey (conducted in June/July 2017) to assess the robustness and replicability of our main findings.

3.3 Survey Design, Information Treatment, and Empirical Model

3.3.1 Main Survey Experiment

General framework

To implement the main survey experiment, we ran an online survey with 4,901 students from four large German universities (Technical University of Dresden, University of Munich, University of Konstanz, and Technical University of Chemnitz). We obtained access to the universities' mailing lists and invited students to participate in a "short opinion survey on refugees" via email. The email informed students that the survey would take about 5 minutes, participants would have the chance to win Amazon gift vouchers after survey completion, and that the survey would be anonymous.[20] The survey was conducted using the software Qualtrics (Qualtrics, Provo, UT), and the field time was from June to August 2016.

As is typical for experiments in economics, our study relies on a self-selected sample of university students. Appendix Table A3.1 compares basic characteristics of students in our sample (share of females, share of non-Germans, and faculty) with official administrative student statistics from the two larger universities. While we do not claim to have a sample that is representative of students in Germany, Appendix Table A3.1 shows that our sample closely resembles the student populations at the respective university in terms of gender and faculty composition. Non-German students are under-represented in our sample because the survey was conducted in German.

[20] We were able to guarantee anonymity and simultaneously offer the chance to win Amazon gift vouchers (which were delivered via email) because survey answers were saved in a different file than email addresses. This was known to all respondents before the start of the survey. Furthermore, the survey software prevented respondents from participating in the survey with the same computer more than once.

Survey Questions

We designed the survey questions to measure respondents' (i) beliefs about refugees' education level, (ii) labour market competition concerns, (iii) fiscal burden concerns, (iv) other concerns related to refugees (such as increasing crime), (v) general attitudes towards refugees, and (vi) aspects that shape respondents' attitude towards refugees, such as economic consideration . Appendix Table A3.18 contains the wording and the answer categories of all questions in the main survey (translated into English).[21]

Beliefs about refugees' education level. To assess whether the information treatments (described below) indeed shift beliefs about refugees' education level in the intended directions, we asked respondents to indicate their belief about refugees' education level after randomly providing the information on refugees' education level. The effects of the information treatments on the education beliefs constitute the first stage of our instrumental-variables (IV) estimation strategy (see Section 3.3.3).

Labour market competition, fiscal burden, and other concerns. To assess the relevance of the labour market competition model, we elicited concerns that refugees increase labour market competition for both the respondent personally and in general. To assess the relevance of the fiscal burden model, we measured concerns about (i) fiscal revenues and costs, (ii) lower levels of government benefits due to spending on refugees, and (iii) the need for tax increases. To capture other potential channels through which natives' beliefs about refugees' education level might affect attitudes, we elicited additional economic and non-economic concerns (*e.g.,* increased crime) and statements about refugees.

General attitudes towards refugees. Ultimately, we are interested in how natives' beliefs about the education level of refugees translate into general attitudes towards them. To measure general attitudes, we asked respondents whether (i) Germany should admit more or less refugees in the future; whether (ii) the number of refugees that Germany admitted last year was too high or too low; and whether (iii) refugees should be allowed to stay in Germany permanently.

Aspects shaping respondents' attitudes. Finally, we asked respondents about the importance of six different aspects for forming their attitudes towards refugees: human-

[21] Like many other recent economics papers using survey experiments, our outcomes of interest are self-reported attitudes and policy preferences (*e.g.* Alesina, Stantcheva and Teso, 2018; Karadja, Mollerstrom and Seim, 2017; Kuziemko et al., 2015). Importantly, recent survey experiments corroborate the relevance of self-reported attitudes toward migration by showing that they correspond closely to actual political behaviour, such as the probability of signing an online petition or donating to charity (*e.g.* Grigorieff, Roth and Ubfal, 2016; Haaland and Roth, 2017; Alesina, Miano and Stantcheva, 2018).

itarian aspects, economic aspects, refugees' willingness to integrate, religion/culture of refugees, refugees' criminal behaviour, and personal experience with refugees. We included this question for two purposes: First, it allows us to investigate which aspects of attitude formation become more, or less, important when respondents' beliefs about refugees' education level are changed. Second, comparing the relative importance of the various aspects helps understanding the channels through which education beliefs affect general attitudes. At the end of the survey, we elicited a set of demographic characteristics, including respondents' migration and family background, as well as refugee-related information, such as personal experience with refugees, and labour-market-related information, such as expected future earnings.

To avoid the risk that general attitudes towards refugees are contaminated by priming respondents beforehand with refugee-related statements, we first elicited respondents' general attitudes, then their beliefs about refugees' education level, followed by specific concerns (labour market competition, fiscal burden, and others) and aspects shaping respondents' attitudes. Note that respondents were not able to return to earlier questions to revise earlier answers. On each screen, except for the final questions on demographic characteristics, we randomized the order of questions to avoid question order effects.

Summary indices. We combine answers to individual questions to create four summary indices: a summary index for general attitudes, for labour market competition, for fiscal-burden, and for other concerns/statements, respectively. Each of these four indices is created in three steps: First, we demean the outcomes of all individual questions (concerns are coded from 1="completely disagree" to 5="completely agree"; general attitudes are coded from 1=very negative attitude to 5=very positive attitude). Second, we standardize the demeaned outcomes of all individual questions by dividing by its standard deviation. Third, we compute the mean across the standardized items that enter a specific summary index. The advantage of using summary indices is their robustness to over-testing because only few indices are used. Another advantage is that measurement error is reduced if measurement error is not perfectly correlated across individual items (see also Anderson, 2008).

Information treatments

To identify a causal effect of beliefs about refugees' education level on attitudes towards them, we randomly assigned respondents to one of three groups (control group, treatment *High Skilled*, and treatment *Low Skilled*) that differed by the type of information on refugees' education level they were provided at the beginning of the survey.

Control group. Participants in the control group were shown the following text: "With this survey, we would like to learn about your opinion on refugees. Please think of the current refugee situation in Germany when answering the survey." Note that this text does not contain any information about refugees' education level.

Treatment High Skilled. Participants in this group were given the following information:"With this survey, we would like to learn about your opinion on refugees. Please think of the current refugee situation in Germany when answering the survey. In this context, a study has found that the education level of refugees is rather high since 43 percent of the refugees from Syria have attended a university." The information on refugees' education level in this treatment is based on the UNHCR (2015) study (see Section 3.2).

Treatment Low Skilled. Participants in this group were given the following information: "With this survey, we would like to learn about your opinion on refugees. Please think of the current refugee situation in Germany when answering the survey. In this context, a study has found that the education level of refugees is rather low because 65 percent of the school students in Syria do not reach the basic level of academic competencies." The information on refugees' education level in this treatment is based on the Woessmann (2016) study (see Section 3.2).

Note that we did not deceive our participants since the information provided reflects the interpretation of the authors of the two studies (and is not our interpretation of their results).[22]

3.3.2 Follow-up Survey Experiment

The information on refugees' education level provided in the main survey has two potential drawbacks: First, due to the lack of available data for other source countries, the provided information only refers to refugees from Syria, the major source country of refugees in Germany. Second, the information provided not only includes the study results *(i.e.,* 43 percent university attendance rate versus 65 percent of 8th-grade students lacking basic academic competencies), but also reflects the interpretations of the respective authors (*i.e.,* refugees are rather highly educated versus low-educated). While the interpretations of authors are typically provided when study results are disseminated by the media, explicitly incorporating authors' interpretations in our infor-

[22] Note that providing information on results from specific academic studies is not unusual for information experiments. See, for instance, Haaland and Roth (2017), who inform their survey respondents about Card's (1999) results on the Mariel Boatlift, explicitly choosing a study with a non-negative finding on the impact of immigration on natives.

mation treatments may trigger experimenter demand effects. To address this issue and to assess the robustness of the findings from the main survey experiment, we conducted a follow-up experiment on a new sample of university students one year after the main survey.

The follow-up survey experiment, conducted in June and July 2017, had a similar general set-up as the main survey experiment. The 582 respondents[23] were randomized into two experimental groups (control group and treatment Information). The follow-up survey, which repeated a subset of six questions from the main survey,[24] was designed to address three issues:

First, it investigates the robustness of our main findings to using an alternative information treatment. The information on refugees' education level was based on a recently published study, the *IAB-BAMF-SOEP Survey of Refugees in Germany* (see Section 3.2 for details). The text in the information treatment reads as follows: *"With this survey, we would like to learn about your opinion on refugees. Please think of the current refugee situation in Germany when answering the survey. In this context, a study has found that 32 percent of adult refugees have a high school degree; the respective share among the German population is 29 percent. 13 percent of refugees hold a university degree; the respective share among the German population is 21 percent"* (see Brücker, Rother and Schupp, 2016).[25] We supplemented this text information with a graphical depiction (see Appendix Figure A3.1). Note that the direction in which this information treatment shifts respondents' beliefs about refugees' education level (if any) is unclear a priori, since it depends on respondents' initial beliefs about the statistics provided (i.e., high school and university completion rates of refugees versus those of Germans). In Section 3.4.2, we show that this information treatment shifted beliefs about refugees' education level upward.

Second, the follow-up survey investigates the persistence of the shift in respondents' beliefs about refugees' education level that is triggered by the information treatment. To this end, we invited all respondents of the follow-up survey to participate in a re-

[23] The follow-up survey was conducted with students from the University of Munich, the University of Konstanz, the Technical University of Chemnitz, and the Catholic University of Eichstätt-Ingolstadt. To identify respondents who had already participated in the main survey one year earlier, we included a screening question. We excluded 12 respondents who reported having already participated in our main survey. Including them in the sample does not change the results.

[24] The following questions were asked again: beliefs about refugees' education level; labour market competition concerns (both questions: "for me personally" and "in general"); concern about fiscal revenues and costs; and two aspects governing opinion formation process (humanitarian aspects and economic aspects).

[25] As with the information provided in the main survey experiment, we remain agnostic about the accuracy of these study results and merely use them as an alternative information treatment.

survey about one week later, which again elicited respondents' beliefs about refugees' education level. We also asked respondents to estimate the share of refugees with a high school degree and university degree, respectively, *i.e.,* the information provided in the *Information* treatment one week before. Out of the 582 respondents to the first survey, 292 (50 percent) participated in the re-survey.[26]

Third, since some questions on attitudes toward refugees might be sensitive questions, we used the item count technique (ICT) to assess the extent of social desirability bias in the questions on labour market competition and fiscal burden concerns as well as aspects of attitude formation (see also Coffman, Coffman and Ericson, 2017). The ICT provides a "veil" of anonymity for sensitive questions that reduces the risk of biases through socially desirable answers. Appendix A3.2 provides a detailed description of the item count technique.

3.3.3 Empirical Model

To estimate the impact of respondents' beliefs about refugees' education level on their attitudes towards refugees, we use an instrumental-variables (IV) strategy. In the first stage, we instrument the belief of respondent i about refugees' education level with the randomly assigned information treatment indicators:

$$BeliefEducationLevel_i = \alpha_0 + \alpha_1 HighSkilled_i + \alpha_2 LowSkilled_i + \delta' X_i + \mu_i + \epsilon_i. \tag{3.1}$$

where $HighSkilled_i$ and $LowSkilled_i$ are binary treatment indicators.[27] X_i is a vector of control variables, including the respondents' demographic characteristics. Importantly, we include fixed effects for university*faculty combinations (μ_i) such that we effectively compare only students in the same faculty in the same university.[28] ϵ_i is the idiosyncratic error term. Since treatment *High Skilled* tends to shift respondents' beliefs about refugees' education level upward, while treatment *Low Skilled* tends to shift them downward„ including both instruments should yield a strong first stage. In the analysis of the follow-up survey, we instrument respondents' education beliefs

[26] This rate is comparable to other recent studies: For instance, take-up in the follow-up surveys was 14 percent in Alesina, Stantcheva and Teso (2018) and Kuziemko et al. (2015), and 66 percent in Haaland and Roth (2017).
[27] Results are very similar when we instead use two binary treatment indicators for treatment *Low Skilled* and for treatment *High Skilled*.
[28] Across the four universities, there are 11 different faculties in total. Given that not all faculties are represented in each university (or in our sample), our sample contains 24 faculty*university cells.

with a binary indicator for whether respondents have been assigned to the information treatment.

In the second stage, we regress the respective outcome of interest (y_i) on the predicted education beliefs of the first stage:

$$y_i = \beta_0 + \beta_1 Belief\widehat{Education Level}_i + \delta' X_i + \mu_i + \epsilon_i, \tag{3.2}$$

Our coefficient of interest is β_1, which gives us the local average treatment effect (LATE). Identification of β_1 relies on the untestable assumption that the information treatments affect our outcomes of interest only through their effects on respondents' beliefs about refugees' education level. In Section 3.4.6, we provide evidence that suggests the validity of this exclusion restriction and show that our results hold when estimating reduced-form effects instead.

3.3.4 Balancing Test

To test whether the randomization balanced the socio-demographic characteristics of respondents across experimental groups in the main survey, we compare the characteristics of respondents in the control group with respondents in the two treatment groups (Table 3.1). We find statistically significant (at the 5 percent level), but small, differences (at the 5 percent level) in only six out of 90 pairwise comparisons; (see columns 2 to 4); six additional coefficients are very small and only marginally significant at the 10 percent level. Interestingly, note that some of the statistically significant differences go in the same direction for the *High Skilled* and *Low Skilled* treatment. For example, the share of parents without college degree is slightly lower in both information treatment groups compared to the control group. This implies that only few characteristics differ statistically significantly between treatment *High Skilled* and treatment *Low Skilled*. Overall, while some differences exist between the control group and treatment groups, they seem to emerge for random, and not systematic, reasons. In line with this interpretation, the information treatment has – as expected – opposite effects on respondents' education beliefs in the High Skilled treatment and Low Skilled treatment (see Section 3.4.2). In our regression analyses, we control for all characteristics reported in Table 3.1.

Since the High Skilled and Low Skilled samples are slightly smaller than the control group sample (by 4 and 2 percent, respectively), selection into survey participation might be a threat to internal validity. If the information treatments decreased respondents' likelihood to finish the survey, then differences in answers across experimen-

tal groups might be driven by attrition rather than by the information provided. To test for non-random attrition, we compare the shares of participants who have been assigned to a treatment group and subsequently completed the survey (see second last row of Table 3.1). Reassuringly, survey completion rates do not differ across treatment groups, indicating that the lower numbers of observations in the information treatments are due to pure chance and that our estimates are internally valid.[29] Appendix Table A3.2 shows that characteristics are also well balanced between control and treatment groups in the follow-up survey (only one out of 30 differences is statistically significant at the 5 percent level) conventional levels.

3.4 Results

3.4.1 Correlation Between Attitudes, Beliefs About Refugees' Education, and Respondents' Socio-Demographic Characteristics

Using the control group, Appendix Table A3.3 presents bivariate correlations between beliefs about refugees' education level and general attitudes towards them.[30] Respondents with more positive beliefs about refugees' education level also have more positive attitudes towards refugees. This is true for the summary index of general attitudes as well as for the three individual items it comprises. In Section 3.4.4, we analyse whether these correlations represent a causal effect of education beliefs on attitudes. In Appendix Tables A3.4 and A3.5, we investigate how respondents' socio-demographic characteristics are related to attitudes towards refugees and to beliefs about refugees' education level, respectively.[31] Overall, males are more sceptical towards refugees, whereas students who spoke to refugees and students who receive

[29] Note that Table 3.1 compares respondents who are included in our analysis sample. Several participants had to be excluded for the analysis: First, we excluded all individuals (482 persons) who clicked on the survey link, but terminated the survey before having been assigned to an information treatment. Second, we excluded 524 participants who answered only the four general attitude questions on the first screen, but nothing else. Third, we excluded 414 participants aged 40 years and older since it is unlikely that these persons are regular students. Fourth, and similarly, we excluded 47 participants who reported that they were not studying (*e.g.,* guest auditors). Finally, we excluded one participant whose comments at the end of the survey suggested that he or she did not answer the survey truthfully. In the full sample (*i.e.,* before applying these sample restrictions), 2,015 participants (34.2 percent) were randomly assigned to the control group, 1,925 participants (32.7 percent) to treatment *High Skilled*, and 1,947 participants (33.1 percent) to treatment *Low Skilled*. All remaining participants completed the survey and are included in the analysis. The completion rates reported at the bottom of Table 3.1 refer to the full sample before applying the sample restrictions (except the first restriction since individuals had not been assigned to a treatment yet). The numbers of observations in our regression analyses are slightly smaller because we excluded respondents with missing covariates from the analyses.

[30] See Appendix Figure A3.2 for histograms of answers to the general attitude questions.

[31] Note reported numbers of observations in our regressions is a bit lower than the numbers reported in the balancing tables because of item non-response. Importantly, treatments status is unrelated to item non-response.

need-based student aid (an indicator for low family income) have more positive attitudes (column 1 of Table A3.4). Consistent with the strong correlations reported in Appendix Table A3.3, respondents' socio-demographic characteristics also predict their beliefs about refugees' education level (see Appendix Table A3.5): Males, older respondents, and students born abroad are less likely to believe that refugees' education level is high. In contrast, students who spoke to refugees and recipients of need-based student aid are more optimistic. Interestingly, additional heterogeneity analyses (not shown) reveal that the information treatment effect on education beliefs is very similar across socio-demographic groups.

3.4.2 Impact of Information Treatment on Beliefs About Refugees' Education Level

Figure 3.1 shows that the two opposing information treatments shift the beliefs about refugees' education level in opposing directions. The information provided in the *Low Skilled* treatment shifts education beliefs downward (left panel); in contrast, the information in the *High Skilled* treatment shifts education beliefs upward (right panel). Table 3.2 presents the results in regression form. The dependent variable in columns 1 and 2 equals 1 if the respondent agrees completely or somewhat that refugees are well educated, and equals 0 otherwise; in columns 3 and 4, the dependent variable equals 1 if the respondent disagrees completely or somewhat (0 otherwise). In columns 5 and 6, we use the original, five-point scale, outcome, with higher values indicating more agreement with the statement that refugees are well educated on average. The *High Skilled* treatment increases the share of respondents who agree with the statement by 14 percentage points. Since the respective share is only 18 percent in the control group, this is a very strong effect. In contrast, the *Low Skilled* treatment strongly decreases the share of respondents with positive views on refugees' education level by 5 percentage points (or 28 percent).[32][33]

Table 3.3 reports the information treatment effect in the follow-up survey. The information provided in this survey (32 percent of adult refugees have a high school degree and 13 percent a university degree; respective shares among the German population

[32] Since we elicited beliefs about refugees' education level on a five-point-scale, we can also investigate how the information treatments affect each answer category. It turns out that the information treatments did not only affect those who "somewhat agree" or "somewhat disagree" with the statement, but also changed the shares of respondents who articulated strong agreement and strong disagreement, respectively (results available upon request).

[33] In Appendix Table A3.6, we regress respondents' education beliefs on the treatment indicator, respondents' characteristics, and interactions between treatment indicator and characteristics, separately for treatment*High Skilled* (columns 1 to 3) and *Low Skilled* (columns 4 to 6). The coefficients on the interaction terms allow us to characterize the compliers in both treatments: Respondents in the diploma-track (male and older respondents) are significantly less likely to react to treatment *High Skilled* (*Low Skilled*) than their counterparts.

are 29 percent and 21 percent) strongly increases the share of respondents who agree that refugees are well-educated by 29 percentage points. This finding has two important implications: First, the information treatment effect in the follow-up survey is very similar to the strong positive effect of the *High Skilled* treatment in the main survey. Second, and more importantly, the strong information treatment effects in the main survey are not due to the way the information was presented, in particular, these effects are not driven by the included interpretation of the numbers (*e.g.,* "...a study has found that the education level of refugees is rather high since...").

Persistence of information treatment effect and effect heterogeneities by initial beliefs

One potential issue with information experiments is that the information provided might trigger experimenter demand effects or priming effects instead of genuine belief updating.[34] We provide two pieces of evidence that suggest that the strong effects of the information treatments on beliefs about refugees' education level are not driven by experimenter demand effects or by priming effects.

First, the effects of the information treatment persist for one week. Combining data from the follow-up survey and the re-survey one week later, we regress respondents' beliefs about refugees' education level on an information treatment dummy, a re-survey dummy, and an interaction term of these two indicators (Appendix Table A3.7). The information treatment not only increases the share of respondents who agree that refugees are well-educated at the moment the information is provided, but substantially increases this share still one week later when the information is not provided (again). As expected, the immediate treatment effect is stronger than the longer-run impact, which is likely due to imperfect recall.

Appendix Table A3.8, using alternative outcomes, again shows that the information treatment has longer-run impacts on respondents' beliefs about refugees' education level: While respondents in the control group underestimate the share of refugees who hold a high school degree by almost 12 percentage points (the control mean in column 1 is 21 percent, the true value is 32 percent), the treatment group holds significantly more accurate beliefs (columns 2 and 3). Interestingly, the treatment does not improve estimates of refugees' university graduation rate (columns 5 and 6). Thus, the positive treatment effect on education beliefs in Table 3.3 stems from correcting respondents' initial beliefs about refugees' high school graduation rate upward. Re-

[34] Experimenter demand effects occur if the information provided contains indications about the experimenter's intentions and respondents answer accordingly to please the experimenter (Zizzo, 2010; de Quidt, Haushofer and Roth, 2018). Similarly, specific words in the information might activate certain concepts in respondents' memory that influence their answering behaviour unconsciously (priming effects).

spondents who were provided the information on these education shares one week earlier are also more confident about their estimates (column 7).[35] Similar to previous studies (Grigorieff, Roth and Ubfal, 2016; Haaland and Roth, 2017; Cavallo, Cruces and Perez-Truglia, 2017), we argue that it is very unlikely that experimenter demand effects or priming effects persist until one week later in the re-survey. This interpretation is consistent with recent evidence by Mummolo and Peterson (2018) who show that survey experiments are robust to experimenter demand effects.

Second, the large sample in the main survey allows estimating heterogeneous treatment effects by respondents' *baseline* beliefs about refugees' education level. For this analysis, we first have to predict the baseline beliefs of respondents in the two information treatments.[36] Column 1 of Appendix Table A3.10 shows that while treatment *High Skilled* increases beliefs about refugees' education level (measured on the original five-point scale) among respondents with high and low baseline beliefs, treatment *Low Skilled* only decreases beliefs about refugees' education level among respondents with high baseline beliefs, but not among those with low baseline beliefs. Using dichotomized measures of education beliefs as outcome variables (see columns 2 and 3), the pattern is identical for treatment *Low Skilled*, which only decreases (increases) the probability that respondents agree (disagree) with the statement that refugees are well educated among those with high baseline beliefs. Similarly, treatment *High Skilled* increases (decreases) the probability to agree (disagree) with the statement if baseline beliefs are low. Interestingly, this treatment particularly increases the prob-

[35] In Appendix Table A3.9, we regress an indicator for participation in the re-survey on the treatment indicator and covariates. While a few covariates are significantly related to re-survey participation, they are jointly insignificant (p=0.89, joint F-test performed on regression of column 2). Most importantly, the insignificant coefficients on the treatment indicator show that attrition does not differ across treatment arms. Thus, providing information to respondents does not affect their probability of participating in the re-survey one week later.

[36] To verify that the information provision indeed affects beliefs about refugees' education level, it was necessary to elicit the education beliefs after providing the information to respondents in the two treatment groups. We abstained from belief elicitation before providing the information to avoid behavioural anomalies such as backfire effects where individuals respond defiantly to belief corrections by reinforcing their initial beliefs (Nyhan and Reifler, 2010). Instead, we imputed the baseline beliefs of respondents in the two treatment groups. To do so, we regressed the education beliefs (using the original five-point scale) of the respondents in the control group on all socio-demographic background characteristics, university, faculty, and opinion formation aspects (except economic aspects since they were affected by the information provision; see Section 3.4.5). We then used the estimated coefficients from the control group and imputed the baseline beliefs of respondents in the two treatment groups, using their background characteristics and opinion aspects. Finally, we split the imputed baseline belief at the median to define high and low baseline beliefs. This imputation procedure seems to work well: First, among respondents in the control group, the reported beliefs and the imputed beliefs are substantially correlated (r=0.58). Second, again using only the control group, the standard deviation of the imputed beliefs is rather large (57 percent) relative to the standard deviation of the reported beliefs.

ability to agree among those with relatively high baseline beliefs.[37] Since almost all patterns in the table indicate belief updating, we consider it highly unlikely that our information treatment effects are driven by experimenter demand effects or priming effects.[38]

3.4.3 Impact of Beliefs About Refugees' Education Level on Labour Market Competition and Fiscal Burden Concerns

We now assess the relevance of the two competing theories, the labour market competition model and the fiscal burden model, in the context of the European refugee crisis. Table 3.4 presents results from IV estimates of the effects of beliefs about refugees' education level (instrumented with the two information treatment indicators) on labour market competition and fiscal burden concerns.[39] The dependent variables in columns 1 and 4 are the summary indices of labour market competition and fiscal burden concerns, respectively (see Section 3.3.1). The outcomes in the remaining columns are binary indicators of agreement with the individual statements that make up the two summary indices.

Consistent with the *labour market competition model*, respondents are more concerned about competition from refugees on the labour market if they believe that refugees are well-educated, rather than low-educated (column 1). This applies to concerns about increased competition for the respondent personally, but particularly to concerns about increased competition on the labour market in general (control mean: 26 percent; see column 3). A post-estimation test reveals that the treatment effect in column 3 is marginally significantly larger than the effect in column 2 (p=0.11). This moderate effect, together with the low level of baseline concerns that refugees increase labour market competition for the respondent personally (control mean is only 4 percent, see column 2), suggests that, while a relevant share of students is con-

[37] One possible explanation for the positive effect of treatment High Skilled on the probability to agree to the statement of persons with high baseline beliefs is that the specific information provided (i.e., 43 percent of refugees have attended a university) constitutes a positive shock to their beliefs about the share of refugees who have attended a university (which we did not measure). This finding is also consistent with the notion that these respondents hold motivated beliefs, which they update selectively (*e.g.* Bénabou and Tirole, 2016).

[38] This approach to distinguishing information effects from other unintended effects was developed by Lenz (2009) and has been applied to various survey experiments, *e.g.,* Cruces, Perez-Truglia and Tetaz (2013), Schueler and West (2016), and Lergetporer et al. (2016).

[39] All results are robust to including survey date fixed effects, indicating that results do not depend on the day when respondents answered the survey.

cerned about increased general labour market competition, most respondents do not perceive refugees as their direct competitors on the labour market.[40]

In contrast, we do not find any evidence for the *fiscal burden model*. Treatment effects on both the summary index (column 4) and on its components (columns 5 to 7) are precisely estimated zeros. While 30 percent of respondents think that refugees will bring more revenues than costs for the government, this share is unaffected by shifts in respondents' education beliefs (column 5). Similarly, the treatment does not affect respondents' concerns that they will have to pay more taxes (control mean: 24 percent, see column 6) or that they will have to forgo future government benefits because of the refugees (control mean: 11 percent, see column 7). Beliefs about refugees' education level also do not affect agreement to other refugee-related statements, for instance, that refugees are a cultural enrichment, or concerns that they increase crime levels (see Appendix Table A3.11).

To account for the fact that we estimate effects of education beliefs on multiple outcomes, we also conduct a multiple hypotheses correction using the procedure of Romano and Wolf (2005). Reassuringly, when accounting for the multiple hypotheses tests performed on the four summary indices in Tables 3.4,3.5,A3.11, the coefficient on the labour-market-concerns index remains highly statistically significant ($p < 0.01$).

3.4.4 Impact of Beliefs About Refugees' Education Level on General Attitudes towards Refugees

Next, we investigate whether the increased labour market competition concerns translate into a change in general attitudes towards refugees. Again using the IV model in equations (1) and (2), we find no effect of beliefs about refugees' education level on general attitudes towards refugees (Table 3.5). This is true for the summary index of general attitudes (column 1) as well as for the individual items that make up the summary index (columns 2 to 4). While attitudes towards refugees are strongly correlated with beliefs about their education level (see Appendix Table A3.3), Table 3.5 implies that these correlations are not driven by an impact of education beliefs on attitudes.

[40] Interestingly, the effect of education beliefs on concerns that refugees increase labour market competition for the respondent personally is basically zero in the follow-up survey experiment, which provides a different type of information. This result is consistent with Appendix Table A3.8, which shows that the information treatment only shifts beliefs about the share of refugees with a high school degree, but not beliefs about university graduation rates. Since our sample of university students is unlikely to consider refugees with a high school degree as competitors on the labour market, it is not surprising that this information treatment does not affect concerns about increased personal labour market competition.

The finding that increased labour market competition concerns do not translate into more negative general attitudes may be surprising at first sight, given that potential labour market impacts of the large refugee inflow in Germany play a prominent role in the public debate. However, our finding is consistent with existing studies on attitudes towards immigration, which find that economic considerations play only a minor role in the attitude formation process (see Dustmann and Preston, 2007; Hainmueller and Hiscox, 2010). In the next section, we provide direct empirical evidence that this interpretation also applies to our study.

3.4.5 Aspects Shaping Attitudes Towards Refugees

To investigate the connection between respondents' beliefs about refugees' education level and general attitudes more closely, we elicited the importance that respondents attribute to various aspects when forming their attitude towards refugees. Table 3.6 presents results of IV regressions in which all outcomes are binary and equal 1 if the respondent considers the given aspect as important, respectively unimportant, for her attitude formation process (and 0 otherwise).[41]

Table 3.6 contains two interesting findings: First, beliefs about refugees' education level do not affect the importance of any aspect of opinion formation except for economic aspects, which become more important (*i.e.*, less unimportant) with higher education beliefs.[42] This suggests that providing information about refugees' education level only triggers respondents' economic considerations. This result is related to an open question in the literature on attitudes towards immigration as to what extent respondents associate the education level of refugees, or immigrants more generally, with economic aspects rather than with social or cultural aspects (Hainmueller and Hiscox, 2010).

The second key finding of Table 3.6 concerns the relative importance of economic aspects versus other aspects when individuals form their attitude towards refugees. Using only respondents from the control group (who have not been affected by any information treatment), we find that *refugees' willingness to integrate* and *humanitarian aspects* are important for most respondents (88 percent and 86 percent, respectively). These aspects are followed by *personal experience with refugees* (70 percent), *refugees' criminal behaviour* (54 percent), and *religion/culture of refugees* (45 percent). Intriguingly, *economic aspects* are the least important aspect: Only 39 percent of respondents consider them important when forming their attitudes towards refugees.

[41] Appendix Table A3.12 reports bivariate correlation coefficients between all opinion formation aspects.
[42] This treatment effect remains significant (p=0.03) after correcting for the multiple hypotheses tested in Table 3.6 using the step-down procedure described in Romano and Wolf (2005).

This pattern also holds when regressing general attitudes on all opinion aspects simultaneously (Appendix Table A3.13): Compared to all other opinion aspects, the relationship between economic aspects and general attitudes is much weaker. The great importance attributed to humanitarian aspects in our sample is similar to Bansak, Hainmueller and Hangartner (2016), who find that humanitarian aspects play a major role in whether natives are willing to accept refugees. However, while these authors also identify employability and religion as being important for shaping natives' attitudes towards refugees, religious and economic aspects are relatively unimportant in our context.

In sum, our results show that shifting beliefs about refugees' education level upward (*i.e.,* refugees are more likely to be considered highly educated) increases labour market competition concerns. However, these economic concerns do not translate into more negative attitudes towards refugees because economic aspects are rather unimportant when individuals form their attitudes towards refugees.

3.4.6 Validity of the Exclusion Restriction

The validity of our IV estimates hinges on the assumption that the only channel through which the information treatments impact the outcome variables of interest is through shifting respondents' beliefs about the education level of refugees. One important potential concern may be that the treatments in our main survey experiment did not only inform respondents about refugees' education level, but also mentioned their country of origin (Syria). Any direct effect (i.e., not operating through education beliefs) of this information on respondents' concerns or attitudes toward refugees would invalidate our IV approach. For several reasons, we consider such direct effects unlikely. First, the fact that most refugees in Germany come from Syria has already been well known within the German public.[43] Therefore, this part of the treatment hardly constituted new information for our survey respondents. Second, the fact that both information treatments mentioned Syrians, but had opposing effects on respondents' labour market concerns (treatment *High Skilled* increased those concerns, treatment Low Skilled reduced them; see below) is hard to reconcile with the notion that information about refugees' origin is important. Third, our findings that the treatments do not affect any other statements/concerns about refugees, such as concerns about increased crime levels (see Appendix Table A3.11), speaks against the

[43] This notion is corroborated by the fact that in 2015, Google documented on average 18 search requests each day in Germany for the German equivalent of "refugees Syria", whereas there were only two requests (one request) for "refugees Afghanistan" ("refugees Iraq") (Google Trends, https://trends.google.de/trends/explore?date=2015-01-01%202015-12-31&geo=DE&q=Fl%C3%BCchtlinge%20Syrien,Fl%C3%BCchtlinge%20Afghanistan,Fl%C3%BCchtlinge%20Irak; [accessed 7 August 2018]).

presumption that the treatment operates through other channels than education beliefs.

Finally, our reduced-form results are in line with our findings from our IV estimates: In Appendix Table A3.14, we regress respondents' labour market competition and fiscal burden concerns on the two information treatment indicators. Consistent with the IV results in Table 4, the treatment *High Skilled* (*Low Skilled*) significantly increases (decreases) the summary index for respondents' labour market concerns (column 1), but does not affect fiscal burden concerns. In line with the IV results in Table 3.5 and Appendix Table A3.11, the reduced-form results in Appendix Tables A3.15 and A3.16 reveal precisely estimated null effects on respondents' general attitudes and other refugee-related statements, respectively. Similarly, Appendix Table A3.17 yields significant and positive treatment effects of treatment *High Skilled* on the importance of economic aspects for shaping respondents' attitudes toward refugees, which corroborates our IV findings in Table 3.6. In sum, the fact that these intention-to-treat effects are in line with our findings in Sections 3.4.3 to 3.4.5 is reassuring since their causal interpretation does not depend on the validity of the exclusion restriction.

3.4.7 Social Desirability Bias

Respondents might perceive some questions on their attitudes towards refugees as sensitive. One prominent concern with sensitive survey questions is that respondents might give socially desirable answers instead of answering honestly. A widely used technique to reduce, or even avoid, social desirability bias is the so-called *item count technique* (ICT). The ICT is designed to foster truthful reporting by providing respondents a "veil" that prevents researchers from inferring an individual's answer to a specific sensitive item. Researchers, however, are still able to draw probabilistic inferences for groups of respondents (See Coffman, Coffman and Ericson (2017) for a detailed description and validation of the ICT). The ICT randomly assigns survey respondents to a *direct response group* whose members are directly asked whether they agree with a sensitive item. Respondents in the *veiled response group*, in contrast, report on how many of N+1 items (which include the sensitive item and N other items) they agree with. In our case N=4 since we use four additional, non-sensitive items to veil answers to the sensitive item. See Appendix A3.2 for a detailed description of how the ICT works.

In the follow-up survey, we used this technique to assess the social desirability bias for five potentially sensitive items: labour market competition concerns (both for the respondent personally and in general); concerns about fiscal revenues and costs; and

aspects shaping the attitude towards refugees (humanitarian aspects and economic aspects). In Table 3.7, we regress the number of items (out of five items) that respondents agree with on a binary indicator for respondents in the *veiled response group*. The small and statistically insignificant coefficients on the veiled indicator in the first three columns indicate that reported labour market competition and fiscal burden concerns are not affected by social desirability bias. On the other hand, the negative, and statistically significant, coefficient for humanitarian aspects suggests that social desirability bias leads to some *over*-reporting of the importance of humanitarian aspects when respondents are asked directly (*i.e.*, when no veil is provided). Less intuitively, we also find a negative coefficient on veiled answers for economic aspects, suggesting that respondents more often report economic aspects to be important when asked directly. Using a non-sensitive placebo item (*"I used a laptop computer for completing this survey"*) shows that the significant coefficients in columns 4 and 5 do not arise mechanically from the ICT. Adding up the mean answers in the direct response group (see "Mean (direct response)" in Table 3.7) and the respective regression coefficient yields social-desirability-bias-adjusted average responses (See Coffman, Coffman and Ericson (2017) for details). Results show that the adjusted share of respondents – in this sample – who consider humanitarian aspects and economic aspects important for their attitude formation process towards refugees is 68 percent (*i.e.,* 95 percent minus 27 percent) and 49 percent (*i.e.*, 69 percent minus 20 percent), respectively. This finding underscores the conclusion of the main survey that economic aspects are much less important than humanitarian aspects. In sum, the evidence from Table 3.7 makes us confident that the direct questions in the main survey experiment generally provide accurate information, especially concerning labour market competition and fiscal burden concerns.

3.5 Discussion and Conclusion

We conducted randomized online survey experiments with more than 5,000 university students in Germany to investigate how beliefs about refugees' education level affect attitudes towards them. We randomly provided information from existing studies on refugees' education level that strongly shifted respondents' education beliefs in the expected direction. Consistent with the labour market competition model, we find that beliefs about refugees' education affect labour market competition concerns. In contrast, we find no effects on fiscal burden concerns or other specific concerns such as increasing crime levels. The labour market competition concerns, however, do not translate into general attitudes towards refugees because economic aspects are rather unimportant for shaping respondents' attitudes.

Our findings have important policy implications. First, the fact that humanitarian aspects are very important for shaping respondents' attitudes towards refugees shows that public opinion is in line with the legal requirements of the Geneva Convention, which stipulates that the decision of granting prosecuted asylum seekers temporary refugee status is independent of their characteristics. This result is similar to that of Bansak, Hainmueller and Hangartner (2016) and indicates that policy makers might have some leeway to increase public acceptance of refugees by highlighting humanitarian, instead of economic, aspects. Second, while the effects of the large refugee inflow on the labour market and on the government budget remain to be seen, our findings suggest that developments in these areas will only have limited impact on public attitudes, at least among high-skilled natives.[44]

We focus on university students as an interesting group of the population since the two economic theories on natives' attitudes towards refugees make opposing predictions for how education beliefs affect attitudes. Yet, one potential shortcoming of our study is that we focus only on the upper part of the skill distribution, but remain silent about less educated natives. To put our results into a broader perspective, we compare university students with other population groups. To do so, we draw on the 2016 wave of the *ifo Education Survey*, a representative opinion survey on education policy in Germany that contains two questions on beliefs about refugees' education level.[45] Comparing respondents with a vocational degree, university graduates, and university students reveals that the latter two groups are more optimistic about refugees' education level: While 35 percent of university students and 27 percent of university graduates believe that refugees' education level is "rather high" or "very high", this view is shared by only 20 percent of respondents with a vocational degree. Similarly, while 47 percent of university students and 43 percent of university graduates believe that refugees will help to reduce the shortage of skilled labour in Germany, only 33 percent of those with a vocational degree hold this belief. This pattern of beliefs, together with the positive relationship between beliefs about refugees' education level and general attitudes towards them (see Appendix Table A3.3), is consistent with the finding that more highly educated natives exhibit more positive attitudes towards im-

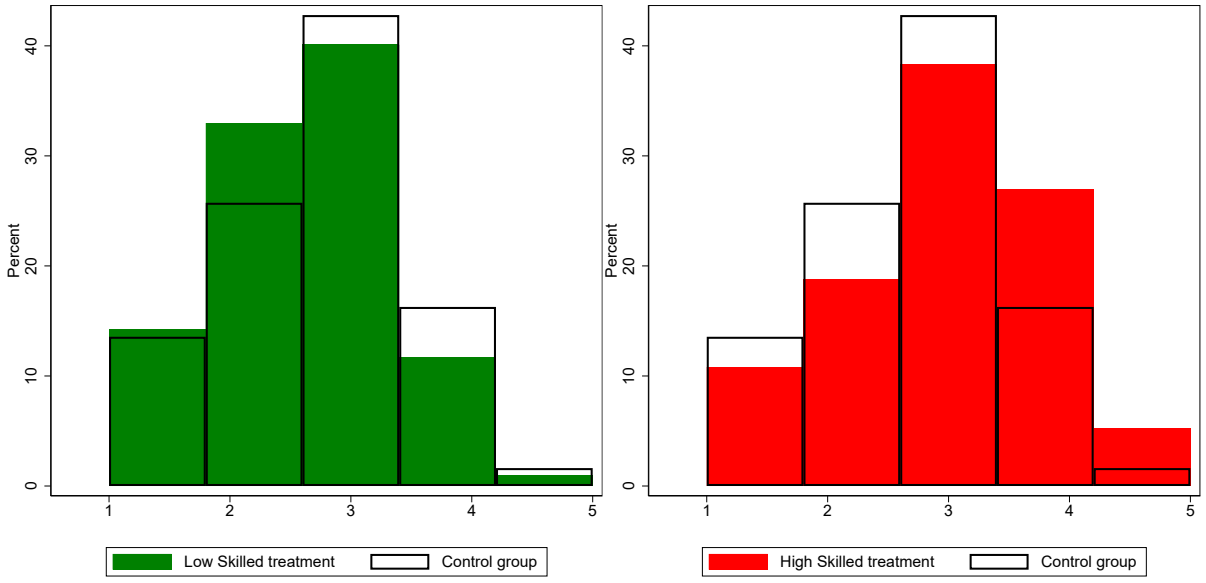
[44] This result differs somewhat from Bansak, Hainmueller and Hangartner (2016), who find that economic concerns are important in the sense that respondents are more likely to accept asylum seekers if they worked in higher-skilled occupations in their home country.

[45] Similar to our survey, one question asked respondents about their beliefs about refugees' average education level on a four-point scale (from 1="very low" to 4="very high"). The second question elicited respondents' agreement with the following statement: *"The refugees will help to reduce the skill shortage of the German economy"* on a five-point scale (from 1="completely disagree" to 5="completely agree"). Note that differences in question wording and the number of answer categories, respectively, hamper a direct comparison of results between the *ifo Education Survey* and our survey. For more information on the *ifo Education Survey*, see Lergetporer, Werner and Woessmann (2017).

migrants (*e.g.,* d'Hombres and Nunziata, 2016). While this suggests that providing information about refugees' education level may affect natives with different education backgrounds very differently, Bansak, Hainmueller and Hangartner (2016) find that the effects of asylum seekers' attributes on their acceptance is homogeneous with respect to respondents education level. In order to investigate the external validity of our findings, we consider the application of our experimental design to other groups of the population an interesting avenue for future research. While survey experiments are certainly subject to some artificiality, we have three reasons for considering this method informative and well-suited for answering our research question. First, in order to identify the causal effect of beliefs about refugees' education level on attitudes with naturally occurring data, one would need detailed measures of attitudes as well as exogenous variation in education beliefs. We are not aware of any data source that fulfils both requirements. Second, Barabas and Jerit (2010) provide evidence for the external validity of survey experiments: They show that the information effects in their survey experiment are also present in a natural setting, in which news exposure covers the same information. Therefore, survey experiments are able to uncover information effects that are also present in a natural environment. Similarly, survey responses on attitudes toward migration have been shown to correspond closely to incentivised, actual political behaviour (see footnote 21). Third, Blinder and Krueger (2004) argue that public opinion surveys are important for the political process as politicians devote enormous resources to assessing public opinion through surveys. In the light of the European refugee crisis, much of the political debate has focused on natives' attitudes towards refugees and asylum policies, which are typically measured in opinion surveys. The present chapter aims at contributing to understanding the underlying determinants that drive public attitudes that may strongly affect the political feasibility of asylum policy.

Figures and Tables

Figure 3.1 : Effect of information treatment on beliefs about refugees' education level



Note: Agreement to statement "On average, refugees are well educated." Answer categories: 1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree", and 5="completely agree."

Table 3.1 : Comparison of socio-demographic characteristics across control and treatment groups

	Mean	Difference to control group		Difference High and Low skilled
	Control group	High skilled	Low skilled	
	(1)	(2)	(3)	(4)
University				
Dresden	0.81	-0.00	0.00	-0.00
Konstanz	0.09	-0.01	-0.01	0.00
Munich	0.08	0.01	0.00	0.01
Chemnitz	0.02	0.00	0.01	-0.00
Male	0.54	-0.02	0.03*	-0.05***
Age	24.37	0.11	0.06	0.05
Bachelor	0.30	0.02	-0.01	0.03*
Master	0.20	0.02	0.02*	-0.01
Diploma	0.28	-0.02	-0.01	-0.01
PhD	0.09	0.00	0.00	0.00
Other study level	0.14	-0.01	-0.00	-0.01
Semester	5.63	-0.10	0.02	-0.12
Born abroad	0.07	0.02**	0.00	0.02*
No parent born abroad	0.86	-0.02	-0.01	-0.01
One parent born abroad	0.06	-0.01	0.01	-0.01
No parent has college degree	0.37	-0.05***	-0.03**	-0.01
Receives need-based student aid	0.42	-0.04**	-0.04**	-0.00
Not encountered refugees	0.14	-0.00	0.01	-0.02
Faculty				
Language, Culture	0.12	-0.00	-0.01	0.00
Psychology	0.00	0.00	0.00	-0.00
Social Sciences and Pedagogy	0.11	-0.00	-0.01	0.00
Law	0.02	0.01*	0.00	0.01
Commercial Information Systems	0.06	-0.00	0.01	-0.01
Business and Economics	0.04	0.01	0.01	0.01
Maths and Science	0.09	0.01	0.01	0.00
Medicine	0.06	0.01	-0.01	0.01
Engineering	0.35	-0.01	0.01	-0.02
Arts and Music	0.00	-0.00	0.00	-0.00
Other faculty	0.13	-0.02*	-0.02	-0.01
Survey completed	0.89	-0.00	0.00	-0.01
Respondents	1,668	1,604	1,629	

Notes: Column (1) reports means of the control group. Columns (2) and (3) report the difference in means between control group and respective treatment group. Column (4) reports the difference in means between low skilled treatment and high skilled treatment group. Significance levels of differences come from linear regressions of characteristics on the respective treatment dummies. All statistics refer to the analysis sample, except for the survey completion rates, which refer to the sample before applying sample restrictions; see Section 3.3.4. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3.2 : Effect of information treatment on beliefs about refugees' education level

	Agree		Disagree		Five-point scale	
	(1)	(2)	(3)	(4)	(5)	(6)
High skilled information	0.144***	0.140***	−0.100***	−0.104***	0.307***	0.312***
	(0.015)	(0.015)	(0.017)	(0.016)	(0.035)	(0.034)
Low skilled information	−0.051***	−0.048***	0.078***	0.071***	−0.142***	−0.125***
	(0.013)	(0.013)	(0.017)	(0.017)	(0.033)	(0.032)
Covariates	No	Yes	No	Yes	No	Yes
Control mean	0.18	0.18	0.39	0.39	2.67	2.67
Observations	4,831	4,831	4,831	4,831	4,831	4,831
Adj. R2	0.04	0.07	0.02	0.07	0.04	0.10

Notes: Dependent variables: agreement to statement "On average, refugees are well educated": Columns (1)+(2): binary variable (1="completely agree" or "somewhat agree", 0 otherwise); Columns (3)+(4): binary variable (1="completely disagree" or "somewhat disagree", 0 otherwise); Columns (5)+(6): integer values from 1 to 5 (1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree"; 5="completely agree"). *Covariates* include all characteristics from Table 3.1. *Control mean* is the mean of the indicated outcome of respondents in the control group. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3.3 : Effect of information treatment on beliefs about refugees' education level (follow-up survey)

	Agree		Disagree		Five-point scale	
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	0.284***	0.295***	−0.193***	−0.204***	0.597***	0.619***
	(0.037)	(0.037)	(0.040)	(0.039)	(0.081)	(0.080)
Covariates	No	Yes	No	Yes	No	Yes
Control mean	0.17	0.17	0.45	0.45	2.62	2.62
Observations	555	555	555	555	555	555
Adj. R2	0.09	0.13	0.04	0.08	0.09	0.14

Notes: Dependent variables: agreement to statement "On average, refugees are well educated": Columns (1)+(2): binary variable (1="completely agree" or "somewhat agree", 0 otherwise); Columns (3)+(4): binary variable (1="completely disagree" or "somewhat disagree", 0 otherwise); Columns (5)+(6): integer values from 1 to 5 (1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree"; 5="completely agree"). *Information treatment* indicates whether the respondent was assigned to the information treatment group (=1) or to the control group (=0). *Covariates* include all characteristics from Table A3.2. *Control mean* is the mean of the indicated outcome of respondents in the control group. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3.4 : Effect of beliefs about refugees' education level on labour market and fiscal burden concerns

	Labour market concerns			Fiscal burden concerns			
	Index	Increase competition for me	Increase competition in general	Index	More revenues than costs	Pay more taxes	Less gov't benefits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Refugees are well	0.267***	0.041**	0.096***	−0.008	0.035	0.007	0.015
educated on average	(0.069)	(0.017)	(0.036)	(0.064)	(0.035)	(0.035)	(0.026)
Control mean	-0.01	0.04	0.26	-0.02	0.30	0.24	0.11
Instrument F statistic	85.9	85.9	85.9	85.9	85.9	85.9	85.9
Respondents	4,818	4,818	4,818	4,818	4,818	4,818	4,818

Notes: Dependent variables: Column (1): index of labour market concerns, consisting of the two indicators in Columns (2) and (3). Column (4): index of fiscal burden concerns, consisting of the three indicators in Columns (5) to (7). Columns (2), (3), (5), (6), and (7): dummy variables which express agreement with the respective statement (1="completely agree" or "somewhat agree", 0 otherwise). See Appendix Table A3.18 for the wording of all survey questions and Section 3.3.1 for the construction of the summary indices. All regressions include the characteristics reported in Table 3.1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3.5 : Effect of beliefs about refugees' education level on general attitudes

	Index	Admit more refugees in future	#Refugees admitted last year	Allowed to stay permanently
	(1)	(2)	(3)	(4)
Beliefs about education	−0.043	−0.011	0.004	−0.007
	(0.071)	(0.035)	(0.032)	(0.037)
Control mean	0.02	0.31	0.22	0.65
Instrument F statistic	86.2	86.0	85.3	86.2
Respondents	4,830	4,805	4,810	4,829

Notes: Results from two-stage least-squares regressions. In the first stage, beliefs about refugees' education level are instrumented with the two binary indicators *high skilled information* and *low skilled information* (for first-stage results, see Table 3.2). Dependent variables: Column (1): index of general attitudes, consisting of the three indicators in Columns (2), (3) and (4). Columns (2)-(4): dummy variables which express agreement with the respective statement (1="completely agree" or "somewhat agree", 0 otherwise). See Appendix Table A3.18 for the wording of all survey questions and Section 3.3.1 for the construction of the summary index. All regressions include the characteristics reported in Table 3.1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3.6 : Effect of beliefs about refugees' education level on opinion formation aspects

	Refugees' willingness to integrate		Humanitarian aspects		Personal experience w/ refugees	
	Important	Unimportant	Important	Unimportant	Important	Unimportant
	(1)	(2)	(3)	(4)	(5)	(6)
Refugees are well	0.005	−0.006	−0.022	0.007	0.019	−0.002
educated on average	(0.025)	(0.016)	(0.028)	(0.019)	(0.034)	(0.024)
Control mean	0.88	0.04	0.86	0.06	0.70	0.12
Instrument F statistic	86.1	86.1	86.7	86.7	86.1	86.1
Respondents	4,831	4,831	4,830	4,830	4,831	4,831
	Refugees' criminal behaviour		Religion/culture of refugees		Economic aspects	
	Important	Unimportant	Important	Unimportant	Important	Unimportant
	(1)	(2)	(3)	(4)	(5)	(6)
Refugees are well	0.022	−0.040	0.070*	−0.059	0.051	−0.107***
educated on average	(0.039)	(0.034)	(0.040)	(0.038)	(0.039)	(0.038)
Control mean	0.54	0.26	0.45	0.37	0.39	0.37
Instrument F statistic	86.2	86.2	86.1	86.1	86.2	86.2
Respondents	4,830	4,830	4,831	4,831	4,828	4,828

Notes: Results from two-stage least-squares regressions. In the first stage, beliefs about refugees' education level are instrumented with the two binary indicators *high skilled information* and *low skilled information* (for first-stage results, see Table 3.2). Dependent variables: importance of various aspects for respondents' opinion formation process toward refugees; respondents rated each aspect on a five-point scale: very important, somewhat important, neither important nor unimportant, somewhat unimportant, and very unimportant. *Important* equals 1 for "very important" or "somewhat important", 0 otherwise; *unimportant* equals 1 for "very unimportant" or "somewhat unimportant", 0 otherwise. All regressions include the characteristics reported in Table 3.1. *Control mean* is the mean of the indicated outcome of respondents in the control group. See Appendix Table A3.18 for the wording of all survey questions. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3.7 : Effect of veiled response treatment (follow-up survey)

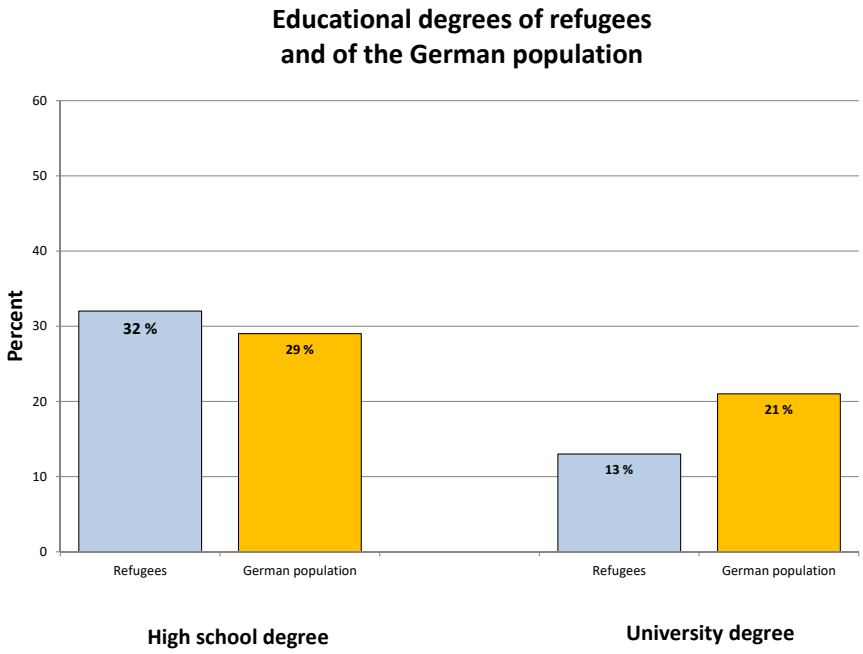
	Increase competition for me	Increase competition in general	More revenues than costs	Humanitarian aspects	Economic aspects	Laptop usage
	(1)	(2)	(3)	(4)	(5)	(6)
Veiled treatment	−0.045	−0.045	−0.113	−0.270***	−0.206***	−0.112
	(0.076)	(0.076)	(0.081)	(0.094)	(0.075)	(0.090)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Mean (direct response)	0.06	0.44	0.38	0.95	0.69	0.42
Observations	554	554	553	554	555	555
Adj. R2	0.13	0.13	0.07	0.07	0.03	0.05

Notes: Dependent variables: agreement to the statements indicated in the top row (only two answer categories: agree or disagree). *Veiled* equals 1 if respondent has been assigned to veiled response group and 0 if assigned to direct response group. The *direct response group* is directly asked whether they agree with a sensitive item. Respondents in the *veiled response group*, report on how many of N+1 items (which include the sensitive item and N other items) they agree with. *Mean (direct response)* is the mean of the outcome of respondents in the direct response group. *Covariates* include all characteristics from Appendix Table A3.2. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix

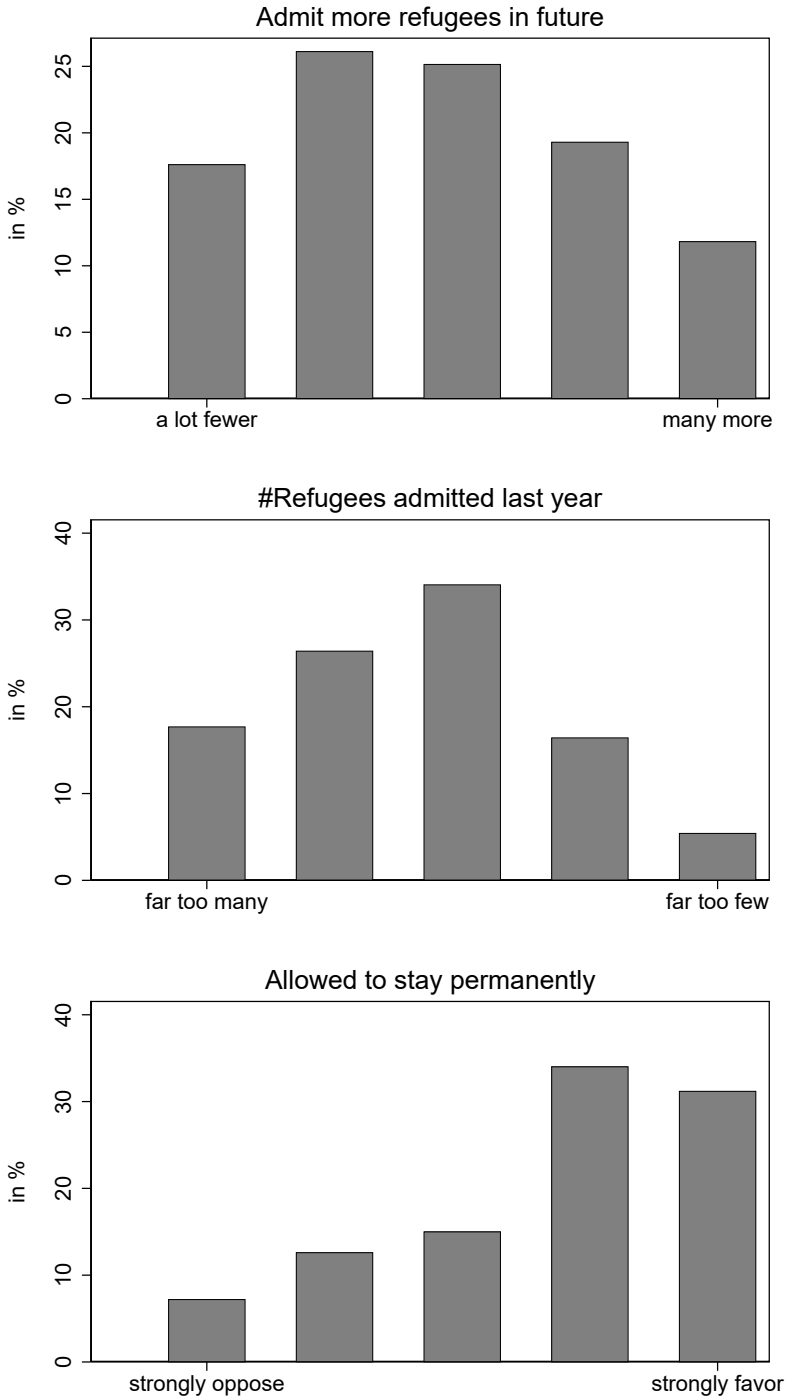
Appendix A3.1 Appendix Tables and Figures

Figure A3.1 : Graphical depiction used in information treatment in follow-up survey



Notes: This figure shows the graphical depiction used in the information treatment in the follow-up survey, which was provided (in German) to participants in addition to written information; see Section 3.2. The original German labels in the graph were: "Weiterführender Schulabschluss " (high school degree) and "Universitäts- oder anderer Hochschulabschluss" (university degree).

Figure A3.2 : General attitudes toward refugees



Notes: Figure shows distribution of answers to the general attitude questions, measured on a five-point scale. Figures are based on respondents in control group only.

Table A3.1 : Comparison of sample characteristics to university student populations

University of Munich			**Technical University Dresden**		
	Admin Data	Our Sample		Admin Data	Our Sample
Female	59.9%	62.6%	Female	42.4%	44.1%
Non-German	16.2%	8.7%	Non-German	13.1%	8.1%
Faculty			*Faculty*		
Catholic Theology	1.6%	1.9%	Mathematics and Science	11.7%	10.0%
Protestant Theology	1.5%	0.9%	Education and Pedagogy	11.5%	7.9%
Law	8.9%	6.6%	Law	2.8%	3.0%
Business Administration	5.5%	3.8%	Philosophy	6.1%	10.7%
Economics	2.3%	3.8%	Linguistics and Literature	2.8%	4.4%
Medicine	12.6%	12.0%	Economics and Business	7.8%	6.2%
Veterinary	3.6%	4.3%	Electrical and Computer Engineering	7.3%	6.1%
History and Art History	4.2%	4.7%	Computer Science	5.1%	5.9%
Philosophy	2.2%	2.4%	Mechanical Science and Engineering	17.1%	15.9%
Psychology and Pedagogy	6.4%	9.9%	Architecture	3.2%	2.0%
Cultural Studies	5.9%	3.8%	Civil Engineering	5.2%	4.9%
Linguistics and Literature	15.5%	15.8%	Environmental Sciences	6.9%	7.0%
Social Sciences	4.9%	7.6%	Transportation and Traffic Science	4.1%	8.9%
Mathematics, Computer Studies and Statistics	9.2%	9.9%	Medicine	8.5%	7.1%
Physics	4.8%	4.5%			
Chemistry and Pharmacy	4.3%	2.8%			
Biology	3.9%	2.8%			
Geology	2.7%	2.6%			

Notes: The administrative data refer to the summer term 2016 for the University of Munich and to the winter term 2015/2016 for the Technical University Dresden. For the University of Konstanz (University of Chemnitz), we only had access to the faculties of "Business Administration and Economics" and "Political Science" ("Business Administration and Economics") and therefore cannot compare faculty compositions. However, the share of females in our sample also match the administrative records of these two universities.

Table A3.2 : Comparison of socio-demographic characteristics across control and treatment group (follow-up survey)

	Mean Control group	Difference between control group and information treatment
	(1)	(2)
Ingolstadt	0.07	-0.01
Munich	0.59	0.02
Konstanz	0.21	0.01
Chemnitz	0.13	-0.03
Male	0.41	0.06
Age	25.53	-0.85
Bachelor	0.49	-0.00
Master	0.24	-0.00
PhD	0.09	0.01
Other study level	0.18	0.00
Semester	4.84	-0.19
Born abroad	0.09	-0.02
Mother born abroad	0.17	-0.03
Father born abroad	0.18	-0.03
No parent has college degree	0.38	0.05
Government aid	0.26	-0.01
Spoken to refugees	0.62	0.03
Language, Culture	0.10	0.04
Psychology	0.03	-0.01
Social Sciences and Pedagogy	0.10	0.02
Law	0.03	0.00
Commercial Information Systems	0.09	-0.01
Business and Economics	0.27	0.02
Maths and Science	0.18	0.02
Medicine	0.11	-0.06**
Engineering	0.00	-0.00
Arts and Music	0.04	0.00
Other faculty	0.06	-0.02
Participated in both waves	0.52	-0.03
Veiled	0.49	0.01
Respondents	293	289

Notes: Column (1) reports means of the control group. Column (2) reports the difference between control group and information treatment group. Statistical significance is based on linear regressions of characteristic on information treatment dummy. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.3 : Correlations between beliefs about refugees' education level and general attitudes

Bivariate correlations with beliefs about refugees' education level:	
Attitudes index	0.593***
Germany should admit more refugees in future	0.524***
Number of refugees Germany admitted last year	0.506***
Refugees should be allowed to stay in Germany permanently	0.571***

Notes: Correlations between beliefs about refugees' education level and general attitudes toward refugees. Correlations are based on control group only. *Attitude index* is based on the three indicators in rows 2, 3 and 4. See Appendix Table A3.18 for the wording of all survey questions and Section 3.3.1 for the construction of the summary index. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.4 : Relationship between general attitudes toward refugees and socio-demographic characteristics

	Attitude index	Admit more refugees in future	Too few refugees last year	Allow permanent stay
	(1)	(2)	(3)	(4)
Male	−0.172***	−0.029	−0.014	−0.082***
	(0.046)	(0.025)	(0.022)	(0.024)
Age	−0.016**	0.003	0.004	−0.012***
	(0.007)	(0.004)	(0.003)	(0.004)
Born abroad	−0.212*	−0.002	0.017	−0.174***
	(0.119)	(0.057)	(0.051)	(0.063)
At least one parent born abroad	−0.077	−0.040	−0.043	−0.026
	(0.079)	(0.043)	(0.038)	(0.045)
At least one parent w/ college degree	0.033	0.022	0.035	0.028
	(0.047)	(0.025)	(0.022)	(0.025)
Spoken to refugees	0.307***	0.196***	0.155***	0.132***
	(0.060)	(0.032)	(0.028)	(0.036)
Seen refugees	−0.017	0.059*	0.038	−0.011
	(0.062)	(0.032)	(0.028)	(0.038)
Need-based student aid	0.109**	0.002	0.006	0.065***
	(0.047)	(0.025)	(0.022)	(0.025)
Field of study and degree indicators	Yes	Yes	Yes	Yes
University indicators	Yes	Yes	Yes	Yes
Observations	1,646	1,636	1,641	1,645
Adj. R2	0.06	0.03	0.03	0.06

Notes: Dependent variables: Column (1): index of general attitudes, consisting of the three indicators in Columns (2), (3) and (4). Columns (2)-(4): dummy variables which express agreement with the respective statement (1="completely agree" or "somewhat agree", 0 otherwise). See Appendix Table A3.18 for the wording of all survey questions and Section 3.3.1 for the construction of the summary index. Includes only respondents from the control group. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.5 : Relationship between beliefs about refugees' education level and respondents' socio-demographic characteristics

	Agree	Disagree	Five-point scale
	(1)	(2)	(3)
Male	−0.120***	0.156***	−0.392***
	(0.021)	(0.025)	(0.049)
Age	−0.011***	0.013***	−0.030***
	(0.003)	(0.004)	(0.007)
Born abroad	−0.017	0.150**	−0.248**
	(0.046)	(0.064)	(0.120)
At least one parent born abroad	−0.012	0.014	−0.019
	(0.034)	(0.047)	(0.084)
At least one parent w/ college degree	0.004	0.013	−0.030
	(0.021)	(0.026)	(0.050)
Spoken to refugees	0.060**	0.005	0.030
	(0.028)	(0.036)	(0.067)
Seen refugees	0.017	0.045	−0.090
	(0.028)	(0.038)	(0.070)
Need-based student aid	0.010	−0.064**	0.099**
	(0.021)	(0.026)	(0.049)
Field of study and degree indicators	Yes	Yes	Yes
University indicators	Yes	Yes	Yes
Observations	1,638	1,638	1,638
Adj. R2	0.04	0.05	0.07

Notes: Dependent variables: agreement to statement "On average, refugees are well educated": Column (1): binary variable (1="completely agree" or "somewhat agree", 0 otherwise); Column (2): binary variable (1="completely disagree" or "somewhat disagree", 0 otherwise); Column (3): integer values from 1 to 5 (1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree"; 5="completely agree"). See Appendix Table A3.18 for exact wording of outcome. Estimations based on control group only. Robust standard errors reported in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A3.6 : Characterizing compliers with the treatments on beliefs about refugees' education level

	Agree	Disagree	Five-point scale	Agree	Disagree	Five-point scale
	(1)	(2)	(3)	(4)	(5)	(6)
High skilled information	0.060	−0.036	0.098			
	(0.117)	(0.131)	(0.274)			
x_male	0.014	−0.029	0.064	0.054**	−0.009	0.072
	(0.032)	(0.034)	(0.072)	(0.027)	(0.036)	(0.067)
x_age	0.003	−0.004	0.009	0.010***	−0.010*	0.017*
	(0.005)	(0.005)	(0.011)	(0.004)	(0.005)	(0.010)
x_born_abroad	0.040	−0.144*	0.268	−0.001	0.002	−0.079
	(0.072)	(0.087)	(0.180)	(0.058)	(0.089)	(0.165)
x_atleastoneparentforeign	−0.044	0.091	−0.222*	−0.006	−0.039	0.034
	(0.056)	(0.066)	(0.132)	(0.044)	(0.065)	(0.113)
x_parentuniversity	0.006	0.016	−0.023	0.018	−0.011	0.045
	(0.033)	(0.036)	(0.075)	(0.028)	(0.037)	(0.069)
x_spoketorefugee	0.048	0.001	0.084	0.001	−0.055	0.081
	(0.044)	(0.050)	(0.099)	(0.036)	(0.050)	(0.090)
x_metrefugeepers	0.007	−0.013	0.030	−0.017	−0.027	0.040
	(0.045)	(0.053)	(0.103)	(0.037)	(0.053)	(0.095)
x_bafoeg	0.001	0.034	−0.067	0.025	0.000	0.071
	(0.033)	(0.036)	(0.075)	(0.027)	(0.036)	(0.068)
x_master	−0.015	−0.005	0.011	−0.032	−0.006	−0.032
	(0.045)	(0.050)	(0.103)	(0.037)	(0.052)	(0.095)
x_diplom	−0.085**	0.069	−0.180*	0.006	0.011	−0.027
	(0.040)	(0.046)	(0.095)	(0.035)	(0.047)	(0.090)
x_promotion	0.001	0.023	−0.023	−0.047	0.050	−0.022
	(0.061)	(0.070)	(0.148)	(0.051)	(0.074)	(0.133)
x_other	−0.024	−0.018	0.011	−0.010	−0.040	0.081
	(0.053)	(0.053)	(0.115)	(0.044)	(0.056)	(0.105)
Male	−0.117***	0.157***	−0.386***	−0.121***	0.158***	−0.395***
	(0.020)	(0.025)	(0.048)	(0.020)	(0.025)	(0.048)
Age	−0.011***	0.013***	−0.030***	−0.011***	0.013***	−0.030***
	(0.003)	(0.004)	(0.007)	(0.003)	(0.004)	(0.007)
Born abroad	−0.020	0.148**	−0.249**	−0.014	0.152**	−0.256**
	(0.046)	(0.064)	(0.119)	(0.046)	(0.064)	(0.119)
At least one parent born abroad	−0.015	0.018	−0.025	−0.017	0.008	−0.014
	(0.034)	(0.047)	(0.083)	(0.034)	(0.047)	(0.083)
At least one parent w/ college degree	0.004	0.014	−0.032	0.003	0.014	−0.032
	(0.021)	(0.026)	(0.049)	(0.021)	(0.026)	(0.049)
Spoken to refugees	0.060**	0.010	0.024	0.055**	0.005	0.022
	(0.028)	(0.036)	(0.067)	(0.028)	(0.035)	(0.067)
Seen refugees	0.018	0.048	−0.090	0.012	0.047	−0.101
	(0.028)	(0.038)	(0.070)	(0.028)	(0.037)	(0.070)
Need-based student aid	0.015	−0.069***	0.114**	0.010	−0.066***	0.106**
	(0.020)	(0.025)	(0.049)	(0.020)	(0.025)	(0.049)
Master	0.016	−0.036	0.035	0.009	−0.037	0.030
	(0.028)	(0.037)	(0.069)	(0.028)	(0.037)	(0.069)
Diploma	0.005	−0.026	0.016	0.001	−0.015	0.006
	(0.027)	(0.035)	(0.068)	(0.027)	(0.035)	(0.067)
PhD	0.028	−0.117**	0.183*	0.029	−0.103**	0.178*
	(0.039)	(0.052)	(0.098)	(0.038)	(0.052)	(0.098)
Other study level	0.002	−0.046	0.044	0.005	−0.070*	0.090
	(0.035)	(0.042)	(0.084)	(0.035)	(0.042)	(0.083)
Low skilled information				−0.320***	0.358***	−0.679***
				(0.097)	(0.137)	(0.253)
University*faculty FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,227	3,227	3,227	3,242	3,242	3,242
Adj. R2	0.06	0.05	0.08	0.03	0.06	0.08

Notes: Dependent variables: agreement to statement "On average, refugees are well educated": Columns (1)+(4): binary variable (1="completely agree" or "somewhat agree", 0 otherwise); Columns (2)+(5): binary variable (1="completely disagree" or "somewhat disagree", 0 otherwise); Columns (3)+(6): integer values from 1 to 5 (1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree"; 5="completely agree"). Variables starting with "$x_$" are the respective treatment interactions with the indicated covariates (High Skilled in columns 1-3 and Low Skilled in columns 4-6). Robust standard errors reported in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A3.7 : Persistence of information treatment effects on beliefs about refugees' education level (follow-up survey)

	Agree	Disagree	Five-point scale
	(1)	(2)	(3)
Information treatment	0.357***	−0.276***	0.770***
	(0.052)	(0.053)	(0.105)
Re-survey	0.043*	−0.057*	0.106**
	(0.023)	(0.032)	(0.046)
Information treatment * re-survey	−0.143***	0.114***	−0.292***
	(0.048)	(0.042)	(0.075)
Covariates	Yes	Yes	Yes
Control mean	0.16	0.44	2.66
Information treatment effect in re-survey	0.214***	-0.162***	0.478***
	(0.054)	(0.054)	(0.103)
Observations (respondents)	281	281	281
Adj. R2	0.15	0.13	0.19

Notes: Dependent variables: agreement to statement "On average, refugees are well educated." Column (1): binary variable (1="completely agree" or "somewhat agree", 0 otherwise); Column (2): binary variable (1="completely disagree" or "somewhat disagree", 0 otherwise); Column (3): integer values from 1 to 5 (1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree"; 5="completely agree"). *Information treatment effect in re-survey* is the linear combination of the coefficients on *Information treatment* plus *Information treatment * re-survey*. *Covariates* include all characteristics from Appendix Table A3.2. Regressions only include respondents who participated in the follow-up survey and in the re-survey about one week later; see Section 3.2. Robust standard errors, adjusted for clustering at the respondent level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.8 : Persistence of information treatment effects on beliefs (follow-up survey)

	Estimates high school degree			Estimates university degree			Confidence
Outcome	Raw	Abs. deviation	Within 5pp	Raw	Abs. deviation	Within 5pp	7-point scale
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information treatment	9.260***	−4.809***	0.301***	3.572***	0.433	0.082	0.934***
	(1.605)	(1.120)	(0.054)	(1.114)	(0.866)	(0.063)	(0.160)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	21.34	14.01	0.17	13.64	6.46	0.50	3.13
Observations	281	281	281	281	281	281	281
Adj. R2	0.10	0.05	0.11	0.02	−0.06	−0.02	0.11

Notes: Dependent variables: Columns (1)-(3): respondents' estimates of the share of refugees in Germany who hold a high school degree; Col. (1): raw estimate (in percent); Col. (2): absolute deviation of estimate from the respective value provided in the information treatment in the survey one week before; Col. (3): dummy variable indicating that estimate is within 5 percentage points from the respective value provided in the survey one week before. Columns (4)-(6): respondents' estimates of the share of refugees in Germany who have a university degree; Col. (4): raw estimate (in percent); Col. (5): absolute deviation of estimate from the respective value provided in the information treatment in the survey one week before; Col. (6): dummy variable indicating that estimate is within 5 percentage points from the respective value provided in the survey one week before. Column 7: respondents' confidence (on a seven-point scale) that their estimates are correct. *Covariates* include all characteristics from Appendix Table A3.2. *Control mean* is the mean of the indicated outcome of respondents in the control group. Includes only respondents who participated in the follow-up survey and in the re-survey about one week later; see Section 3.2. Robust standard errors, adjusted for clustering at the respondent level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.9 : Effect of information treatment on participation in re-survey

	(1)	(2)
Information treatment	−0.033	−0.328
	(0.041)	(0.374)
Munich	0.391***	0.251
	(0.111)	(0.169)
Chemnitz	−0.104	−0.250
	(0.146)	(0.217)
Konstanz	0.190**	0.274**
	(0.092)	(0.133)
Male	−0.016	−0.016
	(0.044)	(0.067)
Age	−0.007**	−0.008**
	(0.003)	(0.003)
Bachelor	0.018	0.017
	(0.076)	(0.123)
Master	0.032	0.054
	(0.080)	(0.127)
PhD	0.023	−0.095
	(0.091)	(0.138)
Semester	0.001	0.002
	(0.008)	(0.011)
Born abroad	−0.167*	−0.170
	(0.100)	(0.129)
Mother born abroad	−0.018	−0.001
	(0.092)	(0.119)
Father born abroad	−0.034	0.046
	(0.080)	(0.108)
No parent has college degree	−0.013	−0.001
	(0.043)	(0.064)
Government aid	−0.002	−0.012
	(0.050)	(0.076)
Spoken to refugees	−0.006	−0.044
	(0.043)	(0.062)
Language, Culture	0.025	0.036
	(0.108)	(0.157)
Social Sciences and Pedagogy	0.083	0.055
	(0.109)	(0.169)
Law	0.097	0.006
	(0.162)	(0.242)
Commercial Information Systems	0.147	0.146
	(0.134)	(0.195)
Business and Economics	0.090	−0.068
	(0.119)	(0.172)
Maths and Science	0.149	0.136
	(0.104)	(0.153)
Medicine	0.066	0.083
	(0.125)	(0.182)
Arts and Music	−0.074	−0.152
	(0.138)	(0.208)
Information x covariates	No	Yes
Control mean	0.52	0.52
Observations	555	555
Adj. R2	0.09	0.07

Notes: Dependent variable: dummy variable that equals 1 if respondent participates in re-survey one week later; 0 otherwise. *Information treatment* indicates whether the respondent has been assigned to the information treatment group (=1) or to the control group (=0). *Covariates* include all characteristics from Appendix Table A3.2. *Control mean* is the mean of the indicated outcome of respondents in the control group. Column (2) additionally includes interactions between the information treatment dummy and all covariates. An F-test that all interaction terms in Column (2) are jointly insignificant is not rejected (p-value = 0.89). Robust standard errors reported in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A3.10 : Effect of information treatment on beliefs about refugees' education level by baseline beliefs

	Five-point scale	Agree	Disagree
	(1)	(2)	(3)
High skilled information	0.329***	0.108***	−0.140***
	(0.050)	(0.017)	(0.025)
× high baseline education belief	−0.010	0.070**	0.060**
	(0.063)	(0.029)	(0.030)
Low skilled information	−0.004	0.004	0.020
	(0.046)	(0.014)	(0.024)
× high baseline education belief	−0.226***	−0.103***	0.091***
	(0.059)	(0.024)	(0.032)
High baseline education belief	0.832***	0.176***	−0.417***
	(0.043)	(0.018)	(0.023)
Controls	Yes	Yes	Yes
Respondents	4,829	4,829	4,829
Adj. R2	0.22	0.11	0.19

Notes: Dependent variables: agreement to statement "On average, refugees are well educated": Column (1): binary variable (1="completely agree" or "somewhat agree", 0 otherwise); Column (2): binary variable (1="completely disagree" or "somewhat disagree", 0 otherwise); Column (3): integer values from 1 to 5 (1="completely disagree", 2="somewhat disagree", 3="neither agree nor disagree", 4="somewhat agree"; 5="completely agree"). Baseline beliefs about refugees' education level have been imputed for respondents in the High Skilled and Low Skilled treatments. The imputation procedure is described in Section 4.2. *Covariates* include all characteristics from Table 3.1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.11 : Effect of beliefs about refugees' education level on other refugee-related statements

	Index	Cultural enrichment	Integrate into society	Beneficial for Germany	Increase crime	Integrate into labor market	Language skills obstacle	Good for economy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Refugees are well	−0.031	−0.006	0.038	0.049	0.035	0.041	−0.018	0.061*
educated on average	(0.057)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.024)	(0.037)
Instrument F statistic	85.8	85.8	85.8	85.8	85.8	85.8	85.8	85.8
Respondents	4,824	4,824	4,824	4,824	4,824	4,824	4,824	4,824

Notes: Results from two-stage least-squares regressions. In the first stage, beliefs about refugees' education level are instrumented with the two binary indicators high skilled information and low skilled information (for first-stage results, see Table 2). Dependent variable: Column (1): summary index, consisting of the outcomes in Columns (2) to (8); see Section 3.1 for the construction of the summary index. Columns (2) to (8): dummy variables that express agreement to the indicated outcomes; 1="completely agree" or "somewhat agree", 0 otherwise. See Appendix A for the wording of all survey questions. All regressions include the characteristics reported in Table 3.1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.12 : Correlations between opinion aspects

	Humanitarian aspects	Economic aspects	Refugees' criminal behavior	Religion/culture of refugees	Refugees' willingness to integrate
Humanitarian aspects	1				
Economic aspects	-0.150***	1			
Refugees' criminal behavior	-0.252***	0.311***	1		
Religion/culture of refugees	-0.126***	0.228***	0.245***	1	
Refugees' willingness to integrate	0.00819	0.262***	0.407***	0.205***	1
Personal experience with refugees	0.184***	-0.00687	0.0288	0.118***	0.155***

Notes: Pairwise correlations between the six opinion aspects reported. Correlations are based on control group only. See Appendix Table A3.18 for the wording of all survey questions. Each aspect is measured on a five-point scale: very important, somewhat important, neither important nor unimportant, somewhat unimportant, and very unimportant. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.13 : Relationship between opinion formation aspects and general attitudes toward refugees

	Humanitarian aspects	Economic aspects	Refugees' criminal behaviour	Religion/culture of refugees	Refugees' willingness to integrate
Humanitarian aspects	1				
Economic aspects	-0.150***	1			
Refugees' criminal behaviour	-0.252***	0.311***	1		
Religion/culture of refugees	-0.126***	0.228***	0.245***	1	
Refugees' willingness to integrate	0.00819	0.262***	0.407***	0.205***	1
Personal experience with refugees	0.184***	-0.00687	0.0288	0.118***	0.155***

Notes: Pairwise correlations between the six opinion aspects reported. Correlations are based on control group only. See Appendix Table A3.18 for the wording of all survey questions. Each aspect is measured on a five-point scale: very important, somewhat important, neither important nor unimportant, somewhat unimportant, and very unimportant. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.14 : Information treatment effects on labour market and fiscal burden concerns

	Labor market concerns			Fiscal burden concerns			
	Index	Increase competition for me	Increase competition in general	Index	More revenues than costs	Pay more taxes	Less gov't benefits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High skilled information	0.070**	0.013	0.032**	0.023	–0.004	0.018	0.010
	(0.030)	(0.008)	(0.016)	(0.029)	(0.016)	(0.015)	(0.011)
Low skilled information	–0.051*	–0.006	–0.008	0.036	–0.024	0.021	0.006
	(0.029)	(0.007)	(0.015)	(0.028)	(0.016)	(0.015)	(0.011)
Respondents	4,827	4,827	4,827	4,827	4,827	4,827	4,827

Notes: Dependent variables: Column (1): index of labor market concerns, consisting of the two indicators in Columns (2) and (3). Column (4): index of fiscal burden concerns, consisting of the three indicators in Columns (5) to (7). Columns (2), (3), (5), (6), and (7): dummy variables which express agreement with the respective statement (1="completely agree" or "somewhat agree", 0 otherwise). See Appendix Table A3.18 for the wording of all survey questions and Section 3.1 for the construction of the summary indices. All regressions include the characteristics reported in Table 1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.15 : Information treatment effects on general attitudes

	Index	Admit more refugees in future	#Refugees admitted last year	Allowed to stay permanently
	(1)	(2)	(3)	(4)
High skilled information	−0.034	−0.023	0.001	−0.006
	(0.031)	(0.016)	(0.014)	(0.016)
Low skilled information	−0.017	−0.023	−0.001	−0.002
	(0.030)	(0.016)	(0.014)	(0.016)
Respondents	4,852	4,823	4,828	4,851

Notes: Dependent variables: Column (1): index of general attitudes, consisting of the three indicators in Columns (2), (3) and (4). Columns (2)-(4): dummy variables which express agreement with the respective statement (1="completely agree" or "somewhat agree", 0 otherwise). See Appendix Table A3.18 for the wording of all survey questions and Section 3.1 for the construction of the summary index. All regressions include the characteristics reported in Table 3.1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.16 : Information treatment effects on other refugee-related statements

	Index	Cultural enrichment	Integrate into society	Beneficial for Germany	Increase crime	Integrate into labor market	Language skills obstacle	Good for economy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High skilled information	−0.014	−0.002	0.012	0.023	0.008	0.023	−0.006	0.025
	(0.026)	(0.016)	(0.017)	(0.017)	(0.016)	(0.017)	(0.011)	(0.017)
Low skilled information	−0.003	0.002	−0.003	0.004	−0.008	0.008	0.001	0.001
	(0.026)	(0.016)	(0.017)	(0.017)	(0.016)	(0.017)	(0.010)	(0.017)
Respondents	4,834	4,834	4,834	4,834	4,834	4,834	4,834	4,834

Notes: Dependent variable: Column (1): summary index, consisting of the outcomes in Columns (2) to (8); see Section 3.1 for the construction of the summary index. Columns (2) to (8): dummy variables that express agreement to the indicated outcomes; 1="completely agree" or "somewhat agree", 0 otherwise. Appendix Table A3.18 for the wording of all survey questions. All regressions include the characteristics reported in Table 3.1. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.17 : Information treatment effects on opinion formation aspects

	Refugees' willingness to integrate		Humanitarian aspects		Personal experience w/ refugees	
	Important	Unimportant	Important	Unimportant	Important	Unimportant
	(1)	(2)	(3)	(4)	(5)	(6)
High skilled information	0.011	−0.002	−0.013	0.003	0.009	−0.004
	(0.011)	(0.007)	(0.012)	(0.008)	(0.015)	(0.011)
Low skilled information	0.011	0.002	−0.005	0.000	0.000	−0.004
	(0.011)	(0.007)	(0.012)	(0.008)	(0.015)	(0.011)
Control mean	0.88	0.04	0.86	0.06	0.70	0.12
Respondents	4,853	4,853	4,852	4,852	4,854	4,854
	Refugees' criminal behavior		Religion/culture of refugees		Economic aspects	
	Important	Unimportant	Important	Unimportant	Important	Unimportant
	(1)	(2)	(3)	(4)	(5)	(6)
High skilled information	−0.000	−0.017	0.028	−0.023	0.032*	−0.063***
	(0.017)	(0.015)	(0.018)	(0.017)	(0.017)	(0.017)
Low skilled information	−0.013	−0.000	−0.001	0.001	0.014	−0.026
	(0.017)	(0.015)	(0.017)	(0.017)	(0.017)	(0.017)
Control mean	0.54	0.26	0.45	0.37	0.39	0.37
Respondents	4,852	4,852	4,853	4,853	4,850	4,850

Notes: Dependent variables: importance of various aspects for respondents' opinion formation process toward refugees; respondents rated each aspect on a five-point scale: very important, somewhat important, neither important nor unimportant, somewhat unimportant, and very unimportant. *Important* equals 1 for "very important" or "somewhat important", 0 otherwise; *unimportant* equals 1 for "very unimportant" or "somewhat unimportant", 0 otherwise. All regressions include the characteristics reported in Table 3.1. *Control mean* is the mean of the indicated outcome of respondents in the control group. See Appendix Table A3.18for the wording of all survey questions. Robust standard errors reported in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A3.18 : Wording of survey questions (main survey)

Content	Wording	Type of question
Perceived education level	"On average, the refugees are well educated."	Agreement with statement, closed-ended, 5 answer categories: completely agree, somewhat agree, neither agree nor disagree, somewhat disagree, completely disagree
Labour market concerns 1: "Increase competition for me"	"The refugees will increase competition on the labour market for me personally."	See above
Labour market concerns 2: "Increase competition in general"	"In general, the refugees will increase competition on the labour market."	See above
Fiscal concerns 1: "More revenues than costs"	"The refugees will bring more revenues than costs for the government."	See above
Fiscal concerns 2: "Pay more taxes"	"Due to the government spending for refugees, I will have to pay more taxes in the future."	See above
Fiscal concerns 3: "Less government benefits"	"Due to the government spending for refugees, I will have to forgo government benefits in the future."	See above
Other concerns 1: "Cultural enrichment"	"The refugees are a cultural enrichment for Germany."	See above
Other concerns: "Integrate into society"	"Germany will succeed in integrating the refugees into society."	See above
Other concerns 3: "Beneficial for Germany"	"Generally speaking, the refugees are beneficial for Germany."	See above
Other concerns 4: "Increase crime"	"The crime rate will rise due to refugees' criminal behaviour."	See above
Other concerns 5: "Integrate into labour market"	"Germany will succeed in integrating the refugees into the labour market."	See above
Other concerns 6: "Language skills obstacle"	"Lack of language skills of the refugees are an obstacle for their labour market integration."	See above
Other concerns 7: "Good for economy"	"Overall, the refugees are good for the German economy."	See above

Notes: The table continues on the next page.

Wording of survey questions (main survey) cont'd

Content	Wording	Type of question
General attitudes 1: "Admit more refugees in future"	"Compared to the current situation, should Germany admit more refugees, less refugees, or the same number in the future?"	Closed-ended, 5 answer categories: much more, somewhat more, the same amount, somewhat less, much less
General attitudes 2: "Number of refugees admitted last year"	"What do you think about the number of refugees which Germany admitted last year?"	Closed-ended, 5 answer categories: far too many, somewhat too many, about the right amount, somewhat too few, far too few
General attitudes 3: "Allowed to stay permanently"	"Do you favour or oppose that refugees are allowed to stay in Germany permanently?"	Closed-ended, 5 answer categories: strongly favour, somewhat favor,neither favor nor oppose, somewhat oppose, strongly oppose
Aspects governing opinion formation process 1: "Humanitarian aspects"	"Humanitarian aspects"	Importance of aspect, closed-ended, 5 answer categories: very important, somewhat important, neither important nor unimportant, somewhat unimportant, very unimportant
Aspects governing opinion formation process 2: "Economic aspects"	"Economic aspects"	See above
Aspects governing opinion formation process 3: "Refugees' willingness to integrate"	"Refugees' willingness to integrate"	See above
Aspects governing opinion formation process 4: "Religion/culture of refugees"	"Religion/culture of refugees"	See above
Aspects governing opinion formation process 5: "Refugees' criminal behaviour"	"Refugees' criminal behaviour"	See above
Aspects governing opinion formation process 6: "Personal experience with refugees"	"Personal experience with refugees"	See above

Appendix A3.2 Description of the item count technique (ICT)

The item count technique (ICT) is a well-established experimental survey method to measure the extent of social desirability bias. This bias arises when respondents, instead of answering truthfully, provide answers they believe to be socially desirable (Maccoby & Maccoby 1954, Edwards 1957, Fisher 1993). Our ICT design largely follows that in Coffman et al. (2017). Respondents are randomly assigned to either a direct response group or a veiled response group. (Respondents keep their group assignment for all questions.) Participants in the direct response group are asked to answer a sensitive question directly (e.g., agreement with the statement "Economic aspects are important for my opinion formation process toward refugees"). In addition, they are asked to indicate how many other N statements they agree with. These N statements can include sensitive and nonsensitive items. We decided to include other statements on refugees that were not related to the sensitive item of interest. In contrast, respondents in the veiled response group report how many of all N+1 statements (the sensitive statement plus the N other statements) they agree with. All N+1 statements are the same as in the direct response group. The difference in the average agreement with the N+1 statements between the veiled response group and the direct response group is interpreted as the extent of under- or over-reporting due to social desirability bias. Adding this difference to the share of respondents who agree with the sensitive statement in the direct response group yields the true mean share of agreement with the sensitive statement. In addition to using the ICT technique for sensitive statements, we followed Coffman et al. (2017) and conducted an additional ICT experiment for a nonsensitive placebo item ("I used a laptop computer for completing the survey"). This (non-critical) placebo item is unlikely to be affected by social desirability bias, which means that the average agreement with the placebo item should not differ between the direct response group and the veiled response group. To compare average numbers of agreement across the two groups of respondents, the ICT requires that all items are binary. Therefore, we use dummy variables to measure our ICT outcomes in the follow-up survey experiment ("agree" versus "disagree") instead of using five-point scales as in the main survey. Note that the randomization of respondents for the information treatment was completely independent of the randomization of respondents for the ICT.

4 Entry Barriers and the Labour Market Outcomes of Incumbent Workers: Evidence from a Deregulation Reform in the German Crafts Sector*

4.1 Introduction

Regulations make opening a business an expensive and time-consuming endeavour in many countries. Potential entrepreneurs have to incur costs that range from the time needed to do the paper work to the effort of acquiring occupational licenses needed to run a business (Djankov, 2009; Ciccone and Papaioannou, 2007; Kleiner, 2000). Entry barriers are often justified based on the presumption that they secure quality standards of products and services by discouraging entry of low-quality firms (Arruñada, 2007). However, many entry barriers serve primarily the interests of incumbent firms to create rents for themselves (Stigler, 1971), which they often share with their workers (*e.g.,* Card, Devicienti and Maida, 2014; Blanchflower, Oswald and Sanfey, 1996; Hildreth and Oswald, 1997). Because incumbent firms with market power also find it optimal to restrict output and therefore also employment,[1] governments around the world have started to deregulate entry barriers to increase the efficiency of the economy (Djankov, 2009). While numerous papers show that reducing entry barriers trigger entrepreneurial activity, innovation, employment, and productivity, most of them are based on country-level, region-level, or firm-level (panel) data.[2] Much less is known about labour market consequences of reducing entry barriers for incumbent workers, which requires longitudinal worker-level data to track individuals over time.

In this chapter, we use longitudinal administrative social security data to study the labour market consequences of a deregulation reform for incumbent workers in Germany. In 2004, the German government passed a law that reformed the German Trade and Crafts Code *(Handwerksordnung)* to foster entrepreneurial activity and to reverse the negative employment trend in craft occupations. The law removed the requirement to hold the educational certificate of a Master Craftsman *(Meisterbrief)* to found a business in 52 out of 93 craft occupations (see Appendix Table A4.1 for a full list of craft occupations). The reform was very successful in increasing the number of businesses in deregulated occupations: two years after the reform the number of businesses had doubled, and had tripled after ten years (see also, *e.g.,*

* This chapter was coauthored by Philipp Lergetporer, University of Munich and ifo Institute and Jens Ruhose, Leibniz Universität Hannover.

1 See, *e.g.,* Bertrand and Kramarz (2002); Blanchard and Giavazzi (2003); Felbermayr and Prat (2011).

2 Djankov (2009) provides an overview. Section 4.3 presents a summary of the literature.

Rostam-Afschar, 2014; Runst et al., 2018*a*).[3] Occupations remained regulated if the specific trade was hazardous and quality controls were needed to prevent dangers to health or life of third parties, or if apprenticeship education activity in the trade was high. Deregulated occupations include costume tailors, weavers, tile layers, and millers, while occupations such as plumbers, electrical technicians, hairdressers, and butchers remained regulated. Overall, the reform affected a large part of the German economy because the crafts sector makes up 14 percent of social security employment, constitutes 16 percent of all businesses, and generates 10 percent of all revenue in the economy (Statistisches Bundesamt, 2016).

Our main analysis compares labour market outcomes of the same incumbent worker before and after the reform in deregulated (treatment group) and regulated (comparison group) occupations using a matched difference-in-differences approach (Heckman, Ichimura and Todd, 1997; Heckman et al., 1998; Todd, 2008). Compared to workers in regulated occupations, those in deregulated occupations are, among other things, more likely to be female, foreign-born, older, less educated, have lower average earnings, and are more likely to be employed in manufacturing. Therefore, we use entropy balancing (Hainmueller, 2012) to construct weights for each worker in the comparison group such that the average characteristics of the comparison group match *exactly* the average characteristics of the treatment group before the treatment. Compared to conventional matching, entropy balancing has the advantage that it does not take the detour via estimating a propensity score to construct a comparison group, but instead implements a non-parametric matching algorithm that reweights the comparison group observations such that they satisfy pre-specified balancing requirements. The procedure accounts for observable differences between the groups and the regression adjustment eliminates remaining unobserved individual heterogeneity.

We find that the daily gross earnings of incumbent workers in deregulated occupations grew significantly less than those of workers in regulated occupations after the reform. Over the period from 2004 to 2014, workers in deregulated occupations experienced a negative average effect on their earnings of about 2.3 percent (4 percent when using the non-matched comparison group) relative to workers in regulated occupations. Year-specific estimates show that the treatment effect becomes gradually larger over time to -4.3 percent in 2014 (non-matched sample: -7 percent). We also find that unemployment among incumbent workers increased by 0.7 percentage points (about 20 percent of the unemployment rate in our analytical sample) more in the deregulated occupations than in the regulated occupations (non-matched sample: one percentage point). Because firms in the crafts sector are usually rather small, we also examine heterogeneity by the workers' employer firm size and find that the effects for earnings and unemployment are largest among firms with less than 20 employees. The results are robust to a series of identification checks. For example, we find that pre-reform earning trends are very similar between the treatment and the comparison groups (matched

[3] There is a small, but growing literature on the effects of the reform of the German Trade and Crafts Code on various outcomes (see Runst et al. (2018*b*) for an overview). Most of the studies are concerned with the number of firms entering the market after the reform (Rostam-Afschar, 2014; Koch and Nielen, 2016; Müller, 2014, 2016; Runst et al., 2018*a*; Zwiener, 2017).

and non-matched), lending credibility to the common trend assumption.[4] We also carefully evaluate the effects of the EU enlargement in 2004 and 2007 and the economic crisis in 2009, which may have affected workers in deregulated and regulated occupations differently.

While the earnings and unemployment effects on incumbent workers are economically relevant, we argue that they are relatively small when factoring in the large increase in the number of new firms. For example, Bruhn (2011) finds that a similar reform in Mexico decreased the income of incumbent businesses by 3 percent (similar to our effects), despite increasing the number of new firms by only 5 percent (compared to over 300 percent in our setting). U.S. studies that examine deregulation reforms in the airline industry (Card, 1986, 1998; Hirsch and Macpherson, 2000) and the trucking industry (Hirsch, 1993; Rose, 1987) document relative wage decreases of up to -10 percent to -15 percent for workers in deregulated industries. The comparison suggests that the new firms in our study, which emerged after the reform in 2004, did not generate strong competitive pressure. Nevertheless, the large increase in the number of firms was sufficient to cause a slow adjustment in the pay scheme and employment structure of incumbent firms over time. The slow pace of the adjustment process is likely due to the low reform-induced competitive pressure and relatively strong labour market institutions in Germany.

Alternatively, it could be that increased labour demand by incumbent and new firms increased wage rates, which would have attenuated the negative reform effects for incumbent workers. However, we find significantly *declining* employment trends (similar to Koch and Nielen, 2016; Zwiener, 2017) and almost constant average wage trends in deregulated occupations compared to regulated occupations. Additional analyses using cross-sectional data from the German microcensus confirm these results for trends in net monthly income for both employed and self-employed craftsmen (self-employed are not included in the social security records) (see also Fredriksen, 2017). The non-positive employment trends after the reform, which are in contrast to theoretical predictions and most of the empirical literature, most likely arise because newly established firms remained one-man businesses with very little apprenticeship activity and low survival rates: about 60 percent of newly established firms had already disappeared after five years (Müller, 2014, 2016). We also discuss firm adjustments over price decreases without affecting earnings due to, for example, investments in technology, physical capital, and innovation. However, none of these economic factors seem to have changed significantly after the reform in incumbent firms. The most plausible reasons for these non-effects are that customers do not perceive goods and services offered by firms with and without a Master Craftsman certificate to be perfect substitutes (Fredriksen, Runst and Bizer, 2018). Furthermore, we provide suggestive evidence that former employees who likely become self-employed are not positively selected from the workforce (in terms of earnings in 2003), which may also explain higher failure rates relative to their established and experienced competitors. Overall, the results of this study caution the deregulation of product markets

[4] This makes it also unlikely that occupations were deregulated due to their pre-reform economic performance.

where new competitors would be too weak to exert strong competitive pressure on incumbent firms.

Our study contributes primarily to the literature that examines labour market effects of deregulation policies within countries.[5] Almost all of these studies find that reducing entry barriers leads to increasing entrepreneurial activity and overall employment growth.[6] In contrast to this chapter, most previous studies use regional- and firm-level variation in the exposure to a deregulation reform and do not exploit worker-level information. We also relate to a large U.S. literature on the effect of reducing entry barriers in specific industries on wages (see Fortin and Lemieux (1997) and Peoples (1998) for overviews). These studies show that industry wages decrease significantly after deregulating market entry. However, the evidence often relies on cross-sectional data from the current population survey (CPS), which, other than our data, does not allow to control for unobserved worker characteristics.

The remainder of the chapter is structured as follows. Section 4.2 provides the institutional background and explains the reform in detail. Section 4.3 discusses the theoretical background and the related literature. Section 4.4 introduces the data. Section 4.5 presents our main analysis of the reform effects on incumbent workers' labour market outcomes. Section 4.6 sets our findings in perspective to the overall reform effects on employment and wage growth, and on self-employed labour market outcomes. Section 4.7 provides a discussion of our findings and connects them with the current literature. Section 4.8 concludes.

4.2 Reform of the German Trade and Crafts Code 2004

Crafts occupations such as carpenters and stonemakers have historically played an important role in the German economy and continue to do so until today (Statistisches Bundesamt, 2016). Since the middle ages (with short interruptions), craft occupations had been strictly regulated and monitored by guilds (Brenke, 2008). The role of the guilds was to regulate the supply of craftsmen through licenses and apprenticeship training, protect its members through contract enforcement, reduce information asymmetries on quality as well as invest in human capital and technology (Ogilvie, 2004, 2014).

In more recent years and until 2004, the supply of craftsmen had been regulated in two ways in Germany. First, only individuals with a "Master Crafts Certificate" *(Meisterbrief)* were able to open their own business and offer services in their trade. Second, only master craftsmen were allowed to educate apprentices. The Master Crafts Certificate is a professional qualification administered by the chambers of crafts and trade in the respective federal states. The certificate requires roughly two years of coursework and costs for the examination vary between two to ten thousand Euros (ZDH, 2014). As prerequisite for the Master Craft Certificate exams,

[5] Section 4.3 provides a detailed literature review.

[6] See studies by Aghion et al. (2008), Bertrand and Kramarz (2002), Branstetter et al. (2014), Bruhn (2011), Kaplan, Piedra and Seira (2011), Mullainathan and Schnabl (2010), and Yakovlev and Zhuravskaya (2013).

craftsmen have to successfully finish their apprenticeship in their respective craft trade and usually have to acquire some work experience as a journeymen.[7]

In 2004, the German government deregulated the access to self-employment by removing the requirement to hold a master crafts certificate for 52 out of 93 craft occupations (see Appendix Table A4.1 for a full list of regulated and deregulated craft occupations). The other 41 occupations remained regulated with the exception that journeymen who had worked for at least six years in their occupation (four of them in a leadership position) also received the option to open their own business in 35 occupations.[8] The reform goals for the amendment of the German Trade and Crafts Code were to reverse the negative development in the number of craftsmen and apprenticeships, ease entry into entrepreneurial activity, and ensure future employment. The reform was first announced at Chancellor Gerhard Schröder's government declaration[9] in March 2003 and then quickly came into force on January 1, 2004, less than one year after the initial declaration.

The reform of the German Trade and Crafts Code was a less prominent and less widely known part of a comprehensive welfare and labour relation reform package aimed at enhancing economic growth and increase competitiveness referred to as "Agenda 2010" or the "Hartz Reforms". Implemented by the coalition government between the Social Democrats and the Green Party, the Hartz reforms were a reaction to a decade of disappointing economic performance in the post reunification area.[10] The Hartz reforms did not affect institutions involved in the wage setting process, but were primarily concerned with limiting unemployment benefits, liberalizing temp agency work, reforming active labour market policies, and reorganizing the Federal labour Agency. While some argue that the reform explains at least some of the improvements in competitiveness of the German industrial sector, some others argue that the Hartz reforms were not essential (Dustmann et al., 2014).

Occupations remained regulated for two reasons. First, security concerns for customers prevented the deregulation of occupations with tasks that need high quality control. In total, 31 occupations remained regulated for this reason. Second, 11 occupations remained regulated because they had already a high apprenticeship education activity. The second restriction argument was added as a compromise following political negotiations between opposing political parties, representatives of employers, labour unions, and employees. As an overall result of these negotiations, deregulated occupations represent only 11 percent

[7] Journeymen get their name from the old custom that after passing their apprenticeship examination, young craftsmen left their master to go on their journey and work in different locations to acquire new knowledge and techniques. In modern times, this custom has mostly seized to exist.

[8] However, Figure 4.1 indicates that this weaker deregulation did not affect the number of firms in these occupations.

[9] Gerhard Schröder's Government Declaration: http://gerhard-schroeder.de/2003/03/14/regierungserklarung-agenda-2010/

[10] At that time, Germany was "the sick man of Europe" (The Economist, 2004). In 2003, the year before the reform was introduced, the German economy was in recession, with high labour cost and an unemployment rate of 11.6 percent (Statistisches Bundesamt, 2018*a*).

of craft businesses in 2003. Further anecdotal evidence suggests that regulated occupations were prominently represented in the constituencies of the governing politicians. This is consistent with the fact that some occupations are very similar to each other, yet one has remained regulated (*e.g.,* metal worker) and the other has been deregulated (*e.g.,* metal and bell founder).

The reform had strong effects on business formation and entrepreneurial activity. Using aggregated firm registry data from the German Confederation of Skilled Crafts *(Zentralverband des Deutschen Handwerks (ZDH))*, Figure 4.1 displays the changes in the number of businesses in the deregulated occupations and in the regulated occupations (relative to 2003). While the number of businesses in the still regulated occupations remained relatively constant, the number of businesses in deregulated occupations doubled between 2003 and 2006 and more than tripled between 2003 and 2014. On average, the stock of businesses in deregulated occupations increased annually by 26 percent in the first three years and then increased further by about 5 percent each year from 2007 to 2014.[11]

Using data from the German Microcensus[12], Figure 4.2a shows the shares of self-employed individuals in both the deregulated and regulated occupations (see Section 4.4.2 for description of the data). The picture reveals an increase in the share of self-employed craftsmen in deregulated occupations.[13] As may be expected, another effect of the reform was a drastic and immediate decline in the number of Master Craft examinations in the deregulated occupations. As shown in Figure 4.2b, Master Craft Certificate examinations decreased by 60 percent after the reform (see also Koch and Nielen, 2016). Examinations do not fall to zero because they are still offered and remained to have a value by signalling high quality standards (Fredriksen, Runst and Bizer, 2018).

4.3 Labour Market Effects of Entry Barriers: Related Literature and Some Theory

Entry barriers are costs that new entrants have to incur to become active in a given product market. These costs can range from standardization of products, minimum capital requirements, and time-consuming registration procedures to licenses that are required to run a

[11] Supporting evidence on the role of entry barriers in the German crafts sector on the likelihood of becoming self-employed comes from Prantl and Spitz-Oener (2009). They use the German reunification as a natural experiment to show that the entry barriers impeded entry into self-employment in East Germany compared to entry in West Germany. Prantl (2012) further shows that entry barriers suppressed short-lived and long-lived entrants.

[12] Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

[13] Columns (7) and (8) of Table 4.8 confirms an increase of more than 2 percent among self-employed individuals in deregulated compared to regulated occupations after the reform. This finding confirms Rostam-Afschar (2014) and Runst et al. (2018*a*) who study the effects of the reform on entry into self-employment more rigorously. See also Koch and Nielen (2016); Müller (2014, 2016); Zwiener (2017).

business (Djankov, 2009; Kleiner, 2000). Advocates for entry barriers argue that they can secure and improve the quality of the products and services provided (Arruñada, 2007). For example, Anderson et al. (2016) show that occupational licensing in medical professions can be highly beneficial because the licensing of midwives in the early 20th century in the United States led to reductions in infant and mother mortality. However, opponents argue that entry barriers lead to inefficient allocations of resources because they restrict competition and create rents for incumbent firms (Peltzman, 1976; Posner, 1975; Stigler, 1971).[14] With entry barriers in place, theory usually predicts that incumbent firms produce less output and charge higher prices than they would in a more competitive environment.[15]

While the effects on wages and employment of reducing entry barriers are theoretically ambiguous and seem to depend on a multitude of factors, such as the level of initial competition, labour market institutions, and political economy factors as well as the technology and the specific industry in question (see Djankov (2009) for a survey of the literature), standard economic theory predicts that reducing entry barriers for firms lead to increasing entrepreneurial activity through increasing the number of active firms. Evidence in favour of this theory comes from numerous cross-country studies,[16] and also from studies examining deregulation reforms within countries by using regional and time variation in the reform exposure.[17] For example, Bruhn (2011) and Kaplan, Piedra and Seira (2011) document that a reform in Mexico, which simplified the local registration process for new businesses, has increased the number of new businesses by 5 percent. Branstetter et al. (2014) report an increase of 17 percent for a similar reform in Portugal. Aghion et al. (2008) report an average increase in the number of factories by 6 percent after a large deregulation reform in India, which removed licensing requirements to establish a new factory, expand capacity, start a new product line, or change location for entire industries.

Because incumbent firms with market power find it optimal to restrict output and therefore also employment (Bertrand and Kramarz, 2002; Blanchard and Giavazzi, 2003; Felbermayr and Prat, 2011), deregulation and new firm entry should predict increases in employment levels. Most of the literature confirms this link at the country and regional level. For example, Bertrand and Kramarz (2002) and Ciccone and Papaioannou (2007) show that excessive entry barriers harm employment growth. Other studies show that stringent anticompetitive product market regulation increases unemployment (Bassanini and Duval, 2006; Feldmann, 2008; Griffith, Harrison and Macartney, 2007). If incumbent firms and new entrants are effective

[14] De Soto (1989) argues that entry barriers may also benefit bureaucrats and politicians by collecting bribes from entrants. (Mukoyama and Popov, 2014) provide a political economy model for the existence and dynamics of entry barriers.

[15] See, *e.g.,* Djankov (2009); Gittleman, Klee and Kleiner (2018); Kleiner and Krueger (2013); Weeden (2002).

[16] See, *e.g.,*Ciccone and Papaioannou (2007); Dreher and Gassebner (2013); Djankov et al. (2002); Fisman and Sarria Allende (2010); Klapper, Laeven and Rajan (2006).

[17] Studies exist for Belgium (Schaumans and Verboven, 2008), France (Bertrand and Kramarz, 2002), Peru (Mullainathan and Schnabl, 2010), Portugal (Branstetter et al., 2014), Mexico (Bruhn, 2011; Kaplan, Piedra and Seira, 2011), India (Aghion et al., 2008), and Russia (Yakovlev and Zhuravskaya, 2013).

competitors, meaning that they engage in similar markets (market commonality) and compete for similar resources (resource similarity) (Chen, 1996), then incumbent firms may react to increased competition by increasing investments (Alesina et al., 2005) and innovative activity (Aghion, Howitt and Prantl, 2015; Griffith, Harrison and Simpson, 2010) to keep their long-run competitive advantage. While new firm entry can also be detrimental to innovation and growth by diminishing rents and thereby decreasing incentives to innovate and invest (Aghion et al., 2005),[18] evidence for the United Kingdom and cross-country studies show a positive effect of deregulation on innovation (Aghion et al., 2009; Blundell, Griffith and van Reenen, 1999). Thus, it is not surprising that most studies eventually find beneficial effects of deregulation reforms on productivity and economic growth.[19] For consumers, the literature shows that deregulation reforms lead to lower prices in the affected industries and sectors. For example, Bruhn (2011) confirms that prices decrease after deregulation in Mexico. Schivardi and Viviano (2011) study the market effects of barriers to entry in the Italian retail market and find that higher entry barriers lead to higher consumer prices. Bertrand and Kramarz (2002) confirm that for the French retail industry. Thus, an overwhelming part of the literature shows that there are mostly positive economic effects from removing entry barriers.

In this study, we are mainly interested in understanding potential earnings and employment outcomes of workers who work in a firm that is affected by a deregulation. In many cases, the main beneficiaries of entry regulations are incumbent firms, which are protected from competition by entry barriers and can therefore raise economic rents through charging markups on prices.[20] There is strong evidence that firms share their economic rents with their employees, causing higher wages in many regulated markets and industries.[21] To protect their product market position, firms may try to save costs following a deregulation reform by revising wages of their workers. They may do that by re-negotiating labour contracts and collective bargaining agreements with labour unions (Peoples, 1998). However, to retain their competitive advantage, firms may also choose to invest in the human capital of their workforce because firms with more skilled labour work more efficiently. Evidence on this channel comes from Fernandes, Ferreira and Winters (2014) and Guadalupe (2007) who show that returns to skills increase after a deregulation and from Bassanini and Brunello (2011) who show that firms invest more in the training of their employees. Earnings for incumbent workers may also raise when labour demand increases because of increasing product demand. Furthermore, because incumbent firms may invest more in technology and innovation, they might be able to keep their competitive advantage without reducing earnings of their workers. Thus, theoret-

[18] Aghion et al. (2009) argue that it depends on the specific industry and its' technological advancement how firms react to increasing competition. Thus, a higher entry threat can encourage innovation in sectors that are initially close to the technological frontier, whereas it may discourage innovation in sectors that are initially far below the technological frontier.

[19] See, for example, Aghion et al. (2004, 2005, 2008, 2009); Barseghyan (2008); Dawson and Seater (2013); Djankov, McLiesh and Ramalho (2006); Schivardi and Viviano (2011).

[20] See, *e.g.,* Djankov (2009); Gittleman, Klee and Kleiner (2018); Kleiner and Krueger (2013); Weeden (2002).

[21] See, for example, Arai and Heyman (2009); Card, Devicienti and Maida (2014); Christofides and Oswald (1992); Blanchflower, Oswald and Sanfey (1996); Guertzgen (2009); Hildreth and Oswald (1997); Rusinek and Rycx (2013).

ically, whether deregulation increase or decrease wages of incumbent workers is ambiguous. Moreover, some labour markets (such as the German labour market) are characterized by substantial downward rigidity of wages (*e.g.,* through strong labour unions or high minimum wages).[22] If firms cannot easily revise wages, they may choose to adjust their labour costs over the employment margin.

Thus, while the economy-wide effects of reducing entry barriers are likely positive for employment, innovation, and productivity, those policies may harm incumbent workers by decreasing their wages and perhaps increase unemployment. However, in the longer run, incumbent workers may benefit from additional employment opportunities offered by new firms, raising average wages in the labour market due to increasing labour demand. Thus, it is important to distinguish conceptually between long- and short-run welfare effects for incumbent workers because the reduction and redistribution of rents through deregulation may also induce firms to adjust over longer periods (Blanchard and Giavazzi, 2003).

There is a large U.S. literature on the effect of reducing entry barriers in specific industries on earnings (see Fortin and Lemieux (1997) and Peoples (1998) for overviews).[23] Most of these studies show that industry wages decrease significantly after deregulating market entry. For example, workers in the airline industry (Card, 1986, 1998; Hirsch and Macpherson, 2000) and the trucking industry (Hirsch, 1993; Rose, 1987) see their wages decrease by between 10 percent to 15 percent after a major entry deregulation. MacDonald and Cavalluzzo (1996) find that wages in the railroad industry first increase, but then decrease substantially—indicating the importance of studying long-run patterns. However, the evidence often relies on cross-sectional data from the current population survey (CPS), which does not allow to control for unobserved worker characteristics. Moreover, because reform effects depend on the predetermined characteristics of the labour and product market, labour market effects from the deregulation of one entry barrier cannot easily be generalized to the deregulation of other entry barriers in other markets.

Other studies that evaluate deregulation reforms within countries rely on regional variation in the exposure to a deregulation reform. For example, Bruhn (2011) provide some evidence that deregulation may also reduce wages of incumbent employees in Mexico. The reform that increased the number of new firms by only 5 percent decreased the income of incumbent businesses by 3 percent, which is comparable to our effect size. Bertrand and Kramarz (2002) also suggest negative earnings effects for a deregulation in the French retail sector.

[22] Fiori et al. (2012) document an interaction between product market and labour market regulation in a panel of OECD countries. They show that product market deregulation is more effective at the margin when labour market regulation is high. See also Blanchard and Giavazzi (2003), Ebell and Haefke (2009), Felbermayr and Prat (2011), and Koeniger and Prat (2007) for theoretical contributions.

[23] Studies include sectors such as banking (Black and Strahan, 2001; Cuñat and Guadalupe, 2009; Wozniak, 2007), airline (Card, 1986, 1998; Hirsch and Macpherson, 2000), trucking (Hirsch, 1993; Rose, 1987), railroads (MacDonald and Cavalluzzo, 1996), and telecommunication (Majumdar, 2015).

Hardly any evidence exists about the long-run employment prospects of incumbent workers after a deregulation reform and none of these studies use longitudinal (administrative) worker-level data. Based on the theoretical and empirical findings from the literature, we can derive some expectations about the labour market effects of the reform of the German Trade and Crafts Code. Compared to other deregulation reforms studied in the literature, the evidence that we present in Section 4.2 indicates that the reform led to a massive increase in the number of new firms. Under the assumption that those firms compete with incumbent firms by targeting similar consumers and providing comparable goods and services, we should expect that the reform placed competitive pressure on prices in the same craft occupations.

If incumbent firms want to retain their profit margins, we may observe decreasing earnings over time as firms obtain the option to re-negotiate wage contracts. We may also observe increasing unemployment. However, because most craft services are labour intensive, we should observe that employment is increasing in deregulated craft occupations with the establishment of new firms. Incumbent firms may try to retain and train their workforce, which may lead to average earnings growth in the long-run. Therefore, the overall reform effect is an empirical question.

There already exists a set of related studies that look at the economic effects of the reform of the German Trade and Crafts Code (Runst et al., 2018*b*, provide an overview). The burgeoning literature finds surprisingly little positive effects on overall employment and wage growth in deregulated occupations for self-employed and employees (Koch and Nielen, 2016; Fredriksen, 2017; Zwiener, 2017). However, these studies are based on either cross-sectional or firm-level data, which does not allow them to track the same individual over time. Some sociological studies use cross-sectional German microcensus data to look at wages of workers in occupations that are regulated versus those that are not regulated (Bol, 2014; Bol and Weeden, 2015). However, this approach informs us about wage-level differences across those occupations, but does not inform us about the causal effects of the deregulation reform.

Another study from sociology comes from Damelang, Haupt and Abraham (2018). Using SIAB data and a simple difference-in-differences design, they concentrate on incumbent worker earnings, but do not study unemployment. Their analysis stops already four years after the reform and they do not provide year-specific treatment effects. However, the literature implies that it is important to distinguish conceptually between long- and short-run welfare effects because the reduction and redistribution of rents through deregulation may also induce firms to adjust over longer periods. Moreover, we analyze pretreatment trends in detail and correct for different sample compositions, which turns out to matter for the results.

4.4 Data

4.4.1 Sample of Integrated Labour Market Biographies (SIAB)

Our main analysis uses the German Sample of Integrated Labour Market Biographies (SIAB) from the Institute for Employment Research (IAB) (Antoni, Ganzer and vom Berge, 2016).[24] The dataset provides detailed administrative longitudinal earnings records on a two percent random sample of individuals who are subject to social security. We also have information on the detailed occupation, educational background, industry, employment status, and some information about the firm.

The analytical sample consists of full-time employees between 25 and 55 years old in the year of the deregulation, 2003, who work in either a deregulated or regulated craft occupation in 2003. To avoid (potentially endogenous) switching before the reform, we additionally require that the individual has held the same occupation within the same firm for three years prior to the reform. We also only look at individuals who report more than ten Euro of daily wages in the five years before the treatment, *i.e.*, from 1998 to 2003. This should not be a binding restriction for full-time regular employees (Card, Heining and Kline, 2013). Finally, we drop employees who work in large firms, *i.e.*, firms with more than 1500 employees, because craftsmen firms are usually small to medium firms. It is likely that this sample may still include firms that are not registered in the crafts sector (*e.g.,* firms may belong to the industrial sector or the public sector) because the SIAB data do not allow us to exactly identify those firms. Because those firms are not directly affected by the reform, removing entry barriers in the craft sector should not have a direct effect on their workers. However, the reform may have an indirect effect on those workers in the long-run because workers within the same occupation are able to switch across industry boundaries. This is why we include also workers who work in larger firms and who potentially do not work in the crafts sector.[25] In further analyses, we show results by firm size because small firms (*e.g.,* with less than 20 employees) are most likely to be crafts firms.

To assign whether an individual is employed in a regulated or deregulated craft occupation, we take the official Trade and Crafts Code, listing the names of occupations which remain regulated (*Annex A*) and which were deregulated (*Annex B1*). We map these occupations into the 3-digit German Classification of Occupations 1988 (KldB88), which the SIAB data use to classify occupations. Appendix Tables A4.2 and A4.3 show the mapping of occupations from

[24] This study uses the weakly anonymous Sample of Integrated labour Market Biographies (years 1975-2014). Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

[25] Runst et al. (2018*a*) propose to circumvent this assignment problem by including only occupations where the share of craft apprentices within each occupational code exceeds 60 percent. Data for this exercise comes from the Federal Institute for Vocational Education and Training. Replicating the results of Rostam-Afschar (2014), they find (as expected) stronger reform effects on self-employment in the German microcensus. However, dropping entire occupations from the sample because of low apprenticeship activity may lead to an endogenous sample selection. We prefer to split the sample by firm size to capture firms registered in the crafts sector.

the Trade and Crafts Code into the KldB88. For 38 occupations, the occupation name in the Trade and Crafts Code and in the Classification of Occupations 1988 are identical.[26] Further 39 occupations can be matched without any difficulties because the occupation names only differ slightly, such as using a more modern word for the occupation. Thirteen occupations where the mapping is not so obvious, are mapped from the Crafts and Trade Code using digressions and further research. These include cases where the 3-digit KldB88 is not detailed enough to list the specific occupation separately. For them, we look at the 4-digit level of the classification to see in which code the occupation is included. Only four small occupations cannot be matched at all.

To study also economy-wide reform effects (see Section 4.6), we also use the SIAB data to look at overall changes in employment and earnings in deregulated and regulated occupations before and after the reform. For this analytical sample, we keep all employees who are employed or unemployed and are between 20 and 60 years old. We again drop workers who work in firms that have more than 1,500 employees. For the year 2003, this sample shows that about 11.7 percent work in craft occupations.[27] The data further show that 9.3 percent of all employment subject to social security is in regulated occupations, 2.4 percent is in deregulated occupations, 14 percent are unemployed, and 74 percent work in other occupations not related to the craft sector.

4.4.2 Microcensus

Because the SIAB data only contain dependent employees who are subject to social security, we also use data from the German Microcensus to provide evidence on the earnings effects on self-employed individuals (FDZ der statistischen Ämter des Bundes und der Länder, 2012). Moreover, we can check the robustness of our results from the SIAB data by studying the impact of deregulation on the net income of employees.

The German Microcensus is a compulsory household survey, which contains a 1 percent random sample of persons and households in Germany.[28] The survey provides basic socio-demographic and labour market data in the form of yearly repeated cross sections. The advantage of this survey is that answering the questions is mandatory, which avoids selective non-response.

For our analysis, we use the waves from 1997 to 2012. The microcensus does not report income by different income sources. To avoid the inclusion of income that is, *e.g.,* raised

[26] This also counts cases in which the Trade and Crafts Code occupations matches two occupations in the KldB88, such as “Bricklayer and Concretor”, which is one occupation in the Craft and Trade Code but corresponds to the occupations of “bricklayer” and “concretor” separately in the KldB88.
[27] The number is comparable to official statistics for employment in the crafts sector for the year 2014 (Statistisches Bundesamt, 2016).
[28] We use the full 1 percent sample of the scientific use file (SUF), which is only available for on-site use at Research Data Centers of the Federal Statistical Office and the statistical offices of the Länder.

through capital and real estate, we keep only individuals who report labour earnings as their primary source of net income. The analytical sample consists of individuals who work in either regulated or deregulated craft occupations, are between 18 and 65 years of age, and work full-time. The microcensus uses the Classification of Occupations 1992, which is similar to the 1988 classification in the SIAB. Based on the 4-digit 1992 classification of occupations, we use the same procedure to classify occupations into deregulated and regulated reform occupations (see Appendix Table A4.4 for the mapping).

4.5 Reform Effects on Incumbent Workers

4.5.1 Basic Empirical Setup

Using the administrative SIAB data, we track the labour market performance of individuals who worked in deregulated occupations before the reform in the year 2003 and compare them to individuals who worked in regulated occupations. According to our sample setup, workers are in the treatment group if they held an occupation in the years 2001, 2002, and 2003 that was eventually deregulated. Workers are in the comparison group if they have worked in an occupation that remained regulated. Equation (4.1) shows the basic setup of the resulting difference-in-differences model.

$$y_{it} = \alpha_1 + \alpha_2 deregulated_{j(i)} \times post_{2003} + \mu_t + \mu_i + \epsilon_{it} \tag{4.1}$$

where y are labour market outcomes (log daily earnings and unemployment) for worker i at time t. The variable $deregulated$ is an indicator variable that is one if occupation j is deregulated and zero otherwise. Assignment of occupation j to individual i is based on the last year before the reform in 2003. $Post$ is one if year t is after the reform in 2004 and zero for years before the reform. We are mainly interested in the interaction between $deregulated$ and $post$, where the coefficient α_2 gives the average reform effect for workers who work in a deregulated occupation compared to someone who works in a regulated occupation. The effect is causal if the treatment group would have developed in the same way as the comparison group had there been no treatment *(common trend assumption)*. μ_i are individual fixed effects, which partial out unobserved individual heterogeneity. μ_t are time fixed effects that take out common time specific effects. ϵ_{it} is an error term that is clustered at the individual level.

4.5.2 Covariate Balancing before Treatment

Table 4.1 displays the mean and variance for a large set of individual covariates for the treatment and comparison group. Comparing means across the two samples, the table shows that the treatment and comparison group are very different in their characteristics prior to

the reform. Individuals in regulated occupations compared to individuals in deregulated occupations constantly earn on average six to seven percent more over the entire period from 1998 to 2003. Some reasons for this persistent earnings gap are that individuals in deregulated occupations, compared to those in regulated occupations, are more likely to be female, more likely to have a foreign citizenship, and less well educated. Because employees in deregulated occupations are older on average than employees in regulated occupations, they also have higher job and firm tenure. Another difference is that 77 percent of workers in deregulated occupations work in the manufacturing sector, where this is only the case for 38 percent in regulated occupations. Workers in regulated occupations are very likely to work in the construction sector (34 percent versus 6 percent in deregulated occupations) because the construction sector comprises many occupations with hazardous crafts. This also explains why the tasks of regulated compared to deregulated occupations are more likely to be categorized as complex specialized tasks than professional tasks.

The table also documents that regulated and deregulated occupations differ in terms of covariate variances. For example, the standard deviation of the log earnings variables show that variances in the treatment group are larger than in the comparison group across all periods. Studying earnings growth prior to the treatment at the bottom of the table, we observe that there are only small differences in mean earnings growth. From 1998 to 2003, earnings in the deregulated occupations have increased by 23.3 percent on average. Earnings growth in the regulated occupations was very similar at 24.3 percent. However, the variance in the earnings growth is twice as large in the regulated compared to the deregulated occupations. This difference gets even stronger when looking at the earnings growth since 1994.

4.5.3 Regression-adjusted Matched Difference-in-Difference

Large compositional differences between the deregulated and regulated groups are a potential threat to the common trend assumption of the simple difference-in-difference estimator because it is unclear whether the two groups would have developed similarly without the reform. Moreover, because reform effects may be driven by a particular population group, compositional differences could also complicate the interpretation of the results. While the overrepresentation of an affected population group does not bias estimates of the causal reform effects (given that the common trend assumption holds), it may undermine external validity because the effects are scaled up or down depending on the relative weights of the population group that is affected most. Thus, an analysis that is based on samples that are comparable before the reform may more likely reflect the reform effect for a randomly drawn employee from one of the deregulated occupations.

To allow for an apple-to-apple comparison that is not driven by compositional effects, we use entropy balancing to make the comparison group as similar as possible to the treatment group before the reform. Hainmueller (2012) formulates entropy balancing as a non-parametric data pre-processing method for binary treatment studies. The method incorporates covariate

balancing in the first and second moments (and potentially higher moments) of the covariate distribution directly into a maximum entropy reweighting function. By doing that, the approach constructs weights $w(i,k)$ (with i indicating individuals in the treatment group and k in the comparison group) for each observation in the comparison group such that prespecified balancing constraints are exactly fulfilled. In this study, we require exact balancing on all covariates reported in Table 4.1. Showing covariate balancing after reweighting becomes redundant because means and variances of all covariates of the comparison group are by construction identical to the means and variances of the treatment group. The advantages of entropy balancing compared to classical propensity score matching are that (i) iterative and somewhat arbitrary searches for a matching function becomes redundant, (ii) that the method retains all observations and merely reweights them, which is helpful in reducing standard errors in the estimation, and (iii) most importantly that it can also handle differences in the variance of covariates. Especially the last point seems to be important because Table 4.1 reveals large differences in the earnings variance prior to the reform between the treatment and comparison group.

Using the weights $w(i,k)$, Equation (4.2) formulates the estimator for the treatment effect (Heckman, Ichimura and Todd, 1997; Heckman et al., 1998; Todd, 2008). In this setting, n_1 is the number of treated individuals and group membership is indicated by I_1 (treated) and I_0 (comparison), respectively. The counterfactual comparison group is a weighted average of the change in outcome variables with weights equal to $w(i,k)$.

$$\widehat{\alpha}_2 = \frac{1}{n_1} \sum_{i \in I_1} \left[(Y_{1i}^{after} - Y_{0i}^{before}) - \sum_{k \in I_0} w(i,j)(Y_{0j}^{after} - Y_{0j}^{before}) \right] \tag{4.2}$$

The estimator is implemented in a difference-in-differences regression (see Equation (4.1)) with each individual weighted by $w(i,k)$ and standard errors clustered at the individual level. The advantage of the regression adjustment is that we can additionally partial out time-invariant selection on unobservables by including individual fixed effects.

4.5.4 Main Results

We start by plotting average earnings of the treatment group and the (non-matched and matched) comparison group over time in Figure 4.3. Prior to 2003, we observe that workers in our analytical sample perform reasonably well with steadily increasing nominal earnings.[29] Supporting the common trend assumption, both groups perform very similar over the entire period from 1994 to 2003. After the reform, however, earnings trajectories start to diverge. While earnings in regulated occupations seem to further increase, the earnings growth in the deregulated occupations is notably flatter. Compared to the hypothetical earnings growth of

[29] Note that the steady earnings growth is likely an artefact of the sample selection where we require that each individual reported strictly positive earnings from 1998 to 2003. Earnings are also not adjusted for inflation.

the matched comparison group, we observe a growing earnings gap between treatment and comparison group.

Using Equation (4.1) to estimate the average earnings effect of the reform from 2004 to 2014, Table 4.2 reports that gross daily earnings of incumbent workers in deregulated occupations decrease relative to the earnings of workers in regulated occupations by about 4 percent (Column (1)). Weighting the comparison group by matching weights, the effect halves to -2.3 percent (Column (2)). In Figure 4.4a, we plot coefficient estimates and 95 percent confidence intervals of yearly treatment effects to study the dynamics of the effect over time (see also Appendix Table A4.5). In the non-matched sample, we see that the negative effect on daily earnings becomes significant in 2007, three years after the reform. Daily earnings are 2 percent lower for incumbent workers in deregulated than in regulated occupations at this time. The effect becomes more negative over time and decreases to -7 percent in 2014. The reform effect becomes gradually larger over time because new firms may need some time to be established and incumbent firms need some time to adapt to the reform. The reform effect may also become larger because wage rates that are negotiated in collective bargaining agreements need some time until they can be changed and renegotiated. Results for the matched sample are generally smaller, but not significantly different from the non-matched results. In this sample, we already observe a significant negative coefficient directly in the year after the reform. While estimates show some recovering in the years 2005 and 2006, earnings decrease to -4.3 percent in 2014. Thus, it seems that the reform has caused a lower earnings growth for incumbent workers in the deregulated occupations compared to the earnings growth in the regulated occupations.

Effect sizes are comparable to bargained earnings increases in collective bargaining agreements, which are recently around two to three percent of earnings.[30] However, the effects of deregulation appear rather modest when compared to the effects identified by Goldschmidt and Schmieder (2017) who document wage losses for jobs that have been outsourced of about 10 percent to 15 percent. The same is true when compared to wage losses after deregulation reforms in the U.S. airline industry (Card, 1986, 1998; Hirsch and Macpherson, 2000) and the trucking industry (Hirsch, 1993; Rose, 1987), which document wage decreases by between 10 percent to 15 percent after deregulation. The results are more comparable to the findings of other deregulation reforms, *e.g.,* in Mexico (Bruhn, 2011; Kaplan, Piedra and Seira, 2011), only that the increase in the number of businesses due to the deregulation reform is much higher in our study. Moreover, when clustering standard errors at the occupation level to allow for arbitrary correlation of errors across workers within the same occupation, we cannot reject that the effects are statistically different from zero. This casts first serious doubts on whether the reform has induced strong competitive pressure.

[30] The yearly average collective agreement wage increase for all industries from the years 1998 to 2016 lies at 2.3 percent, at 2.1 percent in the construction industry and at 2.5 percent in the raw materials and capital goods industry (see collective agreement database of the Hans-Böckler Foundation (2018)).

Firms may also react over the extensive margin by laying off workers. Therefore, Columns (3) and (4) in Table 4.2 report the results from linear probability models on the average unemployment rate in our sample. Compared to the matched (non-matched) comparison group, workers in deregulated occupations face a higher unemployment risk of 0.7 percentage-points (one percentage-point) on average. To be categorized as unemployed for this analysis, the employment records have to list the worker as unemployed or that the worker is searching for a job while currently employed.[31] Figure 4.4b shows point estimates and 95 percent confidence intervals for yearly treatment effects. While we do not observe effects for the first three years, in 2007 and 2008 the probability of being unemployed increases to statistically significant 0.8 to 0.9 percentage-points.[32] The probability peaks in 2009 with 1.5 percentage-points and then hovers around one percentage-point until 2014.

In the interpretation of the effect size, we have to keep in mind that the sample consists only of individuals who held stable jobs within the same firm between 2001 and 2003. Thus, while the unemployment rate in this sample is zero in 2003, 3.5 percent of the workers experienced some unemployment in the period between 2003 and 2014. This indicates that the effect is about 20 percent $(= 0.7/3.5)$ of the relative long-term unemployment probability of the sample. However, because overall employment in deregulated occupations is rather small (in 2003, only 2.4 percent of all employment subject to social security was in deregulated occupations; see Section 4.4.1.), the impact on the nation-wide unemployment rate may also be small.

In further analyses, Appendix Table A4.6 shows that the reform tended to decrease full-time regular employment and increased marginal employment. However, results are not significantly different from zero when using the matched comparison group. Therefore, we conclude that this adjustment mechanism is mainly driven by the different sample compositions and less likely to be a direct effect of the reform.

4.5.5 Identification

The identifying assumption of these estimations is that earnings and employment in the deregulated occupations would have developed equally compared to earnings and employment in the regulated occupations in the absence of the reform *(common trend assumption)*. We already presented visual evidence that this seems to be the case in our sample (see Figures 4.3 and 4.4).[33] Table 4.1 also already documented that there is barely any economically meaningful difference in the average earnings growth between the treatment and comparison

[31] We do not consider individuals who drop out of the sample or report zero earnings.
[32] Effects are again not significant when clustering at the occupation level.
[33] Appendix Figure A4.1 plots average gross daily log earnings of incumbent workers in the regulated and deregulated occupations relative to the year 2003. The figure reiterates that trends before the reform are very similar.

group.[34] Those non-significant pretreatment trends in the non-matched sample suggest that differences between the matched and non-matched reform effects are more likely due to different compositions of workers in regulated and deregulated occupations and not due to different pre-reform economic trends.

To investigate common trends in pretreatment periods more formally, we move the reform to the year 1998 to check whether the earnings pattern has already diverged between regulated and deregulated occupations before 2003 in our baseline sample.[35] Results in Appendix Table A4.8 confirm the descriptive analysis from Figure 4.3 that there are no meaningful pretreatment trends in our baseline sample (Columns (1) and (2)). However, it could be that earnings have diverged between the two occupational groups more generally. This may also affect future earnings of workers in our sample. Therefore, we use a new sample of workers, imposing the same set of sample restrictions as in our baseline case with 1998 as the reference year. Matching is performed on the years 1993 to 1998. On average, we find that workers in deregulated occupations even have slightly *higher* earnings over time prior to the reform year in 2003. However, there are significant and negative effects in the non-matched sample for earnings in 2002 and 2003 of about -0.8 percent and -1.6 percent, respectively. The matched sample does not show any economically significant earnings effects over the same period. This underscores the importance of using matching weights to correct for different sample compositions.

The common trend assumption may also be violated if the reform was anticipated by firms and workers, which would give them the opportunity to self-select into one of the treatment groups. This is unlikely because the reform was passed and implemented within the year of 2003, which made it difficult for workers to switch occupations and for firms to adjust their business model already prior to the reform. In addition, restricting the sample to regular employees who work in the same occupation and in the same firm for three years prior to the reform avoids that our results are affected by endogenous switching. However, because the sample would be larger by about a third without these restrictions, effects may be subject to a sample selection bias. Results are qualitatively and quantitatively similar when we allow for occupational and firm switches before the reform and include non-regular employees (including unemployment and apprenticeship spells) (see Appendix Table A4.9). This implies that the results do not strongly depend on the sample restriction and it also implies that switching before the reform is not systematically related to the reform.

[34] Appendix Table A4.7 shows that matching successfully addresses remaining differences in the earnings growth between the two groups.

[35] The reference year 1998 is arbitrarily chosen. However, the analysis needs a sufficient number of years before and after any given pseudo treatment year. We use five pretreatment years as in the baseline case. By starting in the year 1993, the analysis avoids major economic changes that are due to the German reunification in 1989/90. The analysis stops in 2003 with the start of the true treatment. Thus, the years 1993 and 2003 establish the natural upper and lower bounds for a common trend analysis. In any case, showing yearly treatment effects make the exact year of the pseudo-treatment less important.

Another threat to identification comes from changes in the economy which affect deregulated occupations more negatively than regulated occupations in the period after 2004. As outlined in Section 4.2, the reform was part of a larger reform package that aimed at reforming labour market institutions (especially unemployment insurance systems and assistance) and the organization of the Federal Employment Agency. It is unlikely that these other reforms had differential impacts on regulated and deregulated occupations, especially because the wage setting process was not affected at all (Dustmann et al., 2014). We are more concerned about two economic events that happened closely after the reform. The first event is the enlargement of the European Union in 2004 and 2007, which granted citizens from new member countries access to the German labour market.[36] While the data clearly show that the share of foreign workers is twice as large in deregulated occupations than in regulated occupations (see Table 4.1), these shares do not change much over time. For example, the SIAB data show that overall 5.6 percent (10.7 percent) of workers in regulated (deregulated) occupations were foreign citizens in the year 2003. These numbers change to 5.7 percent and 10.8 percent in 2010, representing a negligible increase.[37] The most likely reason for this low increase in the foreign workforce is that Germany allowed unlimited access to the German labour market for the new 2004 (2007) member countries only from 2011 (2014) onwards. Nevertheless, we use a regional approach to test whether changes in the share of foreign workers at the county level affects the reform estimates. The share of foreign workers at the county level is computed based on all workers in the SIAB data. Columns (1) and (3) of Table 4.3 show the results of a triple difference-in-differences model, which interacts the yearly share of foreign workers with the treatment interaction. We do not observe that the share of foreign worker interacts in meaningful ways with the treatment effect.

A second event is the economic and financial crisis of 2009. While earnings start to decrease more in deregulated occupations than in regulated occupations already in 2007, Figure 4.4a suggests that workers in deregulated occupations are hit more by the crisis than workers in regulated occupations in 2009. However, the larger reform effect in 2009 seems to disappear again in 2010 with effect sizes that are almost the same as in 2008. The most likely reason for this seemingly transitory shock is that the crisis affected had its strongly effects on the export-oriented manufacturing sector which also comprised the largest share of individuals working in deregulated occupations (see Table 4.1).[38] Rinne and Zimmermann (2012) document that manufacturing production recovered quickly after the crisis, which may explain the transitory increase of the reform effect in 2009. Running regressions within industry groups, Appendix

[36] In 2004, ten countries joined the EU: Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia. In 2007, Bulgaria and Romania joined the EU.

[37] Occupation-level regression results confirm no relationship between the share of foreign workers in deregulated compared to regulated occupations and the reform (Column (7) in Table 4.7). We stop in 2010 because the Federal Employment Agency introduced a new occupational classification (KldB2010) in the year 2011, which is very close to the International Standard Classification of Occupations 2008. However, the major revision makes it impossible to clearly map old and new occupations, which basically invalidates comparisons over longer time periods.

[38] Rinne and Zimmermann (2012) document that the GDP in manufacturing decreased by 18 percent in 2009.

Table A4.10 shows that the reform effects within manufacturing are in line with the baseline effects. The average effect seems more driven by the reform effects in the construction and trade, maintenance, and repair industries, which are almost two times as large as the effect in manufacturing. This shows that it is important to match on industry groups in the entropy balancing such that the effect is not dominated by a particular characteristic of the sample.

However, Rinne and Zimmermann (2012) also document that the crisis hit some regions harder than others because economic activity and industries are unequally distributed across Germany. Using the unemployment share of the county as an indicator for the severity of the crisis, we estimate another triple difference-in-differences model to test the interaction of the crisis with the reform effect at the regional level (Columns (2) and (4) of Table 4.3). We use again all workers in a county to compute regional unemployment shares by using the SIAB data. While we do not find that reform-induced unemployment effects are stronger in regions with high unemployment growth, we do find that the earnings effect is slightly stronger with increasing unemployment: a one percentage-point increase in the regional unemployment rate (seven percent increase in the national unemployment rate in 2003)[39] increases the reform effect by 0.17 percentage-points. While this does not seem to be a strong effect when compared to the main reform effect of -2.34 percent, this indicates that local economic conditions matter to a certain extent for the outcome of the reform.

Economic shocks may also hit one particular occupation across industries (instead of several occupations within one industry). This may be a threat to our identification because it would make other occupations that do not experience the shock bad comparisons. In our main analysis, we match on industries in the entropy balancing because we are more concerned about industry-specific economic shocks (see above) than about occupation-specific shocks. Nevertheless, Appendix Table A4.16 provides alternative results when matching on seven craft groups, categorizing occupations of the crafts sector along similar tasks (see Appendix Table A4.1).[40] The classification of occupations comes from the German Confederation of Skilled Crafts and includes (i) building and interior finishes trades, (ii) electrical and metal-working trades, (iii) woodcrafts and plastic trades, (iv) clothing, textiles, and leather crafts and trades, (v) food crafts and trades, (vi) health and body care trades as well as the chemical and cleaning sector, and (vii) graphic design. Compared to the baseline specification, we find larger reform effects when matching on craft groups instead of industry groups. This suggests that controlling for industry-specific shocks may be more important.

A further threat to identification is that workers in deregulated occupations switch systematically to close occupations in regulated occupations because they may receive higher earnings there. This would contaminate the comparison group because the comparison group would

[39] The average unemployment rate among all workers subject to social security payments in 2003 was equal to 14 percent. The national rate of unemployment (including the entire workforce) was somewhat lower at 11.6 percent in 2003 Federal Statistical Office (2018*a*).

[40] The overlap between industries and craft groups is not large enough to consider craft occupation-by-industry specific effects.

also be affected by the treatment. To shed some light on the extent of switching, we estimate whether workers switch occupations and firms more frequently in deregulated compared to regulated occupations after the reform. Based on the matched comparison group, Appendix Table A4.11 reveals that occupational and firm switching is not systematically different between deregulated and regulated occupations (Columns (1) and (3)).[41] However, at a more aggregated level, workers are slightly more likely to switch from a deregulated occupation to a regulated occupation after the reform than the other way around (Column (2) in Appendix Table A4.11). Given that we assign treatment status based on the occupation in 2003, occupational switching between occupational groups would lead to an underestimation of the reform effect (assuming that switchers change occupations to earn higher wages).

Another way of studying the impact of potential switching on the reform effect is to drop regulated and deregulated occupations that are similar to each other. From the list of occupations, we identified three occupational groups that have to perform similar tasks. These occupational pairs are (i) tile, slab and mosaic layer and cast stone and terrazzo maker (both deregulated) and bricklayer and concretor (regulated), (ii) metal former, galvaniser, and metal and bell founder (all deregulated) and metal worker (regulated), and (iii) interior decorator (deregulated) and installer and heating fitter (regulated). Because of similar task requirements, skills should be more transferable and workers may find it particularly easy to switch between these occupations. As expected, dropping those occupations from the sample yields larger reform effects (Appendix Table A4.12). Especially dropping occupations in sample (ii) raises reform effects from -2.3 percent to -3.3 percent for earnings and from 0.7 percent to 1 percent for unemployment. One reason could be that workers can switch occupations between these craft occupations very easily. Another interpretation could be that because those occupations are often in the industrial manufacturing sector and not in the crafts sector, the reform would not affect earnings and unemployment in these occupations.

4.5.6 Heterogeneity by Firm Size

As mentioned earlier, the SIAB does not allow to distinguish between craft businesses and businesses in other industries. This may attenuate the reform effect if our sample comprises a lot of firms that are not directly affected by the reform. Because craft businesses are usually rather small with no more than nine employees on average,[42] and because we know that the reform has mainly produced one-man businesses, we expect that the reform effect is strongest among small firms. The effects should also be considerable weaker in larger firms because they may absorb the reform effect more easily and workers who are not working in the craft sector can only be affected indirectly. Table 4.4 shows results by pretreatment average

[41] In the non-matched sample, we observe that occupational switching is stronger in the deregulated occupations than in the regulated occupations after the reform. However, workers in deregulated compared to regulated occupations are less likely to change their firm after the reform. Results not shown.
[42] For example, the Federal Statistical Office (2018*b*) reports that the average firm has about nine employees, ranging from two to 33 employees depending on the specific occupation.

firm size of the individual worker (see Appendix Figures A4.2 and A4.3 for yearly treatment effects). The entropy balancing procedure is rerun within each subsample to ensure a valid comparison group. The findings confirm our expectations. For workers in firms with less than 20 employees, the reform effect in terms of earnings and unemployment is twice as large as in the overall sample. The effect fades out with increasing firm size.

4.5.7 Attrition

Overall, we observe that only 6.8 percent of all person-year observations are missing in our analysis. While workers may disappear from the social security records for various reasons (leave the labour force, migrate abroad, become a public servant, pass away), the reform should also have triggered attrition in the deregulated occupations because self-employed workers are not listed in the social security records. Dropouts account for 79 percent of missing person-year observations (5.4 percent of all person-year observations). Columns (1) and (2) of Table 4.5 document that workers drop out (slightly) more frequently in deregulated occupations than in regulated occupations. Appendix Table A4.13 reveals that yearly treatment effects may also increase up to 1.7 percentage-points in the year 2012. The other 21 percent of missing person-year observations (1.4 percent of all person-year observations) are not in the analysis because workers report missing or zero earnings. This may happen when workers are still registered, but the employer does not report any earnings that are subject to social security payments. We do not know why no earnings are reported for these workers. However, it seems likely that some of them have become self-employed while still being registered with their former employer. Excluding dropouts, Columns (3) and (4) of Table 4.5 document the reporting of zero and missing earnings is also more frequent in deregulated occupations than in regulated occupations. Finally, Columns (5) and (6) of Table 4.5 show highly significant results for selective attrition, which is due to either dropping out or reporting of non-positive earnings, respectively.

To get an idea about the (average) ability of potential new business owners, we compare the earnings in 2003 of workers who we know drop out in future periods to the earnings of workers who do not drop out.[43] Appendix Table A4.14 shows that dropouts who drop out at some year between 2004 and 2014 earn 11 percent less than non-dropouts in 2003 (Column (1)). This negative selection is not different between workers in deregulated versus regulated occupations (Column (2)). However, when concentrating on workers who drop out immediately after the reform in the years 2004 to 2006 (2004 to 2008), we observe that the earnings disadvantage of dropping out of the social security records is five (three) log-points lower for workers in deregulated occupations than in regulated occupations. However, the earnings disadvantage for dropping out is also six (five) log-points larger than in the average over all years, leaving the overall negative earnings selection almost unchanged. Hence, while dropouts from deregulated occupations earn slightly higher earnings than dropouts from

[43] By dropouts, we refer to workers who drop out from the sample due to disappearing from the social security records or due to reporting zero or missing earnings.

regulated occupations, they still earn much lower earnings than workers who do not drop out. It is also interesting to note that the earnings advantage completely vanishes for later dropouts in the years 2009 to 2014, indicating that the positive selection appears only in the early periods.

Dropping out of the sample can also be related to other background characteristics. Controlling for interactions between educational, personal, and job characteristics and whether the worker has worked in a deregulated occupation in 2003, further narrows the earnings gap between dropouts and non-dropouts in deregulated occupations in 2003 (Appendix Table A4.15). However, it is still the case that dropouts are negatively selected compared to non-dropouts in deregulated occupations. These findings suggest that working in a high-paying job prior to the reform creates low incentives to start an own business.

Attrition may also be an identification problem if it is not related to the reform. For example, we would also detect a reform effect if workers in deregulated occupations with an otherwise higher earnings growth dropped out of the sample. We already document above that this is unlikely given that those who drop out are those with below-average earnings. Nevertheless, we can test how large earnings of dropouts need to be for our results to go away. We bring missing observations back into the sample by imputing different percentiles where the percentiles are based on the yearly earnings distribution of workers in the crafts sector. In Table 4.6, we calculate the average reform effects with those imputations at the 10th, 25th, 50th, 75th, and 90th percentile. Estimates are generally in the same ballpark as our baseline effect of -2.34 percent. Even imputing the 90th percentile for everyone how dropped out of the sample gives still a negative and significant reform effect of -1.3 percent.

4.6 Reform Effects on Overall Employment and Earnings

To set the results into perspective, we now turn to economy-wide effects of the reform. Theory suggests that the deregulation should have had beneficial effects for employment growth and perhaps also for earnings growth (with only incumbent workers seeing earnings and employment losses). We then also turn to a discussion about how the earnings of self-employed individuals have changed due to the reform.

4.6.1 Employment and Earnings of Employees (SIAB)

Using SIAB data, Figures 4.5a and 4.5b present the evolution of employees subject to social security before and after the reform in deregulated, regulated, and other occupations (relative to the year 2003). The analytical sample is based on workers who report valid occupational codes, work in firms with less than 1,500 employees, and are between 20 and 60 years old. The analysis starts in the year 1999 and stops in the year 2010 due to changes in the occupational classifications, which do not allow a coherent comparison over time. While total employment in other occupations has increased by about 5 percent in 2010 and employment in regulated

occupations has stabilized at some lower level, we observe ongoing *decreases* in employment levels in deregulated occupations. Results are confirmed by occupation-level regression using occupation fixed effects (Columns (1) to (3) in Table 4.7). They show that employment in deregulated occupations has decreased by about 8 percent more on average than in regulated occupations.[44]

Average earnings for (new and incumbent) employees at the occupational level are lower in deregulated than in regulated occupations by about 1 percent to 1.6 percent (Column (5) and (6) in Table 4.7). However, the average effect is probably only due to the large drop in earnings in the year 2009 (see Figure 4.5c). The most likely reason for this drop is again that the economic crisis hit in particular the manufacturing sector, which employs the majority of workers in deregulated occupations. The analysis suggests that occupation-level regressions are not able to capture the reform effects on incumbent workers because they do not track individual workers over time.

One of the reform goals was to halt the negative trend in apprentice numbers in craft occupations through establishing new firms. However, opponents of the reform argued that the deregulation would do the opposite because the Master Crafts Certificate is seen as a qualification (and also unofficial obligation) to educate apprentices. Furthermore, the deregulation undermined firms' incentives to educate apprentices, because they now could become direct competitors by founding their own business, upon completion of their apprenticeship. After the reform, business owners without a Master Crafts Certificate had to apply for the right to employ apprentices. Using SIAB data, Figure 4.5d indicates that the number of apprentices has decreased more in deregulated occupations than in regulated occupations. However, occupation-level regressions do not find a significant relationship between the number of apprenticeships in deregulated compared to regulated occupations and the reform (Column (4) in Table 4.7). These results are confirmed when examining firm registry data from the German Confederation of Skilled Crafts in Appendix Figure A4.4. While there is a clear continued downward trend in apprentice numbers in craft occupations, the regulated and deregulated occupations do not actually differ in the numbers of apprenticeships and apprenticeship final examinations.[45]

Overall, the results imply that the newly established firms did not create new job opportunities. This is in strong contrast to other deregulation reforms studied elsewhere. Before we discuss likely reasons for these differences to other studies in the literature in the next section,

[44] By contrast, Koch and Nielen (2016) use representative firm panel survey data to evaluate the reform and do not find significant effects on employment. They also do not find effects on lay-offs, limited-term contracts, part-time work, and the number of employees on union tariffs. While our study, which uses administrative individual-level worker data, clearly indicates negative employment effects, the bottom line is that it is highly unlikely that the reform has triggered positive employment growth.

[45] Koch and Nielen (2016) find slightly positive effects on the number of apprentices in deregulated compared to regulated occupations when using firm survey data. Results may differ because the survey data contain only firms with at least one employee subject to social security, biasing the results toward larger firms.

we examine the performance of self-employed craftsmen. The analysis should give us an indication about the role of the entrepreneurs in explaining the reform effects.

4.6.2 Net Income of Self-Employed and Employees (Microcensus)

The SIAB data do not allow us to identify self-employed individuals because they are not subject to social security payments. To study reform effects on the income situation of self-employed individuals, we use repeated cross-sections from the German Microcensus. The large sample size enables us to concentrate on the labour market position of self-employed individuals in craft occupations. We can also verify the SIAB results on the overall economic situation of employees in craft occupations, using measures of net income instead of gross earnings. However, the cross-sectional nature of the survey does not allow us to track individuals over time, which leaves us with comparing yearly occupational averages. This raises concerns about common trends in pretreatment periods, which we try to mitigate by controlling for individual covariates and occupation fixed effects in the occupation-level regression analysis. Moreover, we cannot distinguish between newly self-employed individuals and individuals who were already self-employed before the reform.

While we find that the number of self-employed individuals has increased in deregulated compared to regulated occupations (see also Rostam-Afschar, 2014; Runst et al., 2018*a*), we do not find substantial income effects for workers who work in craft occupations (Columns (1) and (2) in Table 4.8). Studying the effects by employment status, we find negative but statistically non-significant effects on self-employed individuals, whereas the effects for employees are slightly positive (Columns (3) and (5) in Table 4.8).[46] However, controlling for age, gender, highest school degree, highest professional degree, and nationality, we do not detect any reform effects (Columns (4) and (6) in Table 4.8). The latter result confirms small overall earnings effects that we found with the SIAB data.

To sum up, the microcensus analysis suggests that the economy-wide economic position of self-employed and employees has not changed dramatically after the reform. This is surprising given that the reform substantially increased entrepreneurial activity. In the next section, we discuss likely reasons for the reform effects.

4.7 Discussion

The reform of the German Trade and Crafts Code in the year 2004 has led to a large increase in the foundation of new businesses. While there has been no notable growth in the number of new establishments in either occupational group, we documented that the number of deregulated businesses increased annually by 26 percent in the first three years and then

[46] Appendix Figure A4.6 plots average log monthly personal income of employees and self-employed individuals in craft occupations over time.

increased further by about 5 percent each year from 2007 to 2014. This is substantially higher compared to other deregulation reforms that are studied in the literature. For example, similar reforms in France, Mexico, Portugal, and India led to increases of between 5 percent and 17 percent in the number of new firms (Aghion et al., 2008; Bertrand and Kramarz, 2002; Branstetter et al., 2014; Bruhn, 2011; Kaplan, Piedra and Seira, 2011).

As theory would suggest, we find that the economic position of incumbent workers in deregulated occupations compared to workers in regulated occupations was negatively affected by the reform. On average, earnings increased less strongly by about 2.3 percent for deregulated occupations than for regulated occupations after the reform. The strength of the earnings reform effect is almost linearly increasing (in absolute terms) over time. For unemployment, we find an average increase of about 0.7 percent. As mentioned earlier, the earnings losses of incumbent workers seem to be rather small compared to the large increase in new firms. In contrast to the theory, however, we do not find increases in overall employment or average earnings for new employees. This is also not in line with almost all of the studies mentioned before because these studies also document that deregulation triggers employment growth and also sometimes earnings growth for other employees.

What can explain the relatively small reform effect with respect to earnings? To have enduring and large labour market effects, the newly established businesses would have to represent stable competitors for incumbent firms. While the aggregate numbers in Figure 4.1 indicate that the stock of businesses is strictly increasing, findings by Müller (2014, 2016) indicate that the composition of firms has changed over time. Studying survival rates of newly founded businesses, he shows that five-year survival rates hovered around 70 percent in both regulated and deregulated occupations before the reform and dropped to around 50 percent in the deregulated occupations after the reform. Thus, about 60 percent of newly established firms had already disappeared after five years (Müller, 2014, 2016). This is in line with the theoretical model of Branstetter et al. (2014) who show that new firms have lower survival probabilities than incumbent firms. Their model further postulates that new entrepreneurs have relatively lower talent than those already in the market. While we cannot compare skills of incumbent entrepreneurs with the skills of new entrepreneurs, our dropout analysis in Section 4.5.7 suggests that new entrepreneurs are not a positive selection when compared to the average worker in the craft occupations. Accordingly, Runst et al. (2018*a*) show that both entry *and* exit rates of firms have increased in the deregulated crafts after the reform.

Furthermore, customers may have discriminated between crafts offered by newly established firms and crafts offered by incumbent firms because incumbent firms still hold a Master Craftsmen Certificate to signal superior quality of their goods and services. In other words, it could be that the two firms were not offering perfectly substitutable crafts, which would have mitigated competitive pressure for incumbent firms. There are some pieces of evidence that this was the case. For example, Fredriksen, Runst and Bizer (2018) show that customers still value the Master Craft certificate and that firms in deregulated occupations use the certificate as a quality signal. While effective competitive pressure should have reduced prices for

customers, there is little evidence that aggregate price indicators for goods and services in the different occupations are largely affected by the reform (RWI, 2012). However, because detailed data on prices are not available, the results of this analysis remain rather speculative. However, if firms do not experience large pressure to lower prices, we should observe that the earnings disadvantage of the reform is smaller for a selected group of workers who remain with the same firm and stay in the same occupation after the reform. Columns (4) to (7) in Appendix Table A4.11 indicate that this is the case. The earnings effect is only half of the average effect for workers who have not changed the firm or the occupation. Treatment effects are much larger for occupation and firm switchers. Moreover, incumbent firms may have reduced competitive pressure by investing more in technology and innovation. However, Koch and Nielen (2016) use data from a representative establishment panel and find that the reform had insignificant effects on investments (per establishment or employee) and product innovations.

What can explain the absence of a reform effect on overall employment and average earnings for other employees? Müller (2014, 2016) shows that the majority of newly founded firms were and remained one-man businesses with very little apprenticeship activity. He documents that shares of one-man businesses in the deregulated occupations increased from 24 percent in 1995 to 61 percent in 2012. The likely reason for limited employment growth was that newly established firms were not able to compete at a large scale with incumbent firms. Therefore, they remained relatively small with little creation of further employment. The absence of higher labour demand through the new firms may also explain why there has not been an effect on average earnings growth for other employees in the economy.

4.8 Conclusion

In this chapter, we study the labour market effects of the reform of the German Trade and Crafts Code in 2004. Abolishing the requirement to hold a Master Crafts Certificate to open a new business increased entrepreneurial activity massively, tripling the number of businesses within ten years. Using administrative longitudinal social security data and German Microcensus data, we find that the reform led to decreasing earnings and increasing unemployment for incumbent workers. However, contrary to the theory, the reform did not trigger employment and earnings growth for others in affected occupations. The most likely reason for this finding is that the newly established firms remained one-man business with low ability to compete against incumbent firms. This makes it also unlikely that the reform strongly affected prices in the crafts sector. While the reform may have increased choice for customers, we conclude that the reform led to an average welfare loss for incumbent craft workers in deregulated occupations because they experienced employment and earnings losses without having the opportunity to exploit new employment possibilities.

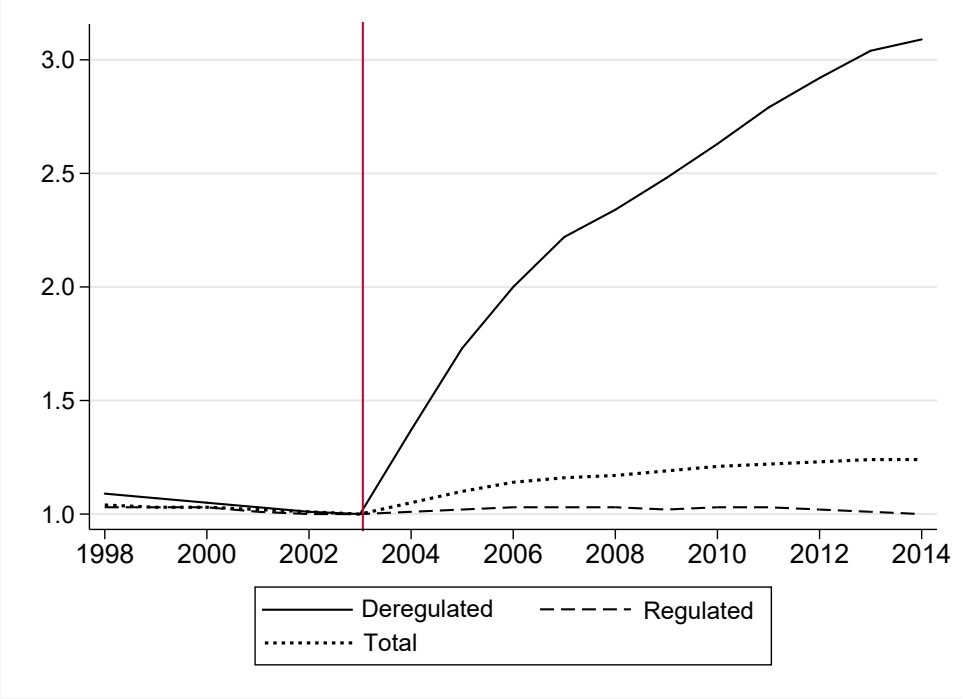
The study contributes to the current literature by documenting the labour market effects of a deregulation reform that failed to produce intended effects of increasing employment

dynamics within an industry. This is the case even if the increase in entrepreneurial activity was much larger in the German reform than it was in reforms studied elsewhere. It is also one of the few studies that uses longitudinal administrative earnings data to track the same workers over long time periods. This allows us to sort out unobserved individual heterogeneity from the impact of different sample compositions.

While decreasing entry barriers should generally foster competition, entrepreneurial activity, innovation, and employment growth, policy makers should be aware that there may be unintended consequences when the newly created businesses do not compete with incumbent firms. If this is the case, it is likely that there are further (more important) barriers in place that hold back new firms from becoming stable competitors. Thus, each deregulation reform should collect and carefully evaluate possible industrial and occupational entry barriers before the reform is implemented. While it is difficult to identify all sorts of entry barriers in advance, the success of each reform has to be constantly monitored and evaluated.

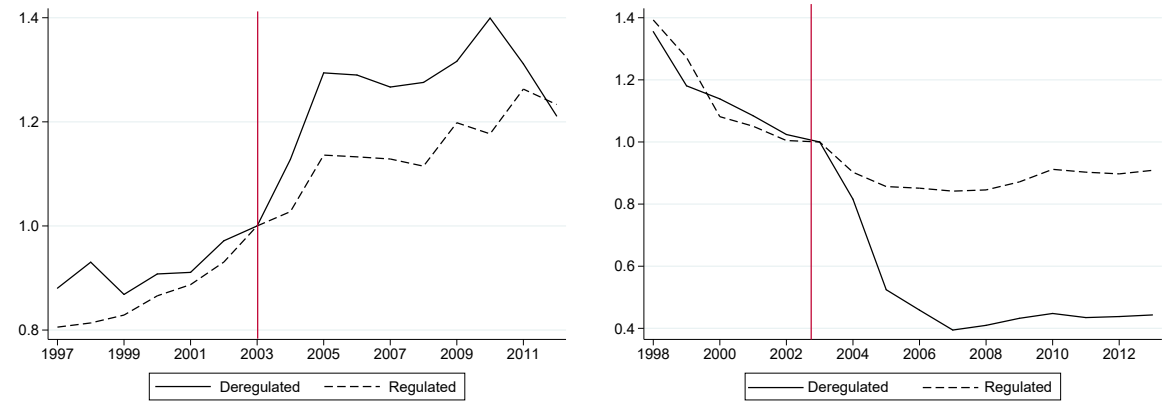
Figures and Tables

Figure 4.1 : Changes in Number of Businesses (relative to 2003)



Notes: The figure shows the number of business in the deregulated and regulated crafts. In 2003, the number of businesses in the regulated (deregulated) crafts sector was 587,762 (74,940).
Source: German Confederation of Skilled Crafts

Figure 4.2 : Self-Employment and Master Craft Examinations (relative to 2003)

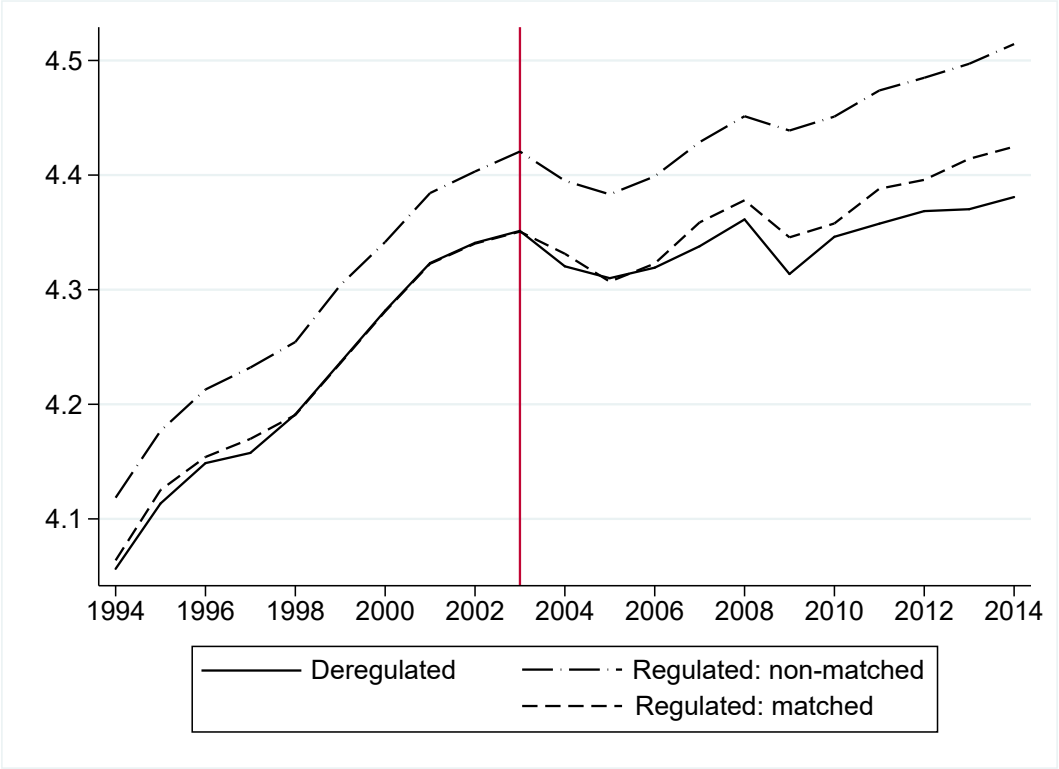


(a) Self-employed

(b) Master Craft Examinations

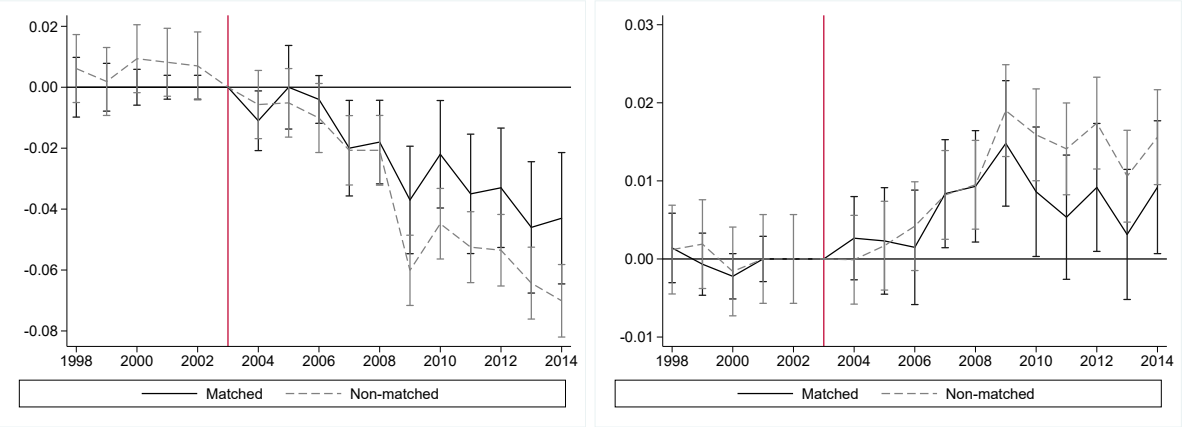
Notes: The figure shows the share of self-employed individuals in Figure (a) and the number of master craft examinations in Figure (b). Both figures show values relative to the year 2003.
Source: Microcensus (Figure (a)) and German Confederation of Skilled Crafts (Figure (b))

Figure 4.3 : Log Earnings Before and After the Reform



Notes: The figure shows average log gross daily earnings for the sample of incumbent workers. Earnings are in current values and not adjusted for inflation.
Source: SIAB

Figure 4.4 : Effect on Log Earnings and Unemployment for Incumbent Workers over Time

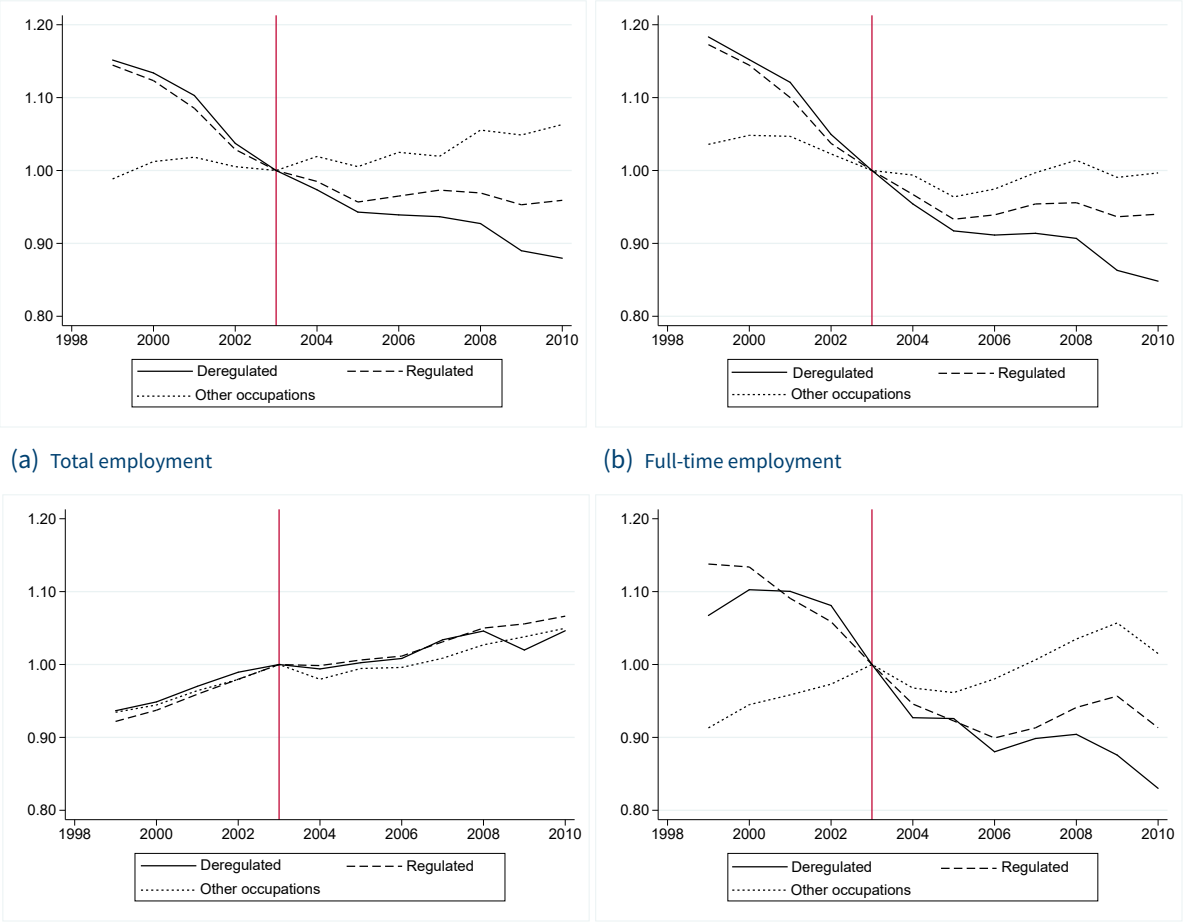


(a) Log daily earnings

(b) Unemployment

Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on log gross daily earnings in Figure (a) and on unemployment in Figure (b) for the sample of incumbent workers.
Source: SIAB

Figure 4.5 : Overall Employment and Earnings (relative to 2003)



(a) Total employment

(b) Full-time employment

(c) Gross daily earnings

(d) Apprenticeships

Notes: The figure shows average employment and gross daily earnings in deregulated, regulated, and other occupations. The analytical sample is based on workers who report valid occupational codes, work in firms with less than 1,500 employees, and are between 20 and 60 years old. Figure (a) plots total employment. Figure (b) restricts the sample to workers who are employed as regular full-time employees. Figure (d) restricts the sample to apprentices. Figure (c) plots average daily earnings over all individuals who report positive earnings. Earnings are in current values and not adjusted for inflation. All time series are relative to the year 2003.
Source: SIAB

Table 4.1 : Covariate Balancing Before Matching, 2003

	(1)	(2)	(3)	(4)	(5)	
	Deregulated		Regulated		Mean difference	
	Mean	Variance	Mean	Variance	Δ	Sig.
Log daily gross earnings 2003	4.351	0.166	4.421	0.148	-0.070	***
Log daily gross earnings 2002	4.340	0.156	4.403	0.135	-0.063	***
Log daily gross earnings 2001	4.323	0.156	4.384	0.133	-0.061	***
Log daily gross earnings 2000	4.281	0.175	4.342	0.152	-0.061	***
Log daily gross earnings 1999	4.236	0.192	4.304	0.168	-0.068	***
Log daily gross earnings 1998	4.191	0.211	4.254	0.192	-0.063	***
Female	0.229	0.177	0.089	0.081	0.140	***
Foreigner	0.086	0.079	0.042	0.040	0.044	***
Age: 12-34	0.183	0.150	0.231	0.178	-0.048	***
Age: 35-45	0.463	0.249	0.474	0.249	-0.011	***
Age: 46-55	0.354	0.229	0.294	0.208	0.060	***
Training: none	0.181	0.148	0.056	0.052	0.125	***
Training: vocational	0.764	0.181	0.872	0.112	-0.108	***
Training: university	0.018	0.018	0.042	0.041	-0.024	***
Training: missing info	0.037	0.036	0.031	0.030	0.006	***
Schooling: Haupt-/Realschule	0.920	0.271	0.895	0.094	0.025	***
Schooling: FH-Reife / Abitur	0.042	0.040	0.074	0.069	-0.032	***
Schooling: missing info	0.038	0.037	0.031	0.030	0.007	***
Ever unemployed	0.035	0.034	0.034	0.033	0.001	
Ever marginally employed	0.002	0.002	0.001	0.001	0.001	***
Changed firm within 6 years before reform	0.201	0.161	0.254	0.190	-0.053	***
Changed occupation within 6 year pre reform	0.145	0.124	0.134	0.116	0.011	**
Industry: manufacturing	0.773	0.176	0.383	0.236	0.390	***
Industry: construction	0.062	0.058	0.338	0.224	-0.276	***
Industry: wholesale and retail trade	0.061	0.057	0.090	0.082	-0.029	***
Industry: real estate and business activities	0.071	0.066	0.057	0.054	0.014	***
Industry: other industries	0.033	0.032	0.132	0.114	-0.099	***
Tasks: professional tasks	0.949	0.049	0.808	0.155	0.141	***
Tasks: complex specialized tasks	0.051	0.049	0.192	0.155	-0.141	***
Job tenure: 0-3 years	0.127	0.111	0.185	0.151	-0.058	***
Job tenure: 4-7 years	0.283	0.203	0.299	0.210	-0.016	***
Job tenure: 8-14 years	0.365	0.232	0.334	0.223	0.031	***
Job tenure: 15-39 years	0.226	0.175	0.181	0.148	0.045	***
Firm tenure: 0-3 years	0.105	0.094	0.142	0.122	-0.037	***
Firm tenure: 4-7 years	0.249	0.187	0.270	0.197	-0.021	***
Firm tenure: 8-14 years	0.360	0.230	0.346	0.226	0.014	**
Firm tenure: 15-39 years	0.286	0.204	0.242	0.183	0.044	***
Employees at firm (2001-2003 average)	241	98,528	175	85,241	66	***
Median wage at firm (2001-2003 average)	85.4	631.6	86.7	934.8	-1.3	***
Regional unemployment 2003	0.115	0.002	0.119	0.003	-0.004	***
Regional foreigner share 2003	0.070	0.003	0.065	0.003	0.005	***
Regional average daily earnings 2003	79.83	79.11	79.44	87.00	0.390	***
Growth regional unemployment 99-03	0.138	0.027	0.123	0.025	0.015	***
Growth regional foreigner share 99-03	0.061	0.109	0.083	0.142	-0.022	***
Growth regional average daily earnings 99-03	0.084	0.0004	0.085	0.0004	-0.001	***
Growth log daily gross earnings 98-03	0.233	0.291	0.243	0.571	-0.010	
Growth log daily gross earnings 98-00	0.139	0.196	0.134	0.444	0.005	
Growth log daily gross earnings 00-03	0.093	0.088	0.103	0.306	-0.010	**
Growth log daily gross earnings 94-03	0.509	0.982	0.539	2.036	-0.030	
Observations	6,521		24,636			

Notes: The table shows mean and variance of covariates for the cross-section of workers in the year 2003 unless stated otherwise. All earnings variables are in current Euros and not adjusted for inflation. Column (5) indicates the significance of a t-test for equality of means in the two samples. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Source: SIAB

Table 4.2 : Reform Effects on Incumbent Workers

	(1)	(2)	(3)	(4)
	Log daily earnings		Unemployment	
	Non-matched	Matched	Non-matched	Matched
Deregulated$_{j(i)} \times$ post$_{2003}$	-0.0404***	-0.0234***	0.0099***	0.0069***
	(0.0021)	(0.0063)	(0.0010)	(0.0024)
Constant	4.256***	4.206***	0.0171***	0.0172***
	(0.0016)	(0.0027)	(0.0008)	(0.0012)
R-squared	0.034	0.022	0.017	0.021
Observations	496,084	493,568	503,705	501,156
Individuals	31,327	31,157	31,327	31,157

Notes: The table shows average effects of the reform on log of gross daily earnings in Columns (1) and (2) and on being unemployed in Columns (3) and (4). Appendix Table A4.5 shows yearly treatment effects. Columns (1) and (3) refer to results using the non-matched comparison group and Columns (2) and (4) refer to results using the matched comparison group. All regressions include year and individual fixed effects. Average log daily earnings are equal to 4.406 (4.351) in the non-matched (matched) sample in 2003. The unemployment rate of our sample is equal to zero in 2003 by construction and equal to 3.6% for the period from 2003 to 2014. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p$<$0.10, ** p$<$0.05, *** p$<$0.01.

Source: SIAB

Table 4.3 : Reform Effect Heterogeneity by Regional Characteristics

	(1)	(2)	(3)	(4)
	Log daily earnings		Unemployment	
	Foreign	Unemployed	Foreign	Unemployed
Deregulated$_{j(i)}$ × post$_{2003}$ × share foreign	0.0067		-0.0331	
	(0.1458)		(0.0506)	
Deregulated$_{j(i)}$ × share foreign	-0.1882		-0.0634	
	(0.1414)		(0.0591)	
Post$_{2003}$ × share foreign	-0.1606		0.0929**	
	(0.1201)		(0.0370)	
Share foreign	0.2065*		0.0776*	
	(0.1092)		(0.0404)	
Deregulated$_{j(i)}$ × post$_{2003}$ × share unemployed		-0.1730*		0.0270
		(0.0995)		(0.0539)
Deregulated$_{j(i)}$ × share unemployed		0.1860**		-0.0243
		(0.0930)		(0.0651)
Post$_{2003}$ × share unemployed		0.0974		0.0695*
		(0.0658)		(0.0371)
Share unemployed		-0.7440***		0.3720***
		(0.0658)		(0.0474)
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0221***	-0.0265***	0.0075***	0.0072***
	(0.0061)	(0.0065)	(0.0023)	(0.0026)
Constant	4.206***	4.186***	0.0173***	0.0285***
	(0.0027)	(0.0030)	(0.0012)	(0.0018)
R-squared	0.022	0.024	0.021	0.025
Observations	493,430	493,430	501,018	501,018
Individuals	31,157	31,157	31,157	31,157

Notes: The table shows average effects of the reform on log of gross daily earnings in Columns (1) and (2) and on being unemployed in Columns (3) and (4). *Share foreign* refers to the percentage of foreign workers at the county level in the SIAB data. *Share unemployed* refers to the percentage of unemployed individuals at the county level in the SIAB data. Regional measures are based on all individuals in the SIAB who are between 20 and 60 years old. Both measures are computed separately by year and are demeaned to facilitate interpretation. All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 4.4 : Reform Effect Heterogeneity by Firm Size

	(1)	(2)	(3)	(4)
	Firm size (Number of employees)			
	Less than 20	20–100	100–250	More than 250
	Panel A: log daily earnings			
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0451***	-0.0363***	-0.0270*	-0.0125
	(0.0140)	(0.0110)	(0.0157)	(0.0105)
Constant	4.069***	4.182***	4.186***	4.321***
	(0.0063)	(0.0046)	(0.0071)	(0.0047)
R-squared	0.012	0.017	0.031	0.055
Observations	152,366	146,645	77,975	116,582
Individuals	9,835	9,246	4,867	7,209
	Panel B: unemployment			
Deregulated$_{j(i)}$ × post$_{2003}$	0.0145***	0.0078*	0.0051	0.0027
	(0.0054)	(0.0046)	(0.0068)	(0.0041)
Constant	0.0302***	0.0160***	0.0162***	0.0120***
	(0.0032)	(0.0023)	(0.0031)	(0.0022)
R-squared	0.023	0.022	0.023	0.018
Observations	155,446	148,999	79,014	117,697
Individuals	9,835	9,246	4,867	7,209

Notes: The table shows average effects of the reform on log gross daily earnings in Panel A and on being unemployed in Panel B. Sample splits, indicated in the column header, are based on the average individual firm size from 2001 to 2003. All regressions include year and individual fixed effects. Entropy balancing is rerun on each subsample and workers in the comparison group are weighted by these matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.
Source: SIAB

Table 4.5 : Attrition from Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Dropped out		Missing earnings		Both	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
Deregulated$_{j(i)}$ × post$_{2003}$	0.0022*	0.0058*	0.0052***	0.0044**	0.0071***	0.0091**
	(0.0013)	(0.0030)	(0.0007)	(0.0018)	(0.0014)	(0.035)
Constant	0.0000	0.0000	-0.0023***	-0.0020***	0.0000	0.0000
	(0.0010)	(0.0009)	(0.0006)	(0.0005)	(0.0014)	(0.0011)
R-squared	0.089	0.092	0.019	0.021	0.098	0.103
Observations	532,559	529,669	503,705	501,156	532,559	529,669
Individuals	31,327	31,157	31,327	31,157	31,327	31,157

Notes: The table documents the extent of sample attrition. In Columns (1) and (2), the dependent variable is an indicator variable that is one if the individual dropped out from the social security records, zero otherwise. In Columns (3) and (4), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings, zero otherwise. In Columns (5) and (6), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings or dropped out of the sample, zero otherwise. Appendix Table A4.13 shows the results for yearly treatments. Columns (1), (3), and (5) refer to results using the non-matched comparison group and Columns (2), (4), and (6) refer to results using the matched comparison group. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 4.6 : Reform Effects: Accounting for Attrition

	(1)	(2)	(3)	(4)	(5)
	Imputed earnings percentile				
	10th	25th	50th	75th	90th
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0274***	-0.0218***	-0.0184***	-0.0157***	-0.0131**
	(0.0065)	(0.0055)	(0.0053)	(0.0054)	(0.0056)
Constant	4.191***	4.191***	4.191***	4.191***	4.191***
	(0.0029)	(0.0025)	(0.0025)	(0.0026)	(0.0026)
R-squared	0.018	0.020	0.035	0.052	0.066
Observations	529,669	529,669	529,669	529,669	529,669
Individuals	31,157	31,157	31,157	31,157	31,157

Notes: The outcome variable is imputed log gross daily earnings of incumbent workers. The imputation is being done for workers who have missing earnings due to attrition (drop out of the sample or report zero/none earnings) from social security records. Missing earnings are imputed by the percentile indicated in the column header. Yearly earnings distributions are calculated based on daily earnings (including zero earnings) in the working age population (20 to 60 years old). Imputing percentiles obtained from earnings distributions that are based on log earnings yield almost identical estimates (results not shown). All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 4.7 : Economy-wide Reform Effects on Occupational Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log employment				Log daily earnings		Share foreign workers
	Total	Other occupations	Full-time	Apprentices			
Deregulated$_j$ × post$_{2003}$	-0.0835***	-0.1920***	-0.1010***	0.0196	-0.0105	-0.0162	-0.0032
	(0.0147)	(0.0160)	(0.0158)	(0.0459)	(0.0180)	(0.0199)	(0.0029)
Regulated$_j$ × post$_{2003}$		-0.1090***					
		(0.0183)					
Constant	5.692***	5.950***	5.539***	2.959***	3.967***	1.732***	0.0956***
	(0.0124)	(0.0098)	(0.0133)	(0.0388)	(0.0095)	(0.2050)	(0.0024)
Control variables	no	no	no	no	no	yes	no
R-squared	0.436	0.080	0.506	0.017	0.294	0.428	0.052
Observations	1,116	4,100	1,116	1,016	801,355	801,355	1,116
Occupations	93	342	93	90	93	93	93

Notes: The table shows regressions at the occupational level. The analytical sample is based on workers who report valid occupational codes, work in firms with less than 1,500 employees, and are between 20 and 60 years old. For regressions in Columns (1) to (4) and (7), we collapse the dataset at the occupation level. In Columns (1) to (4), the dependent variable is the log average number of workers in the occupation. Column (1) use all observations in deregulated and regulated occupations. Column (2) uses all occupations that are available. Column (3) uses only workers who are employed as regular full-time employees. Column (4) restricts the sample to apprentices. In Column (7), the dependent variable is the share of migrants within the sample of individuals with valid occupational codes. In Columns (5) and (6), the dependent variable is log daily earnings and estimation takes place on the microdata. *Control variables:* age, age squared, gender, education (8 cat.), and foreign citizenship. The analysis is restricted to the years between 1999 and 2010. All regressions include occupation and year fixed effects. Standard errors, clustered at the occupational level, in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Source: SIAB

Table 4.8 : Reform Effects on Monthly Income and Self-Employment

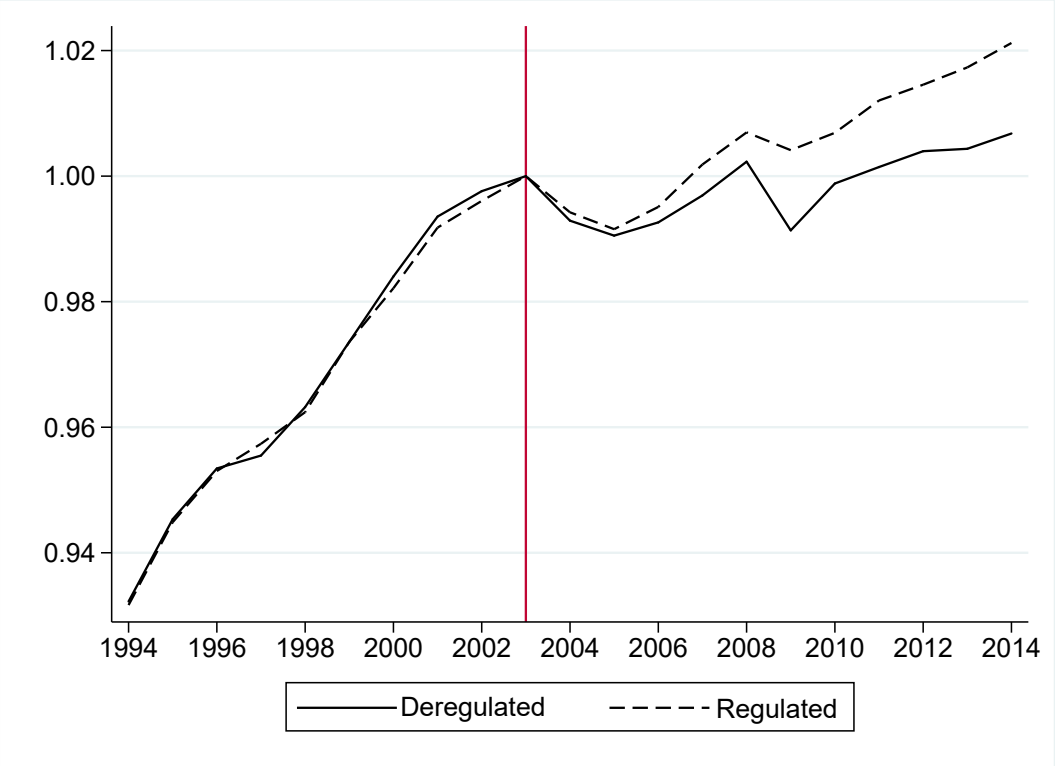
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log monthly income						Self-employed	
	All		Employees		Self-employed			
Deregulated$_{j(i)}$ × post$_{2003}$	0.0117	0.0084	0.0113	0.0002	-0.0326	0.0014	0.0267	0.0223
	(0.0107)	(0.0134)	(0.0108)	(0.0087)	(0.0353)	(0.0352)	(0.0199)	(0.0193)
Constant	7.071***	5.503***	7.052***	5.581***	7.355***	5.817***	0.068***	-0.613***
	(0.0045)	(0.0373)	(0.0047)	(0.0149)	(0.0874)	(0.0557)	(0.0042)	(0.1290)
Control variables	no	yes	no	yes	no	yes	no	yes
R-squared	0.102	0.252	0.136	0.320	0.067	0.111	0.0751	0.1944
Observations	451,098	304,321	396,363	260,666	54,735	43,655	476,892	322,635

Notes: The table shows reform effects on log monthly net personal income in Columns (1) to (6) and on an indicator variable that is one if the individual is self-employed and zero otherwise in Columns (7) and (8). The analytical sample is based on individuals aged between 18 and 65, working full-time, reporting a craft as gainful occupation in the respective year, and reporting labor earnings as their primary source of income. All regressions include occupation and year fixed effects. *Control variables:* age, age squared, gender, highest school degree (8 cat.), highest professional degree (7 cat.), and nationality group (3 cat.). Standard errors, clustered at the occupational level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01. *Source:* Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

Appendix

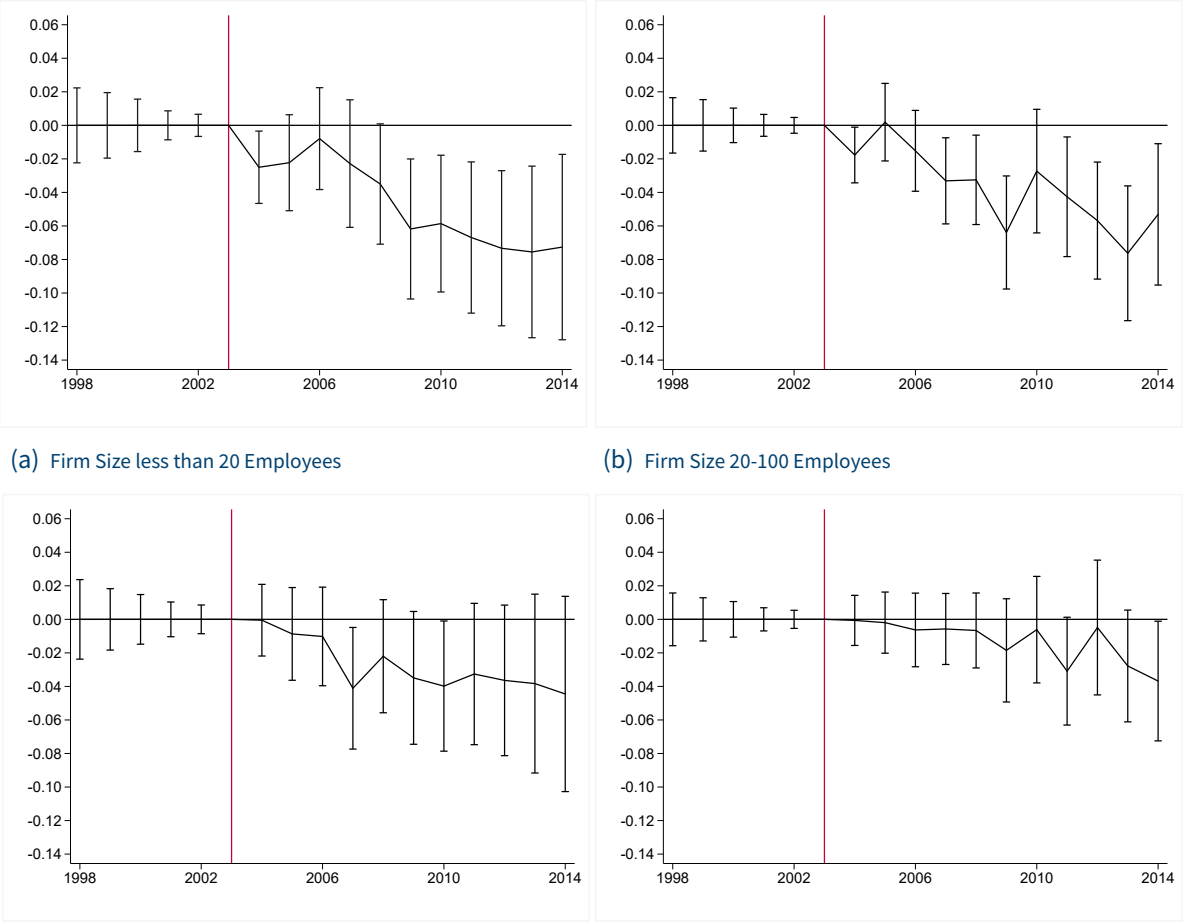
Appendix A4.1 Appendix Figures and Tables

Figure A4.1 : Log Earnings Before and After the Reform Relative to 2003



Notes: The figure plots average log gross daily earnings for the sample of incumbent workers in the regulated and deregulated occupations relative to the year 2003.
Source: SIAB

Figure A4.2 : Effect Heterogeneity by Firm Size: Log Daily Earnings



(a) Firm Size less than 20 Employees

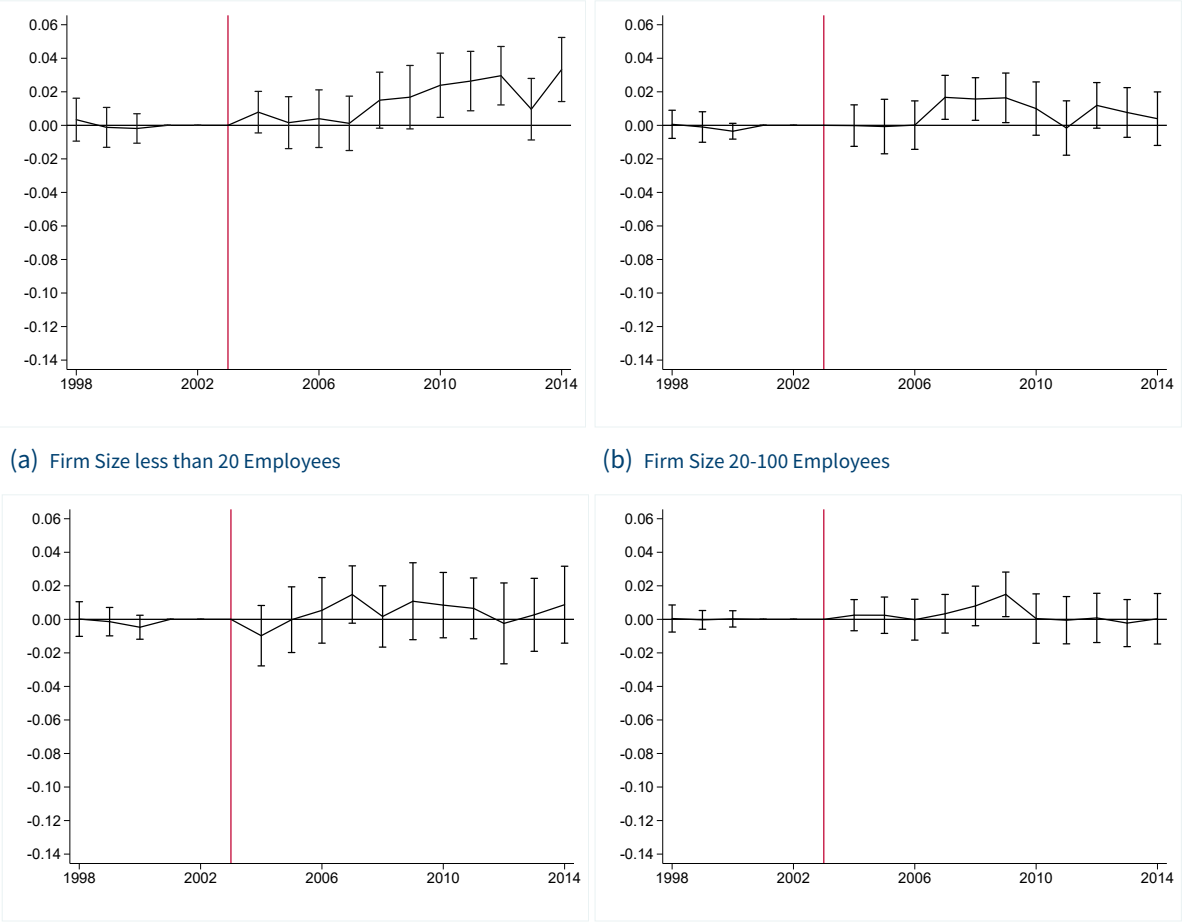
(b) Firm Size 20-100 Employees

(c) Firm Size 100-250 Employees

(d) Firm Size more than 250 Employees

Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on gross daily log earnings using the matched comparison group for incumbent workers from 1998 to 2014 by the size of the firm in the year 2003.
Source: SIAB

Figure A4.3 : Effect Heterogeneity by Firm Size: Unemployment



(a) Firm Size less than 20 Employees

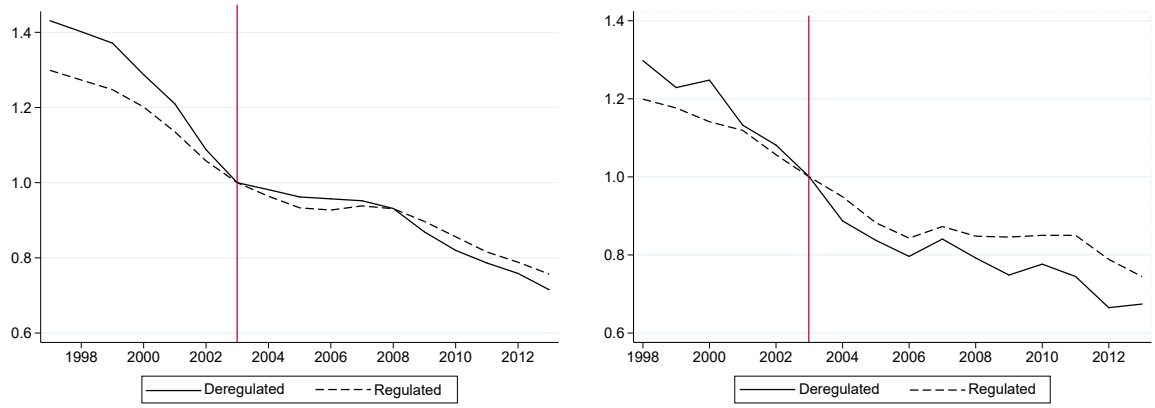
(b) Firm Size 20-100 Employees

(c) Firm Size 100-250 Employees

(d) Firm Size more than 250 Employees

Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on unemployment using the matched comparison group for incumbent workers from 1998 to 2014 by the size of the firm in the year 2003.
Source: SIAB

Figure A4.4 : Apprenticeships and Apprenticeship Examinations

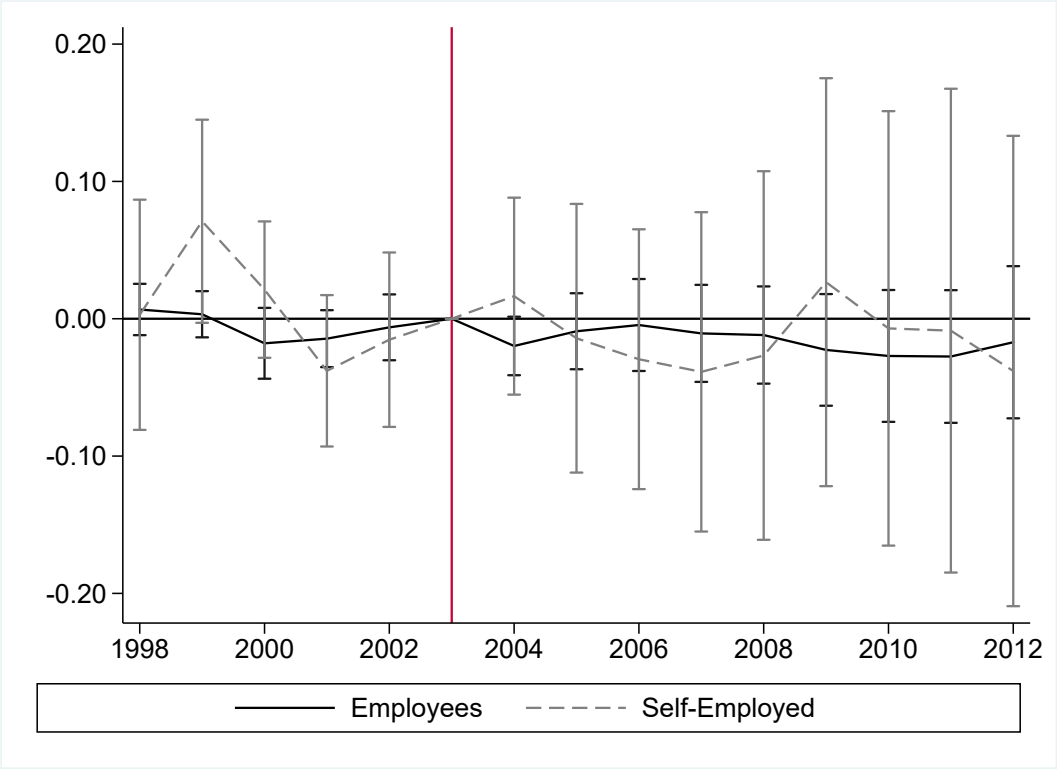


(a) Apprenticeships

(b) Apprenticeships Examinations

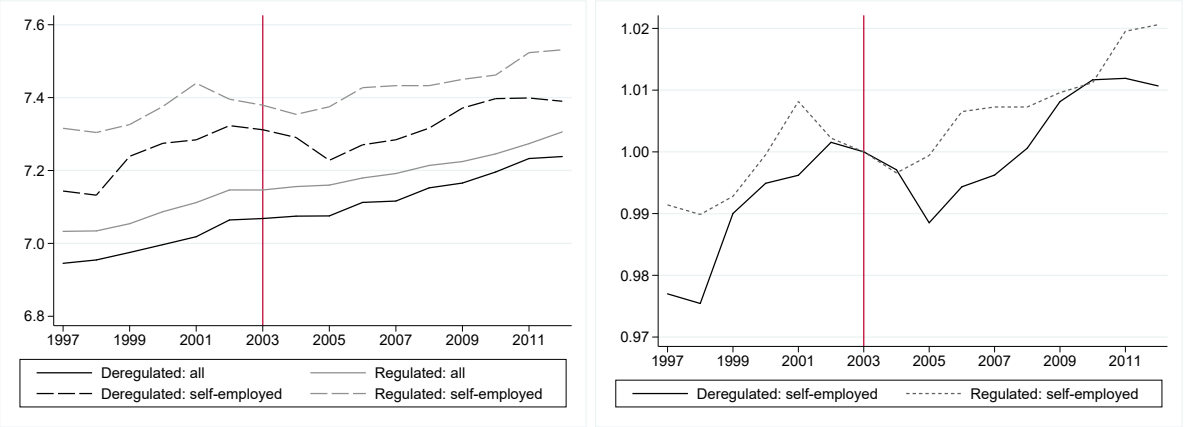
Notes: The figures show the numbers of apprenticeships (Figure (a)) and the number of apprenticeship final examinations (Figure (b)) relative to 2003. Statistics based on firm registry data.
Source: German Confederation of Skilled Crafts

Figure A4.5 : Effects on Net Monthly Income of Self-Employed and Employees



Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on log net monthly income for self-employed individuals and employees who work in craft occupations.
Source: Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

Figure A4.6 : Net Monthly Income of Self-Employed and Employees



(a) Log levels

(b) Relative to 2003

Notes: The figure plots average log monthly personal income of employees and self-employed individuals in craft occupations. Figure (a) plots log levels for all craft workers (incl. employees and self-employed individuals) and for self-employed individuals. Figure (b) plots average income levels for self-employed individuals relative to 2003.
Source: Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

Table A4.1 : List of Regulated and Deregulated Craft Occupations

Deregulated occupations						Regulated occupations					
No	Group	Name	No	Group	Name	No	Group	Name	No	Group	Name
1	I	Tile, slab, and mosaic layer	27	V	Miller	1	I	Bricklayer and concretor	22	II	Gunsmith
2	I	Cast stone and terrazzo maker	28	V	Brewer and maltster	2	I	Stove and air heating mechanic	23	II	Plumber
3	I	Screed layer	29	V	Wine cellarperson	3	I	Carpenter	24	II	Installer and heating fitter
4	II	Vessel and equipment constructor	30	VI	Textile cleaner	4	I	Roofer	25	II	Electrics technician
5	II	Clockmaker	31	VI	Wax chantler	5	I	Road construction worker	26	II	Electrical machine engineer
6	II	Engraver	32	VI	Glass finisher	6	I	Thermal and acoustic insulation fitter	27	III	Joiner
7	II	Metal former	33	VI	Building cleaner	7	I	Well sinker	28	III	Boat builder
8	II	Galvaniser	34	VII	Glass finisher	8	I	Stonemason	29	IV	Rope maker
9	II	Metal and bell founder	35	VII	Precision optician	9	I	Plasterer	30	V	Baker
10	II	Cutting tool mechanic	36	VII	Glass and china painter	10	I	Painter and lacquerer	31	V	Pastry-cook
11	II	Goldsmith and silversmith	37	VII	Precious stone engraver and cutter	11	I	Scaffolder	32	V	Butcher
12	III	Parquet layer	38	VII	Photographer	12	I	Chimney sweep	33	VI	Dispensing optician
13	III	Shutter mechatronics technician	39	VII	Bookbinder	13	II	Metal worker	34	VI	Hearing aid acoustician
14	III	Model builder	40	VII	Typesetter and printer	14	II	Surgical instrument maker	35	VI	Orthotic technician
15	III	Turner (ivory carver) and wooden toy maker	41	VII	Screen printer	15	II	Coachbuilder	36	VI	Orthopaedic shoemaker
16	III	Wood carver	42	VII	Flexographer	16	II	Precision engineer	37	VI	Dental technician
17	III	Cooper	43	VII	Ceramist	17	II	Motorbike and bicycle mechanic	38	VI	Hairdresser
18	III	Basket maker	44	VII	Organ and harmonium maker	18	II	Refrigeration mechanic	39	VII	Glazier
19	IV	Costume tailor	45	VII	Piano and harpsichord maker	19	II	Communication technician	40	VII	Glass blower and glass apparatus maker
20	IV	Embroiderer, weaver, knitter	46	VII	Reed an organ musical instrument maker	20	II	Automotive mechatronics technician	41	VII	Mechanic for tyres and vulcanization
21	IV	Milliner	47	VII	Violin maker	21	II	Mechanic for agricultural machinery			
22	IV	Sailmaker	48	VII	Bow maker						
23	IV	Furrier	49	VII	Metal wind instrument maker						
24	IV	Shoemaker	50	VII	Wooden wind instrument maker						
25	IV	Saddler	51	VII	Plucked instrument maker						
26	IV	Interior decorator	52	VII	Gilder						

Notes: The table shows deregulated craft occupations *(zulassungsfreies Handwerk)* that enter in Annex B1 of the German Trade and Crafts Code and regulated craft occupations *(zulassungspflichtiges Handwerk)* that enter in Annex A of the German Trade and Crafts Code. The group number groups craft occupations into that perform similar tasks. Group I: building and interior finishes trades *(Bau- und Ausbauhandwerke)*. Group II: electrical and metalworking trades *(Elektro- und Metallhandwerke)*. Group III: woodcrafts and plastic trades *(Holzhandwerke)*. Group IV: clothing, textiles and leather crafts and trades *(Bekleidungs-, Textil- und Lederhandwerke)*. Group V: food crafts and trades *(Nahrungsmittelhandwerke)*. Group VI: health and body care trades as well as the chemical and cleaning sector *(Gesundheits- und Körperpflege-, chemische und Reinigungshandwerke)*. Group VII: graphic design *(Glas-, Papier-, keramische und sonstige Handwerke)*.

Table A4.2 : Mapping of Occupations from Crafts and Trade Code to Classification of Occupations 1988: Deregulated Occupations

Crafts and Trade Code – Annex B1		Classification of Occupations 1988 (KldB88)	
Code	Name	Code	Name
1	Fliesen-, Platten- und Mosaikleger	483	Fliesenleger
2	Betonstein- und Terrazzohersteller	112	Formstein-, Betonhersteller
3	Estrichleger	486	Estrich-, Terrazzoleger
4	Behälter- und Apparatebauer	252	Behälterbauer, Kupferschmiede und verwandte Berufe
5	Uhrmacher	286	Uhrmacher
6	Graveure	232	Graveure, Ziseleure
7	Metallbildner	193	Metallzieher
7	Metallbildner	213	Sonstige Metallverformer (spanlose Verformung)
7	Metallbildner	225	Metallschleifer
7	Metallbildner	233	Metallvergüter
7	Metallbildner	244	Metallkleber und übrige Metallverbinder
8	Galvaniseure	234	Galvaniseure, Metallfärber
9	Metall- und Glockengießer	202	Formgießer
10	Schneidwerkzeugmechaniker	291	Werkzeugmacher
11	Gold- und Silberschmiede	302	Edelmetallschmiede
12	Parkettleger	–	n/a
13	Rollladen- und Sonnenschutztechniker	627	Übrige Fertigungstechniker
14	Modellbauer	306	Puppenmacher, Modellbauer, Präparatoren
15	Drechsler (Elfenbeinschnitzer) und Holzspielzeugmacher	183	Holzwarenmacher
16	Holzbildhauer	182	Holzverformer und zugehörige Berufe
17	Böttcher	503	Stellmacher, Böttcher
18	Korb- und Flechtwerkgestalter	184	Korb-, Flechtwarenmacher
19	Maßschneider	351	Schneider
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	342	Weber
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	346	Textilverflechter
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	354	Sticker
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	352	Oberbekleidungsnäher
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	356	Näher, a.n.g.
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	357	Sonstige Textilverarbeiter
21	Modisten	355	Hut-, Mützenmacher

Notes: The table continues next on page.

Table A4.2, cont'd

Crafts and Trade Code – Annex B1		Classification of Occupations 1988 (KldB88)	
Code	Name	Code	Name
22	Segelmacher	362	Textilausrüster
23	Kürschner	378	Fellverarbeiter
24	Schuhmacher	372	Schuhmacher
25	Sattler und Feintäschner	374	Groblederwarenhersteller, Bandagisten
25	Sattler und Feintäschner	375	Feinlederwarenhersteller
25	Sattler und Feintäschner	376	Lederbekleidungshersteller und sonstige Lederverarbeiter
26	Raumausstatter	491	Raumausstatter
27	Müller	432	Mehl-, Nährmittelhersteller
29	Brauer und Mälzer	422	Brauer, Mälzer
29	Weinküfer	421	Weinküfer
30	Textilreiniger	932	Textilreiniger, Färber und Chemischreiniger
31	Wachszieher	–	n/a
32	Gebäudereiniger	934	Glas-, Gebäudereiniger
33	Glasveredler	135	Glasbearbeiter, Glasveredler
34	Feinoptiker	135	Glasbearbeiter, Glasveredler
35	Glas- und Porzellanmaler	514	Kerammaler, Glasmaler
36	Edelsteinschleifer und -graveure	102	Eselsteinbearbeiter
37	Fotografen	837	Photographen
38	Buchbinder	163	Buchbinderberufe
39	Drucker	173	Buchdrucker (Hochdruck)
39	Drucker	174	Flach-, Tiefdrucker
40	Siebdrucker	175	Spezialdrucker, Siebdrucker
41	Flexografen	172	Druckstockhersteller
42	Keramiker	121	Keramiker
43	Orgel- und Harmoniumbauer	305	Musikinstrumentenbauer
44	Klavier- und Cembalobauer	305	Musikinstrumentenbauer
45	Handzuginstrumentenmacher	305	Musikinstrumentenbauer
46	Geigenbauer	305	Musikinstrumentenbauer
47	Bogenmacher	305	Musikinstrumentenbauer
48	Metallblasinstrumentenmacher	305	Musikinstrumentenbauer
49	Holzblasinstrumentenmacher	305	Musikinstrumentenbauer
50	Zupfinstrumentenmacher	305	Musikinstrumentenbauer
51	Vergolder	235	Emaillierer, Feuerverzinker und andere Metalloberflächenveredler
52	Schilder- und Lichtreklamehersteller	834	Dekorationen-, Schildermaler

Table A4.3 : Mapping of Occupations from Crafts and Trade Code to Classification of Occupations 1988: Regulated Occupations

Crafts and Trade Code – Annex A		Classification of Occupations 1988 (KldB88)	
Code	Name	Code	Name
1	Maurer und Betonbauer	441	Maurer
1	Maurer und Betonbauer	442	Betonbauer
2	Ofen- und Luftheizungsbauer	484	Ofensetzer, Luftheizungsbauer
3	Zimmerer	451	Zimmerer
4	Dachdecker	452	Dachdecker
5	Straßenbauer	462	Straßenbauer
6	Wärme-, und Kälte- und Schallschutzisolierer	482	Isolierer, Abdichter
7	Brunnenbauer	465	Kultur-, Wasserbauwerker
8	Steinmetzen und Steinbildhauer	101	Steinbearbeiter
9	Stukkateure	481	Stukkateure, Gipser, Verputzer
10	Maler und Lackierer	511	Maler, Lackierer (Ausbau)
10	Maler und Lackierer	512	Warenmaler, -lackierer
11	Gerüstbauer	453	Gerüstbauer
12	Schornsteinfeger	804	Schornsteinfeger
13	Metallbauer	301	Metallfeinbauer, a.n.g.
14	Chirurgiemechaniker	285	Sonstige Mechaniker
15	Karosserie- und Fahrzeugbauer	285	Sonstige Mechaniker
16	Feinwerkmechaniker	284	Feinmechaniker
17	Zweiradmechaniker	285	Sonstige Mechaniker
18	Kälteanlagenbauer	285	Sonstige Mechaniker
19	Informationstechniker	628	Sonstige Techniker
20	Kraftfahrzeugtechniker	621	Maschinenbautechniker
21	Landmaschinenmechaniker	621	Maschinenbautechniker
22	Büchsenmacher	211	Blechpresser, -zieher, -stanzer
23	Klempner	211	Blechpresser, -zieher, -stanzer
24	Klempner	261	Feinblechner
24	Installateur und Heizungsbauer	262	Rohrinstallateure
25	Elektrotechniker	311	Elektroinstallateure, -monteure
25	Elektrotechniker	622	Techniker des Elektofaches
26	Elektromaschinenbauer	314	Elektrogerätebauer
27	Tischler	501	Tischler
28	Boots- und Schiffbauer	275	Stahlbauschlosser, Eisenschiffbauer
29	Seiler	332	Spuler, Zwirner, Seiler
30	Bäcker	391	Backwarenhersteller
31	Konditoren	392	Konditoren
32	Fleischer	401	Fleischer
33	Augenoptiker	304	Augenoptiker
34	Hörgeräteakustiker	–	n/a
35	Orthopädietechniker	628	Sonstige Techniker
36	Orthopädieschuhmacher	–	n/a
37	Zahntechniker	303	Zahntechniker
38	Friseure	901	Friseure
39	Glaser	485	Glaser
40	Glasbläser und Glasapparatebauer	132	Hohlglasmacher
40	Glasbläser und Glasapparatebauer	133	Flachglasmacher
40	Glasbläser und Glasapparatebauer	134	Glasbläser (vor der Lampe)
41	Vulkaniseure und Reifenmechaniker	144	Vulkaniseure

Table A4.4 : Mapping of Occupations from Crafts and Trade Code to Classification of Occupations 1992 in the Microcensus

Deregulated		Regulated	
Crafts and Trade Code	KldB92	Crafts and Trade Code	KldB92
1. Fliesen-, Platten- und Mosaikleger	4830, 4831, 4832, 4833, 4837, 4839	1. Maurer und Betonbauer	4410, 4411, 4412, 4413, 4414, 4415, 4416, 4417, 4419, 4420, 4421, 4422, 4423, 4424, 4425
2. Betonstein- und Terrazzohersteller	1120, 1121, 1122, 1123, 1124, 1125, 1127, 1129	2. Ofen- und Luftheizungsbauer	4840, 4841, 4842, 4843, 4847
3. Estrichleger	4860, 4861, 4862, 4863, 4864, 4867, 4869	3. Zimmerer	4870, 4871, 4872, 4873, 4874, 4875, 4876, 4877, 4879
4. Behälter- und Apparatebauer	2520, 2521, 2522, 2527, 2529	4. Dachdecker	4880, 4881, 4882, 4883, 4884, 4885, 4887, 4889
5. Uhrmacher	3080, 3081, 3082, 3083, 3084, 3086, 3087, 3089	5. Straßenbauer	4610, 4611, 4612, 4613, 4614, 4615, 4617, 4619
6. Graveure	2940, 2941, 2942, 2943, 2944, 2947, 2949	6. Wärme-, Kälte- und Schallschutzisolierer	4820, 4821, 4822, 4823, 4824, 4825, 4826, 4827, 4828, 4829
7. Metallbildner	3231, 3232, 3237, 3239, 2016	7. Brunnenbauer	4662
8. Galvaniseure	2340, 2341, 2342, 2343, 2347, 2349	8. Steinmetzen und Steinbildhauer	1010, 1011, 1012, 1013, 1014, 1015, 1016, 1017, 1018, 1019
9. Metall- und Glockengießer	2010, 2011, 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2019	9. Stukkateure	4810, 4811, 4812, 4813, 4814, 4817, 4819
10. Schneidwerkzeugmechaniker	2951, 2952, 2953, 2954, 2957, 2959	10. Maler und Lackierer	5101, 5102, 5103, 5107, 5109, 5110, 5111, 5112, 5113, 5114, 5115, 5116, 5117, 5119, 5120, 5121, 5122, 5123, 5124, 5125, 5126, 5127, 5128, 5129
11. Gold- und Silberschmiede	3021, 3022	11. Gerüstbauer	4431, 4437
12. Parkettleger	4915, 4916	12. Schornsteinfeger	8041, 8042
13. Rollladen- und Sonnenschutztechniker	2591	13. Metallbauer	2540, 2541, 2542, 2543, 2547, 2549, 2550, 2551, 2552, 2553, 2557, 2559, 2560, 2561, 2562, 2563, 2567, 2591, 2599
14. Modellbauer	5021, 5022, 5023, 5024, 5025, 5026, 5027, 5028, 5029	14. Chirurgiemechaniker	8570
15. Drechsler und Holzspielzeugmacher	1851, 1855	15. Karosserie- und Fahrzeugbauer	2870, 2871, 2872, 2873, 2877, 2879
16. Holzbildhauer	1852	16. Feinwerkmechaniker	3000
17. Böttcher	5062	17. Zweiradmechaniker	2813
18. Korb- und Flechtwerkgestalter	1858	18. Kälteanlagenbauer	2661, 2662, 2667
19. Maßschneider	3510, 3511, 3512, 3513, 3514, 3515, 3516, 3517, 3518, 3519	19. Informationstechniker	3171
20. Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	3520, 3521, 3522, 3523, 3524, 3525, 3526, 3527, 3529, 3591, 3410, 3411, 3412, 3413, 3414, 3415, 3416, 3417, 3419, 3418, 3440	20. Kraftfahrzeugtechniker	2810
21. Modisten	3541, 3542, 3543	21. Landmaschinenmechaniker	2821
22. Segelmacher	3581	22. Büchsenmacher	3003
23. Kürschner	3780, 3781, 3782, 3783, 3784, 3787, 3789	23. Klempner	2610, 2611, 2612, 2613, 2614, 2617, 2619

Notes: The table is continued on next page.

Table A4.4, cont'd

Deregulated		Regulated	
Crafts and Trade Code	KldB92	Crafts and Trade Code	KldB92
24. Schuhmacher	3720, 3721	24. Installateur und Heizungsbauer	2680, 2681, 2682, 2687, 2690, 2691, 2697, 2699
25. Sattler und Feintäschner	3741, 3742, 3743, 3744, 3745, 3747	25. Elektrotechniker	6220, 6221, 6222, 6223, 6224, 6225, 6226, 6228, 6229
26. Raumausstatter	4910, 4911	26. Elektromaschinenbauer	3130, 3131, 3132, 3133, 3134, 3137, 3139
27. Müller	4351	27. Tischler	5010, 5011, 5012, 5013, 5014, 5015, 5016, 5017, 5018, 5019
28. Brauer und Mälzer	4210, 4211, 4212, 4217, 4219	28. Boots- und Schiffbauer	5063, 5064, 5065
29. Weinküfer	4233	29. Seiler	3323
30. Textilreiniger	9310, 9311, 9312, 9313, 9314, 9315, 9317, 9318, 9319	30. Bäcker	3910, 3911, 3912, 3913, 3914, 3915, 3917, 3918, 3919
31. Wachszieher	1418	31. Konditoren	3920, 3921, 3922, 3923, 3924, 3925, 3927, 3929
32. Gebäudereiniger	9340, 9341, 9342, 9343, 9349	32. Fleischer	4010, 4011, 4012, 4013, 4014, 4015, 4017, 4018, 4019
33. Glasveredler	1350,1351, 1352, 1353, 1354, 1355	33. Augenoptiker	3041
34. Feinoptiker	1356, 1357, 1358, 1359	34. Hörgeräteakustiker	3153
35. Glas- und Porzellanmaler	5140, 5141, 5142, 5143, 5144, 5145, 5146, 5147, 5149	35. Orthopädietechniker	3071
36. Edelsteinschleifer und -graveure	1018	36. Orthopädieschuhmacher	3722
37. Fotografen	8370, 8371, 8372, 8373, 8374, 8375, 8376, 8378, 8379	37. Zahntechniker	3031, 3032, 3037
38. Buchbinder	1780, 1781, 1782, 1783, 1784, 1785, 1789	38. Friseure	9010, 9011, 9012, 9013, 9014, 9015, 9016, 9017, 9018, 9019
39. Drucker	1740, 1741, 1742, 1743, 1749, 1750	39. Glaser	4850, 4851, 4852, 4853, 4854, 4855, 4856, 4857, 4859
40. Siebdrucker	1751	40. Glasbläser und Glasapparatebauer	1310, 1311, 1312, 1313, 1314, 1315, 1316, 1317, 1318, 1319
41. Flexografen	1736	41. Vulkaniseure und Reifenmechaniker	1450, 1451, 1452, 1453, 1454, 1456, 1457, 1458, 1459, 1500, 1501, 1507
42. Keramiker	1210, 1211, 1212, 1213, 1214, 1215, 1216, 1217, 1218, 1219		
43. Orgel- und Harmoniumbauer	3052		
44. Klavier- und Cembalobauer	3051		
45. Handzuginstrumentenmacher	3058		
46. Geigenbauer	3054		
47. Bogenmacher			
48. Metallblasinstrumentenmacher	3053		
49. Holzblasinstrumentenmacher	3056		
50. Zupfinstrumentenmacher	3055		
51. Vergolder	5126		
52. Schilder- und Lichtreklamehersteller	8390, 8391, 8392, 8393, 8394, 8395, 8397, 8399		

Table A4.5 : Yearly Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Log daily earnings		Unemployment		Attrition	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
$\text{Deregulated}_{j(i)} \times I(t=2014)$	-0.0701***	-0.0431***	0.0156***	0.0091**	0.0150***	0.0039
	(0.0060)	(0.0114)	(0.0030)	(0.0043)	(0.0040)	(0.0071)
$\text{Deregulated}_{j(i)} \times I(t=2013)$	-0.0643***	-0.0458***	0.0106***	0.00314	0.0188***	0.0071
	(0.0060)	(0.0105)	(0.0030)	(0.0042)	(0.0040)	(0.0068)
$\text{Deregulated}_{j(i)} \times I(t=2012)$	-0.0535***	-0.0334***	0.0174***	0.0091**	0.0178***	0.0182***
	(0.0059)	(0.0101)	(0.0030)	(0.0041)	(0.0040)	(0.0063)
$\text{Deregulated}_{j(i)} \times I(t=2011)$	-0.0525***	-0.0350***	0.0141***	0.0053	0.0114***	0.0132**
	(0.0059)	(0.0095)	(0.0029)	(0.0040)	(0.0040)	(0.0060)
$\text{Deregulated}_{j(i)} \times I(t=2010)$	-0.0448***	-0.0224**	0.0159***	0.0086**	0.0097**	0.0144**
	(0.0059)	(0.0094)	(0.0029)	(0.0042)	(0.0040)	(0.0056)
$\text{Deregulated}_{j(i)} \times I(t=2009)$	-0.0601***	-0.0374***	0.0190***	0.0148***	0.0067*	0.0169***
	(0.0058)	(0.0090)	(0.0029)	(0.0041)	(0.0040)	(0.0052)
$\text{Deregulated}_{j(i)} \times I(t=2008)$	-0.0207***	-0.0180**	0.0094***	0.0093**	-0.0008	0.0103**
	(0.0058)	(0.0074)	(0.0029)	(0.0036)	(0.0040)	(0.0048)
$\text{Deregulated}_{j(i)} \times I(t=2007)$	-0.0207***	-0.0198***	0.0081***	0.0083**	-0.0014	0.0051
	(0.0058)	(0.0076)	(0.0029)	(0.0035)	(0.0040)	(0.0044)
$\text{Deregulated}_{j(i)} \times I(t=2006)$	-0.0101*	-0.0042	0.0042	0.0014	0.0030	0.0094**
	(0.0057)	(0.0071)	(0.0029)	(0.0037)	(0.0040)	(0.0039)
$\text{Deregulated}_{j(i)} \times I(t=2005)$	-0.0051	0.0001	0.0017	0.0023	0.0018	0.0056*
	(0.0057)	(0.0069)	(0.0029)	(0.0034)	(0.0040)	(0.0031)
$\text{Deregulated}_{j(i)} \times I(t=2004)$	-0.0056	-0.0114**	-0.00007	0.0026	-0.0034	-0.0033
	(0.0056)	(0.0045)	(0.0028)	(0.0027)	(0.0040)	(0.0024)
Baseline: $t = 2003$						
$\text{Deregulated}_{j(i)} \times I(t=2002)$	0.0070	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0056)	(0.0018)	(0.0028)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t=2001)$	0.0082	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0056)	(0.0022)	(0.0028)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t=2000)$	0.0093*	0.0000	-0.0016	-0.0022	0.0000	0.0000
	(0.0056)	(0.0032)	(0.0028)	(0.0014)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t=1999)$	0.0018	0.0000	0.0019	-0.0006	0.0000	0.0000
	(0.0056)	(0.0040)	(0.0028)	(0.0020)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t=1998)$	0.0061	0.0000	0.0011	0.0014	0.0000	0.0000
	(0.0056)	(0.0047)	(0.0028)	(0.0022)	(0.0040)	(0.0000)
Constant	4.254***	4.206***	0.0169***	0.0165***	0.0000	0.0000
	(0.0020)	(0.0034)	(0.0010)	(0.0015)	(0.0014)	(0.0000)
R-squared	0.035	0.022	0.018	0.021	0.098	0.103
Observations	496,084	493,568	503,705	501,156	532,559	529,669
Individuals	31,327	31,157	31,327	31,157	31,327	31,157

Notes: The table shows yearly treatment effects of the reform on log of gross daily earnings in Columns (1) and (2), on being unemployed in Columns (3) and (4), and on attrition in Columns (5) and (6). The outcome variable is log gross daily earnings. Column (1) refers to the non-matched sample, Column (2) to the matched sample from entropy balancing. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.6 : Other Labor Market Adjustment Mechanisms

	(1)	(2)	(3)	(4)
	Full-time regular employee		Marginal employment	
	Non-matched	Matched	Non-matched	Matched
	Panel A: average effects			
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0140***	-0.0029	0.0074***	0.0026
	(0.0015)	(0.0044)	(0.006)	(0.0019)
Constant	0.942***	0.926***	-0.0008	-0.0008
	(0.0012)	(0.0021)	(0.0005)	(0.0006)
R-squared	0.050	0.058	0.016	0.020
	Panel B: yearly effects			
Deregulated$_{j(i)}$ × $I(t = 2014)$	-0.0252***	-0.0033	0.0149***	0.0069*
	(0.0043)	(0.0076)	(0.0019)	(0.0040)
Deregulated$_{j(i)}$ × $I(t = 2013)$	-0.0173***	0.0042	0.0142***	0.0101***
	(0.0043)	(0.0073)	(0.0019)	(0.0035)
Deregulated$_{j(i)}$ × $I(t = 2012)$	-0.0239***	0.0006	0.0110***	0.0037
	(0.0043)	(0.0072)	(0.0019)	(0.0036)
Deregulated$_{j(i)}$ × $I(t = 2011)$	-0.0267***	-0.0019	0.0098***	0.0025
	(0.0043)	(0.0071)	(0.0019)	(0.0034)
Deregulated$_{j(i)}$ × $I(t = 2010)$	-0.0209***	-0.0056	0.0086***	0.00007
	(0.0042)	(0.0066)	(0.0019)	(0.0033)
Deregulated$_{j(i)}$ × $I(t = 2009)$	-0.0242***	-0.0149**	0.0107***	0.0041
	(0.0042)	(0.0062)	(0.0019)	(0.0031)
Deregulated$_{j(i)}$ × $I(t = 2008)$	-0.0112***	-0.0091*	0.0067***	0.0015
	(0.0042)	(0.0055)	(0.0018)	(0.0025)
Deregulated$_{j(i)}$ × $I(t = 2007)$	-0.0072*	-0.0067	0.0056***	0.0009
	(0.0042)	(0.0053)	(0.0018)	(0.0024)
Deregulated$_{j(i)}$ × $I(t = 2006)$	-0.0004	-0.0006	0.0033*	-0.0004
	(0.0041)	(0.0051)	(0.0018)	(0.0022)
Deregulated$_{j(i)}$ × $I(t = 2005)$	0.0041	0.0011	0.0014	-0.0003
	(0.0041)	(0.0046)	(0.0018)	(0.0019)
Deregulated$_{j(i)}$ × $I(t = 2004)$	0.0032	-0.0014	0.0009	0.0014
	(0.0041)	(0.0034)	(0.0018)	(0.0013)
Baseline: $t = 2003$				
Deregulated$_{j(i)}$ × $I(t = 2002)$	0.0010	-0.0014	0.0000	0.0000
	(0.0041)	(0.0013)	(0.0018)	(0.0000)
Deregulated$_{j(i)}$ × $I(t = 2001)$	0.0001	-0.0017	0.0000	0.0000
	(0.0041)	(0.0016)	(0.0018)	(0.0000)
Deregulated$_{j(i)}$ × $I(t = 2000)$	0.0020	0.0003	0.0005	-0.0002
	(0.0041)	(0.0028)	(0.0018)	(0.0006)
Deregulated$_{j(i)}$ × $I(t = 1999)$	0.0004	0.0005	0.0004	-0.0001
	(0.0041)	(0.0035)	(0.0018)	(0.0006)
Deregulated$_{j(i)}$ × $I(t = 1998)$	0.0035	-0.0011	0.0000	–
	(0.0041)	(0.0041)	(0.0018)	–
Constant	0.942***	0.9270***	-0.0008	-0.0008
	(0.0014)	(0.0029)	(0.0006)	(0.0006)
R-squared	0.050	0.058	0.016	0.020
Observations	503,705	501,156	503,705	501,156
Individuals	31,327	31,157	31,327	31,157

Notes: The table shows reform effects on being in full-time regular employment in Columns (1) and (2) and on being in marginal employment in Columns (3) and (4) from linear probability models. Marginal employment is recorded from the year 1999 onwards in the SIAB data. Columns (1) and (3) refer to results using the non-matched comparison group and Columns (2) and (4) refer to results using the matched comparison group. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.7 : Average Earnings Growth After Matching

(1)	(2)	(3)	(4)	(5)
Period	Deregulated	Regulated	Difference	
			Δ	p-value
1998-2003	0.233	0.234	-0.001	0.858
1998-2000	0.139	0.136	0.003	0.673
2000-2003	0.093	0.096	-0.003	0.556
1994-2003	0.510	0.523	-0.013	0.544

Notes: The table shows average growth rates of log daily gross earnings after matching.
Source: SIAB

Table A4.8 : Checking Common Trends in Pretreatment Periods

	(1)	(2)	(3)	(4)
	Baseline sample		New sample	
	Non-matched	Matched	Non-matched	Matched
Panel A: average effects				
Deregulated$_{j(i)}$ × post$_{1998}$	0.0010	0.0042	0.0043**	0.0056
	(0.0019)	(0.0041)	(0.0019)	(0.0040)
Constant	4.030***	3.991***	4.141***	4.088***
	(0.0013)	(0.0029)	(0.0012)	(0.0018)
R-squared	0.245	0.240	0.049	0.069
Panel B: yearly effects				
Deregulated$_{j(i)}$ × $I(t = 2003)$	-0.0061	0.0000	-0.0162***	-0.0003
	(0.0043)	(0.0047)	(0.0045)	(0.0028)
Deregulated$_{j(i)}$ × $I(t = 2002)$	0.0008	0.0000	-0.0087*	-0.0028
	(0.0043)	(0.0043)	(0.0045)	(0.0061)
Deregulated$_{j(i)}$ × $I(t = 2001)$	0.0020	0.0000	0.0047	0.0111**
	(0.0043)	(0.0042)	(0.0045)	(0.0049)
Deregulated$_{j(i)}$ × $I(t = 2000)$	0.0032	0.0000	0.0029	0.0099**
	(0.0043)	(0.0041)	(0.0044)	(0.0039)
Deregulated$_{j(i)}$ × $I(t = 1999)$	-0.0042	0.0000	-0.0010	0.0042
	0.0043	(0.0037)	(0.0044)	(0.0031)
Baseline: $t = 1998$				
Deregulated$_{j(i)}$ × $I(t = 1997)$	-0.0111**	-0.0100**	-0.0074*	0.0000
	0.0044	(0.0040)	(0.0044)	(0.0012)
Deregulated$_{j(i)}$ × $I(t = 1996)$	-0.0059	-0.0062	-0.0080*	0.0000
	0.0044	(0.0048)	(0.0044)	(0.0016)
Deregulated$_{j(i)}$ × $I(t = 1995)$	-0.0048	-0.0081	-0.0070	0.0000
	0.0044	(0.0059)	(0.0044)	(0.0026)
Deregulated$_{j(i)}$ × $I(t = 1994)$	-0.0018	-0.0022	-0.0166***	0.0000
	0.0045	(0.0070)	(0.0044)	(0.0036)
Deregulated$_{j(i)}$ × $I(t = 1993)$	0.0136***	0.0013	-0.0082*	0.0000
	0.0045	(0.0075)	(0.0044)	(0.0041)
Constant	4.027***	3.990***	4.142***	4.088***
	(0.0016)	(0.0047)	(0.0015)	(0.0024)
R-squared	0.245	0.240	0.049	0.069
Observations	332,153	330,383	374,522	357,047
Individuals	31,327	31,157	34,924	32,945

Notes: The table checks common trends in pretreatment periods. The dependent variable in all regressions is log gross daily earnings. Columns (1) and (2) use the same analytical sample that we use in our baseline regression without further restrictions. Columns (3) and (4) use a new analytical sample, pretending that the reform has happened in the year 1998. Matching is based on the same set of covariats as in the baseline analysis. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.9 : Labor Market Effects for Incumbent Workers: Dropping Sample Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Earnings		Unemployment		Attrition	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
Deregulated$_{j(i)} \times$ post$_{2003}$	-0.0353***	-0.0246***	0.0059***	0.0045*	0.0012	0.0043
	(0.0021)	(0.0064)	(0.0010)	(0.0026)	(0.0014)	(0.0036)
Constant	4.113***	4.135***	0.0476***	0.0421***	0.0130***	0.0000
	(0.0017)	(0.0032)	(0.0008)	(0.0017)	(0.0011)	(0.0011)
R-squared	0.034	0.016	0.011	0.013	0.051	0.106
Observations	812,246	657,423	836,388	671,225	914,090	711,382
Individuals	53,770	41,846	53,770	41,846	53,770	41,846

Notes: The table shows results from regressions on the sample that includes occupational and firm switches before the reform and non-regular employees (including unemployment and apprenticeship spells). All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.10 : Effect Heterogeneity by Industry Group

	(1)	(2)	(3)	(4)
	Industry group			
	Manufacturing	Construction	Trade, maintenance, and repair	Real estate, renting, and other business activities
	Panel A: log daily earnings			
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0222***	-0.0469**	-0.0408*	-0.0119
	(0.0070)	(0.0187)	(0.0240)	(0.0326)
Constant	4.261***	4.178***	4.026***	3.909***
	(0.0030)	(0.0092)	(0.0114)	(0.0133)
R-squared	0.026	0.016	0.013	0.015
Observations	232,001	136,192	41,630	83,745
Individuals	14,475	8,726	2,627	5,329
	Panel B: unemployment			
Deregulated$_{j(i)}$ × post$_{2003}$	0.0055**	0.0096	0.0184*	0.0069
	(0.0026)	(0.0086)	(0.0094)	(0.0132)
Constant	0.0138***	0.0288***	0.0259***	0.0283***
	(0.0013)	(0.0050)	(0.0053)	(0.0059)
R-squared	0.020	0.026	0.024	0.028
Observations	235,033	138,910	42,206	85,007
Individuals	14,475	8,726	2,627	5,329
	Panel C: attrition			
Deregulated$_{j(i)}$ × post$_{2003}$	0.0042	0.0491***	0.0187	0.0270*
	(0.0039)	(0.0148)	(0.0144)	(0.0162)
Constant	0.0000	0.0000	0.0000	0.0000
	(0.0012)	(0.0047)	(0.0046)	(0.0052)
R-squared	0.095	0.151	0.119	0.125
Observations	246,075	148,342	44,659	90,593
Individuals	14,475	8,726	2,627	5,329

Notes: The table shows average effects of the reform on log gross daily earnings in Panel A, on being unemployed in Panel B, and on dropping out of the sample (incl. missing/zero earnings) in Panel C. Sample splits, indicated in the column header, are based on the industry group in the year 2003. *Trade, maintenance, and repair* contains wholesale and retail trade, repair of motor vehicles, motorcycles and personal and household goods. Due to small samples, *real estate, renting, and other business activities* also contains other industries that we do not further distinguish. All regressions include year and individual fixed effects. Entropy balancing is rerun on each subsample and workers in the comparison group are weighted by these matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.11 : Reform Effects by Occupational and Firm Switches

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Switch			Log daily earnings			
				Occupation		Firm	
	Occupation	Occupational group	Firm	Same	Different	Same	Different
Deregulated$_{j(i)}$ × post$_{2003}$	0.0060	0.0134***	-0.0040	-0.0098**	-0.0428**	-0.0119***	-0.0396***
	(0.0050)	(0.0047)	(0.0056)	(0.0043)	(0.0174)	(0.0045)	(0.0136)
Constant	0.0865***	0.0691***	0.1270***	4.238***	4.119***	4.234***	4.157***
	(0.0025)	(0.0023)	(0.0027)	(0.0025)	(0.0063)	(0.0027)	(0.0051)
R-squared	0.105	0.090	0.160	0.095	0.045	0.121	0.028
Observations	355,020	355,020	353,079	357,464	136,104	292,099	201,469
Individuals	31,157	31,157	31,157	21,529	9,628	17,544	13,613

Notes: In Column (1), the dependent variable is an indicator variable that is one if the worker has changed occupation. In Column (2), the dependent variable is an indicator variable that is one if the worker has changed the occupation group. In Column (3), the dependent variable is an indicator variable that is one if the worker has changed the firm. The analysis in these two columns is restricted to the years 1999 to 2010 because of non-comparable occupational classifications in the other years. In Columns (4) to (7), the dependent variable is log daily gross earnings. Columns (4) and (5) split the sample by whether the worker works in the same or in a different occupation in 2010 compared to 2003. Columns (6) and (7) split the sample by whether the worker works in the same or in a different firm in 2010 compared to 2003. All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.12 : Excluding Occupations with Similar Tasks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deregulated occupations:	Tile, slab and mosaic layer and cast stone and terrazzo maker		Metal former, galvaniser, and metal and bell founder		Interior decorator		All	
Regulated occupations:	Bricklayer and concretor		Metal worker		Installer and heating fitter		All	
	Earnings	Unemployed	Earnings	Unemployed	Earnings	Unemployed	Earnings	Unemployed
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0231***	0.0067***	-0.0339***	0.0102***	-0.0238***	0.0068***	-0.0353***	0.0104***
	(0.0066)	(0.0025)	(0.0063)	(0.0024)	(0.0063)	(0.0024)	(0.0068)	(0.0026)
Constant	4.201***	0.0171***	4.189***	0.0169***	4.210***	0.0169***	4.186***	0.0162***
	(0.0028)	(0.0013)	(0.0027)	(0.0013)	(0.0027)	(0.0012)	(0.0030)	(0.0013)
R-squared	0.023	0.020	0.020	0.021	0.022	0.021	0.021	0.021
Observations	453,326	460,035	477,717	485,070	461,223	468,362	405,130	411,155
Indivudals	28,563	28,563	30,172	30,172	29,109	29,109	25,530	25,530

Notes: The table shows regressions for log daily earnings and unemployment in subsamples that drop occupations indicated in the column header. Columns (7) and (8) drop all occupations mentioned in the other columns. All regressions include year and individual fixed effects. Entropy balancing is rerun on each subsample and workers in the comparison group are weighted by these matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.13 : Attrition from Sample: Yearly Treatment Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Dropped out		Missing earnings		Both	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
Deregulated$_{j(i)} \times I(t = 2014)$	0.00755**	0.00051	0.0088***	0.0041	0.0150***	0.0039
	(0.0037)	(0.0067)	(0.0023)	(0.0034)	(0.0040)	0.0071
Deregulated$_{j(i)} \times I(t = 2013)$	0.0121***	0.0045	0.0079***	0.0036	0.0188***	0.0071
	(0.0037)	(0.0064)	(0.0022)	(0.0033)	(0.0040)	0.0068
Deregulated$_{j(i)} \times I(t = 2012)$	0.0096***	0.0168***	0.0096***	0.0033	0.0178***	0.0182***
	(0.0037)	(0.0058)	(0.0022)	(0.0034)	(0.0040)	0.0063
Deregulated$_{j(i)} \times I(t = 2011)$	0.0071*	0.0125**	0.0049**	0.0022	0.0114***	0.0132**
	(0.0037)	(0.0055)	(0.0022)	(0.0033)	(0.0040)	0.0060
Deregulated$_{j(i)} \times I(t = 2010)$	0.0004	0.0085*	0.0098***	0.0071**	0.0097**	0.0144**
	(0.0037)	(0.0049)	(0.0022)	(0.0035)	(0.0040)	0.0056
Deregulated$_{j(i)} \times I(t = 2009)$	-0.0013	0.0107**	0.0082***	0.0079**	0.0067*	0.0169***
	(0.0037)	(0.0045)	(0.0022)	(0.0031)	(0.0040)	0.0052
Deregulated$_{j(i)} \times I(t = 2008)$	-0.0078**	0.0034	0.0069***	0.0080**	-0.0008	0.0103**
	(0.0037)	(0.0039)	(0.0022)	(0.0031)	(0.0040)	0.0048
Deregulated$_{j(i)} \times I(t = 2007)$	-0.0018	0.0049	0.0008	0.0018	-0.0014	0.0051
	(0.0037)	(0.0035)	(0.0022)	(0.0029)	(0.0040)	0.0044
Deregulated$_{j(i)} \times I(t = 2006)$	-0.0005	0.0018	0.0037*	0.0082***	0.0030	0.0094**
	(0.0037)	(0.0029)	(0.0021)	(0.0027)	(0.0040)	0.0039
Deregulated$_{j(i)} \times I(t = 2005)$	-0.0003	0.0012	0.0022	0.0046**	0.0018	0.0056*
	(0.0037)	(0.0021)	(0.0021)	(0.0022)	(0.0040)	0.0031
Deregulated$_{j(i)} \times I(t = 2004)$	-0.0004	-0.0009	-0.0030	-0.0023*	-0.0034	-0.0033
	(0.0037)	(0.0020)	(0.0021)	(0.0013)	(0.0040)	0.0024
Baseline: $t = 2003$						
Deregulated$_{j(i)} \times I(t = 2002)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
Deregulated$_{j(i)} \times I(t = 2001)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
Deregulated$_{j(i)} \times I(t = 2000)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
Deregulated$_{j(i)} \times I(t = 1999)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
Deregulated$_{j(i)} \times I(t = 1998)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
Constant	0.0000	0.0000	-0.0023***	-0.0020***	0.0000	0.0000
	(0.0010)	(0.0009)	(0.0007)	(0.0005)	(0.0014)	(0.0011)
R-squared	0.089	0.092	0.019	0.021	0.098	0.103
Observations	532,559	529,669	503,705	501,156	532,559	529,669
Individuals	31,327	31,157	31,327	31,157	31,327	31,157

Notes: The table documents the extent of sample attrition. In Columns (1) and (2), the dependent variable is an indicator variable that is one if the individual dropped out from the social security records, zero otherwise. In Columns (3) and (4), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings, zero otherwise. In Columns (5) and (6), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings or dropped out of the sample, zero otherwise. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.14 : Earnings of Workers Dropping Out of the Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2004–2014		2004–2006		2004–2008		2009–2014	
Outsample$_i$	-0.1138***	-0.1157***	-0.1500***	-0.1769***	-0.1486***	-0.1646***	-0.1033***	-0.1018***
	(0.0082)	(0.0123)	(0.0159)	(0.0261)	(0.0117)	(0.0179)	(0.0084)	(0.0124)
Outsample$_i$ × Deregulated$_{j(i)}$		0.0037		0.0517		0.0315		-0.0029
		(0.0165)		(0.0320)		(0.0234)		(0.0169)
Deregulated$_{j(i)}$		-0.0007		-0.0030		-0.0037		0.0014
		(0.0089)		(0.0078)		(0.0080)		(0.0089)
Constant	4.388***	4.388***	4.362***	4.364***	4.371***	4.373***	4.380***	4.380***
	(0.0044)	(0.0067)	(0.0038)	(0.0057)	(0.0040)	(0.0059)	(0.0044)	(0.0067)
R-squared	0.017	0.017	0.010	0.010	0.015	0.016	0.013	0.013

Notes: The dependent variable is log gross daily earnings. Sample is restricted to the year 2003. *Outsample* is an indicator variable that is one if the worker disappears from the analysis (due to dropping out from the records or due to missing and zero earnings), zero otherwise. In Columns (1) and (2), we consider all dropouts from the sample over all years from 2004 to 2014. In Columns (3) and (4), we consider early dropouts from the sample over the first two years from 2004 to 2006. In Columns (5) and (6), we consider early dropouts from the sample over the years from 2004 to 2008. In Columns (7) and (8), we consider late dropouts from the sample over the years from 2009 to 2014. $N = 31,157$. Workers in the comparison group are weighted by matching weights. Robust standard errors in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.15 : Earnings of Workers Dropping Out of the Sample: Controlling for Individual Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	All dropouts: 2004–2014			Early dropouts: 2004–2006		
Outsample$_i$	-0.1039***	-0.0651***	-0.0573***	-0.1696***	-0.1139***	-0.1048***
	(0.0118)	(0.0111)	(0.0110)	(0.0251)	(0.0214)	(0.0209)
Outsample$_i$ × Deregulated$_{j(i)}$	0.0104	0.0151	0.0292**	0.0629**	0.0511*	0.0714***
	(0.0160)	(0.0148)	(0.0144)	(0.0310)	(0.0267)	(0.0255)
Deregulated$_{j(i)}$	-0.0210	-0.0427**	-0.0225	-0.0208	-0.0413**	-0.0207
	(0.0206)	(0.0192)	(0.0259)	(0.0197)	(0.0190)	(0.0259)
Constant	4.289***	4.481***	4.405***	4.261***	4.472***	4.398***
	(0.0170)	(0.0157)	(0.0198)	(0.0163)	(0.0156)	(0.0199)
Education control variables	yes	yes	yes	yes	yes	yes
Personal control variables	no	yes	yes	no	yes	yes
Job control variables	no	no	yes	no	no	yes
R-squared	0.055	0.319	0.319	0.051	0.319	0.384

Notes: The dependent variable is log gross daily earnings. Sample is restricted to the year 2003. *Outsample* is an indicator variable that is one if the worker disappears from the analysis (due to dropping out from the records or due to missing and zero earnings), zero otherwise. Columns (1) to (3) consider all dropouts from the sample over all years from 2004 to 2014. Columns (4) to (6) consider early dropouts from the sample over the first two years from 2004 to 2006. Control characteristics include the linear effect of the control variable and interactions between the control variable and *deregulated*. *Education control variables:* training (4 cat.) and schooling (3 cat.). *Personal control variables:* age (demeaned), age (demeaned) squared, gender, foreign citizenship. *Age* is demeaned to facilitate interpretation. *Job control variables:* industry (5 cat.), job tenure (4 cat.), and firm tenure (4 cat.). $N = 31,157$. Workers in the comparison group are weighted by matching weights. Robust standard errors in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A4.16 : Treatment Effects with Matching on Craft Groups

	(1)	(2)	(3)
	Earnings	Unemployment	Attrition
	Panel A: average effects		
Deregulated$_{j(i)}$ × post$_{2003}$	-0.0347***	0.0086	0.0097
	(0.0108)	(0.0053)	(0.0067)
Constant	4.205***	0.0169***	0.0000
	(0.0045)	(0.0025)	(0.0021)
R-squared	0.024	0.021	0.105
	Panel B: yearly effects		
Deregulated$_{j(i)} \times I(t = 2014)$	-0.0756***	0.0085	0.0008
	(0.0156)	(0.0084)	0.0139
Deregulated$_{j(i)} \times I(t = 2013)$	-0.0577***	0.0022	0.0041
	(0.0202)	(0.0094)	0.0133
Deregulated$_{j(i)} \times I(t = 2012)$	-0.0416**	0.0020	0.0224**
	(0.0186)	(0.0113)	0.0109
Deregulated$_{j(i)} \times I(t = 2011)$	-0.0419**	0.0054	0.0152
	(0.0172)	(0.0087)	0.0166
Deregulated$_{j(i)} \times I(t = 2010)$	-0.0324*	0.0169**	0.0154
	(0.0184)	(0.0077)	0.0103
Deregulated$_{j(i)} \times I(t = 2009)$	-0.0519***	0.0191***	0.0130
	(0.0164)	(0.0071)	0.0098
Deregulated$_{j(i)} \times I(t = 2008)$	-0.0164	0.0127**	0.0101
	(0.0159)	(0.0055)	0.0086
Deregulated$_{j(i)} \times I(t = 2007)$	-0.0317***	0.0051	0.0063
	(0.0094)	(0.0078)	0.0075
Deregulated$_{j(i)} \times I(t = 2006)$	-0.0154	0.0076	0.0121*
	(0.0111)	(0.0068)	0.0065
Deregulated$_{j(i)} \times I(t = 2005)$	-0.0157*	0.0095*	0.0084*
	(0.0086)	(0.0055)	0.0050
Deregulated$_{j(i)} \times I(t = 2004)$	-0.0180***	0.0040	-0.0013
	(0.0054)	(0.0048)	0.0023
Baseline: $t = 2003$			
Deregulated$_{j(i)} \times I(t = 2002)$	0.0000	0.0000	0.0000
	(0.0018)	(0.0000)	(0.0000)
Deregulated$_{j(i)} \times I(t = 2001)$	0.0000	0.0000	0.0000
	(0.0025)	(0.0000)	(0.0000)
Deregulated$_{j(i)} \times I(t = 2000)$	0.0000	-0.0008	0.0000
	(0.0040)	(0.0018)	(0.0000)
Deregulated$_{j(i)} \times I(t = 1999)$	0.0000	-0.0023	0.0000
	(0.0065)	(0.0047)	(0.0000)
Deregulated$_{j(i)} \times I(t = 1998)$	0.0000	0.0022	0.0000
	(0.0065)	(0.0041)	(0.0000)
Constant	4.205***	0.0158***	0.0000
	(0.0063)	(0.0040)	(0.0021)
R-squared	0.024	0.021	0.105
Observations	493,568	501,156	529,669
Individuals	31,157	31,157	31,157

Notes: The table shows average effects of the reform when matching on craft groups instead of industries. All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.
Source: SIAB

5 Shocking Choice: Trade Shocks, Local Labour Markets and Vocational Occupation Choices

5.1 Introduction

How do individuals cope with changing economic conditions and structures? Understanding how individuals prepare for and react to change is a central economic question because it determines how they fare on the labour market. Economies transition from agricultural, to manufacturing, to service-based economies; a phenomenon commonly referred to as structural change. With these transitions, the composition of labour markets changes in terms of the shares of individuals working in agriculture, manufacturing or services, depending on the stage of development of an economy.[1] At least two factors contribute to this transition: technological progress (Levy and Murnane, 1992) and globalization (Dauth and Suedekum, 2016). Technological progress leads to automation and changes the modes of production and globalization with international trade leads to products being produced all over the world and traded. In such transition times, education is ever more important, because it enables the labour force to adapt to change, both in terms of technological advancement and new required skills (Nelson and Phelps, 1966). In particular, general, rather than specific skills are valuable, because they are transferable and widely applicable. Therefore, they insure the individual from unemployment (Krueger and Kumar, 2004).

This chapter looks at the impact of growing up in a region exposed to structural change on individuals' occupation choices. Do individuals enter vocational occupations that teach them specific or general skills? The focus lies on occupations of young adults in Germany who enter vocational education training through an apprenticeship. The main hypothesis is that individuals exposed to structural unemployment due to plant closures or mass lay-offs in their local labour market may be inclined to choose occupations that shelter them from such forces by teaching adaptable and transferable skills. If individuals fail to update their choices given the state of their local labour market and enter skill-specific occupations, this may have important economic consequences on their future labour market outcomes as they will be less able to adjust to potential new jobs and skill requirements.

To obtain causal evidence of the effects of growing up exposed to structural change, this chapter uses local labour market exposure to import competition (trade shocks) from China

[1] Between 1980 and 2010, the US manufacturing employment share has decreased by 52.0 percent from 21.0 percent to 10.1 percent and for Germany, with traditionally higher manufacturing employment shares, it has still declined by 40.9 percent, from 34.0 percent to 20.1 percent. (U.S. Bureau of Labour Statistics, Percent of Employment in Manufacturing in the United States and Germany, retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/USAPEFANA, [accessed 8 August, 2018].)

and Eastern Europe as an exogenous source of variation, following the seminal paper by Autor, Levy and Murnane (2003). Local labour market exposure to import competition has been shown to decrease local manufacturing employment shares (e.g. Autor, Dorn and Hanson, 2013; Dauth, Findeisen and Suedekum, 2014). This loss in manufacturing employment is the implicit first stage to the reduced form regressions of the effect of import competition on vocational occupation choice in this chapter.

Using longitudinal individual-level administrative social security data, the chapter analyses the causal effect of local import competition exposure at the age of 15 on vocational occupation choices and subsequent labour market outcomes. The data, a 2 percent sample of all individuals in Germany subject to social security, provide detailed information on individuals' occupational history, including the occupation during apprenticeship, earnings, age and which county they work in. My sample contains 192,025 individuals which includes occupational choices made between 1991 and 2013. I exploit the exogenous rise in trade volumes with both China, following the accession to the WTO and Eastern Europe, after the fall of the iron curtain as an exogenous supply shock of manufacturing good imports to Germany (Dauth, Findeisen and Suedekum, 2014).

The measure of local labour market exposure to import competition is defined as the 10-year change in Chinese and Eastern European import exposure per worker in a county. Imports are apportioned to the county according to its share of national industry employment. Variation stems from initial local industry structures with respect to manufacturing employment shares and within-manufacturing specialization patterns with respect to import-intensive industries. The import exposure measure is then the potential local per worker import exposure, given national industry import volumes, in the style of a shift-share measure (Bartik, 1991). In some manufacturing industries, such as textiles or toys in the case of China, or car parts and iron and steel in the case of Eastern Europe, these countries became competitive, started having a comparative advantage and exported goods to Germany, which then posed competitive pressure on regions specialized in these industries. I extend the literature on trade shocks by using time-varying local import exposure, exploiting both county-level and time variation in import exposure. One concern with import exposure is that employment and imports may be positively correlated with unobserved shocks to domestic product demand. This is particularly problematic here, as individuals' vocational education occupation is of central interest, and push and pull factors stemming from labour demand should be shut down as much as possible. To isolate the supply-driven component of imports from China and Eastern Europe, I use imports (and exports) to other high income countries as instruments for Chinese and Eastern European trade with Germany (following e.g., Autor, Dorn and Hanson, 2013; Dauth, Findeisen and Suedekum, 2014). Imports from China and Eastern Europe to other high income countries predict imports to Germany well, but are unrelated to German local labour supply and demand structures.

There is little evidence on adolescents' occupation choices and how they are affected, in particular in vocational education.[2] Vocational education training is the relevant point of entry into a broad class of middle-skill occupations that represent well over half of the German labour force (Statistisches Bundesamt, 2018*b*).[3] Occupations can be classified in terms of their generality and skill-specificity in a detailed and consistent manner. I differentiate between skill-specific and general occupations using a skill-weights-based occupational specificity measure (Lazear, 2009), with which I show that manufacturing and craft occupations are more skill-specific than service and merchant occupations. The chapter analyses to what extent individuals protect themselves from future unemployment from structural change by possessing skills which are transferable and applicable to changing technology or new occupations. For instance, jobs with a high share of computer use will teach the individual transferable skills of IT knowledge that could be applied in another job, if the current one becomes obsolete due to structural change. Since computation is such a central element of technological change, one outcome is whether individuals choose occupations with high computer use. I also look at whether the occupation is manual labour, as manual labour tends to be highly skill-specific. In a second step, this chapter analyses the impact of import exposure on later life labour market outcomes in terms of earnings, unemployment as well as occupational and regional mobility.

The results show that import exposure makes adolescents choose more skill-specific occupation groups in manufacturing and crafts, more import-intensive manufacturing industries in particular, and less general occupations in services and commerce (as merchants). This suggests that individuals do not shelter themselves from future import competition or automation because they do not enter occupations which impart general skills. This also implies that individuals do not adjust away from the predominant industry structure of the county they grew up in. The results also show that in terms of the task content of occupations, individuals exposed to more import competition are less likely to enter occupations with high computer use, and more likely to enter manual occupations.

Moreover, I find that individuals exposed to import competition in their adolescence who enter vocational education are adversely affected on the labour market in later life. They earn less 5 and 10 years after their apprenticeships and also experience less earnings growth. They are surprisingly more mobile in terms of occupational mobility, but less mobile regionally. At least, there is no negative effect on lifetime unemployment duration and they are more likely to be employed immediately following their apprenticeship. This result is in line with the finding from the general versus vocational education literature which shows that skill-specific education makes the transition from schooling to the labour market easier, but increases risk of unemployment in later life and leads to earnings losses (Hanushek et al., 2017*a*; Hampf and Woessmann, 2017). I do not find evidence that it increases unemployment later in life, but I do

[2] See Wolter and Ryan (2011) for a review on the existing literature on vocational education training.

[3] Keeping the level of education fixed at vocational education has the advantage that career paths are comparable and no potential income effects from trade shocks bias the results, since all vocational occupation trainings pay similar wages and are similar in length.

find losses in earnings instead. While the negative labour market outcomes cannot be directly causally linked to the vocational occupation choices due to biases caused by self-selection, suggestive evidence shows that general skill occupation groups shelter individuals from the adverse effects of import competition on earnings growth. The negative effects of import competition on earnings growth seem to be entirely driven by those entering manufacturing occupations.

An obvious channel as to why individuals still enter these occupations that affect them adversely are parents. I show suggestive evidence that while parental occupation is an important driver in adolescents' occupation choices (i.e. individuals enter the same occupation as their parents), having a father that worked in manufacturing and growing up in regions exposed to import competition, actually decreases the probability of individuals to enter a skill-specific manufacturing job. This suggests that potential first-hand negative experiences of job or income loss due to import competition within a family, may work to dissuade individuals from taking up skill-specific occupations.

In terms of threats to identification, I show that the effects are not biased by endogenous subsample sorting in the sense of differential sorting into different educational tracks due to trade shocks. Moreover, using data on local supply and demand ratios of apprenticeship positions, I confirm that the effects are not purely labour demand driven. The results are further robust to various alternative definitions of import exposure. As far as effect heterogeneities go, I find that men and women make very different choices when exposed to import competition. Women, as opposed to men, are more likely to enter service and merchant occupations when exposed to local import competition during adolescence. They also choose occupations with higher computer use. However, women are nevertheless still adversely affected by import competition in terms of later labour market outcomes.

The chapter is related to and contributes to several strands of literature: the literature on (1) general versus skill-specific education, (2) occupational skill-specificity, (3) the effect of business cycles on education choices and other outcomes, and of course (4) the impacts of trade shocks on individuals. It has been shown that general education, in the sense of higher education at university, versus skill-specific apprenticeship-based or vocational education, teaches more transferable skills that allow better adaptation to changing technologies, and therefore act as an insurance against later unemployment. Krueger and Kumar (2004) show that on a country level, economies that favour vocational education grow slower than countries that focus on general education, due to slower adaptations of new technologies, in particular when the pace of technological advancement increases. At the individual level, Hanushek et al. (2017*a*) and Hampf and Woessmann (2017) show that vocational education eases entry into the labour market for young individuals but increases the risk of unemployment in later life and also reduces lifetime income. This chapter contributes to the literature by providing a much more detailed approach to general versus skill-specific education than the dichotomy of vocational versus university education by looking at the skill-specificity across occupations.

I construct an occupational skill-specificity measure based on skill-weights using occupational skills and tasks from a German employment survey to categorize occupation groups by their specificity similar to Gathmann and Schönberg (2010); Geel, Mure and Backes-Gellner (2011). Eggenberger, Rinawi and Backes-Gellner (2018) using skill requirements in Swiss training curricula, find that there is a trade-off between higher wages in more specific occupations but lower occupational mobility and therefore higher risk of unemployment. To my knowledge, my study is the first to look at the impact of economic conditions on the skill-specificity of occupation choices.

Business cycle conditions have been found to affect individuals' schooling decisions and later life outcomes. Dellas and Koubi (2003) find that schooling decisions follow a countercyclical pattern, showing that reduced opportunity costs during recessions play a major role in education decisions of individuals, and Adamopoulou and Tanzi (2017) find that in recessions the likelihood of university students to drop out decreases and on-time graduation increases. Many studies have further shown that economic conditions at the point of labour market entry can have long and persistent effects on individual labour market outcomes. Studies by Kahn (2010) and Oreopoulos, von Wachter and Heisz (2012) show that college graduates in the United States experience income losses that persist for up to ten years when graduating during a recession. The predominant reason for this is the lower quality of the first job placement and skill-mismatch. Altonji, Kahn and Speer (2016) show that higher paying majors are sheltered from the negative effects of a recession. This chapter contributes to that literature because it investigates the effect of trade induced structural change (*i.e.,* a more permanent change in economic conditions) on individual occupation decisions and the effects on later life outcomes.

Lastly, this chapter contributes to the literature on trade shocks. In their seminal paper, Autor, Dorn and Hanson (2013) show that import exposure from China can explain large shares of the declines in manufacturing employment in local labour markets in the US. For Germany, Dauth, Findeisen and Suedekum (2014) looking at trade with both China and Eastern Europe, show that although net exports have actually retained employment within manufacturing because of increased export opportunities, local labour markets with high import competition have still seen decreasing employment in manufacturing and other industries. Import exposure has sped up structural change in German regions which specialized in import intensive industries and suffered clear employment losses (Dauth and Suedekum, 2016). Much of the literature on trade shocks has focused on the impact of regional aggregate employment patterns (Autor, Dorn and Hanson, 2013; Dauth, Findeisen and Suedekum, 2014, 2017), explaining manufacturing employment changes through Chinese (and Eastern European) import competition. Dauth and Suedekum (2016) show that import exposure, driven by large initial shares of import manufacturing industries, speeds up regional structural change, *i.e.,* the decline in manufacturing employment. Several papers look at the effects of trade shocks on individuals, namely on incumbent workers exposed to import competition at the level of their industry. Autor et al. (2014) find that workers more exposed to trade with China through

their industry of employment exhibit lower cumulative earnings and employment and higher receipt of disability insurance. In another paper at the individual incumbent worker level for Germany, Dauth, Findeisen and Suedekum (2018) show that imports reduce earnings and induce workers to leave their industry. In contrast, this study is the first to look at the effect of local labour market import shocks on individuals (not incumbent workers in manufacturing industries), focusing on local import exposure for young labour market entrants and their choice of vocational education occupation.

The rest of the chapter is structured as follows: Section 5.2 explains the institutional background of the vocational education training system in Germany and derives a conceptual framework with relation to the literature. Section 5.3 introduces the empirical set up, including the empirical identification strategy, the definitions of import exposure and occupational specificity and describes the data. Section 5.4 then presents and discusses the results in turn. Section 5.5 provides concluding remarks.

5.2 Conceptual Framework on Trade Shocks and Vocational Occupation Choices with Relation to the Literature

The aim of this chapter is to provide causal evidence on the effect of regional import competition exposure in adolescence on the vocational occupation an individual enters. It focuses on analysing which type of occupation the person enters, what the task content of that occupation is, and what the subsequent labour market outcomes are. This section derives a conceptual framework based on the existing literature on how structural change induced by import competition may affect vocational occupation choices and to which extent these may shelter individuals from structural unemployment. I begin by providing context to the institutional background of the German dual vocational education training system.

5.2.1 Institutional Background on the Vocational Education System in Germany

The German Vocational Education Training (VET) system, also sometimes called the dual system, is an important and firmly established part of the German education system. Its central feature is cooperation between mainly small and medium sized companies, on the one hand, and publicly funded vocational schools, on the other hand. Individuals in VET spend part of their time working as an apprentice at a company and the other part at a vocational school. It is up to the individual to apply to and find an apprenticeship position at a company. VET usually lasts two to three-and-a-half years. The cooperation, contracts and training curricula are regulated by the federal states. The German education system tracks its students from an early age into different school tracks that determine their further course of education and working life (Hanushek and Woessmann, 2006). Usually at the age of 10, at the end of primary school, students are divided into an academic track that will enable them to attend university if completed, and a lower or middle track, upon completion of which students normally go

on to vocational education. While the share of individuals in any cohort in VET used to be substantially higher, up to 70 percent during the 1970s, this share was still at 40 percent in 2016 (Statistisches Bundesamt, 2018*b*). There are over 320 different occupations that require vocational education, which range from manual and technological to service, merchants or public service related occupations. Apprentices consistently make up about 5 percent of the Germany social security based labour force. In 2016, 28 percent of firms in Germany offered apprenticeships and 68 percent of apprentices were offered a full employment contract at their training firm upon completion of VET (BIBB, 2016).

5.2.2 Vocational Occupation Choices

General versus Skill-Specific Education The central idea is that in the broad spectrum of 320 different occupations that individuals enter through VET, some occupations are more skill-specific and some more general in the skills they teach, so that they shelter individuals from structural unemployment caused by automation and trade because of transferable and widely applicable skills. In the literature, it has long been recognized that education is important because it enables adaptation to change, be it in terms of technological advancement, structural change or economic conditions (Nelson and Phelps, 1966). Hanushek et al. (2017*b*) show that returns to skills are larger in faster growing economies, lending evidence to the hypothesis that education enables better adaptation of skills to technologies. The literature further recognizes that general, rather than vocational education is better suited to reap the benefits of education in terms of better adaptation to change, because more general and therefore transferable skills are taught. By general education, the literature usually refers to tertiary education in the form of university education, while skill-, technology-, and occupational-specific education refers to vocational and apprenticeship based education (Ryan, 2003). Krueger and Kumar (2004) show that on a country level, economies that favour vocational education grow slower than countries that focus on general education, due to slower adaptations of new technologies, in particular when the pace of technological advancement increases. The authors offer this as potential explanation for differential growth rates between the United States (more general education) and European countries (more vocational focus), since the rate of technological advancement has picked up since the 1980s. At the individual level, Hanushek et al. (2017*a*) and Hampf and Woessmann (2017) show that vocational education eases entry into the labour market for young individuals but increases the risk of unemployment in later life and also reduces lifetime income. The reason is that while vocational education provides a more seamless transition from the apprenticeship into regular employment, it does not impart enough adaptive skills in case of unemployment later in life. With general education and transferable skills, the risk of unemployment is reduced because individuals are better able to adapt to new occupations and new tasks. In times of globalization, structural change and skill-biased technological change, this trade-off becomes particularly relevant, where whole occupations may cease to exist due to automation processes or are off-shored to other countries.

This chapter applies the same logic of general and skill-specific education horizontally within the vocational education system of Germany, namely through the vocational occupation choice of individuals. While there has been much attention in research on college enrolment and returns to college degrees (for a review see e.g. Oreopoulos and Petronijevic, 2013), or major choices (e.g. Altonji, Arcidiacono and Maurel, 2016; Card and Payne, 2017) and major-specific returns (Kirkeboen, Leuven and Mogstad, 2016; Hastings, Neilson and Zimmerman, 2013), very little attention has been paid to outcomes within vocational education training. Holding a vocational education degree versus not having a professional degree at all is associated with a premium of about 15 percent (Kugler, Piopiunik and Woessmann, 2017). However, very little is known about the different occupations within vocational education, who chooses them and why and which effects they have on individuals' labour market outcomes. Determinants of education and occupational choices are naturally very difficult to pin down, due to the highly personal, multidimensional and therefore endogenous nature of this choice. Causal evidence on the returns to higher versus vocational education would be hard to obtain, due to issues of selection into either higher general education or vocational training, which as in Germany, is often determined by choices at much earlier points in life (Ryan and Unwin, 2001). This chapter investigates individuals' vocational education and career paths, holding the level of education fixed at the level of vocational education training. I argue, that just as with general versus vocational education, there are differences in the generality and skill-, technology- and occupational specificity *within* vocational educations. With these differences, the same argument in terms of adaptation to change, in particular skill-biased technological change holds. For example, while vocational training in manufacturing occupations will offer production-technology specific skills, vocational training in more business or service oriented jobs will offer more transferable skills such as communication or quantitative skills, that are broadly applicable in other occupations. Looking only at vocational education further has the advantage that individuals' level of education is kept fixed and career paths are comparable. The choice of higher versus vocational education would introduce an income question, and trade shocks per se may impact family incomes and therefore distort the effects. In the robustness checks, I investigate whether individuals select intro different education tracks which may lead to higher education differentially, but do not find any effects.

Occupational Specificity The differentiation between general and specific education along the lines of tertiary university versus vocational education, is very simplistic. Instead, this chapter distinguishes skill-specificity between occupations that require VET. Some occupations teach more general skills that are transferable and can be applied in other occupations or other industries. Such skill transferability is particularly important in times of structural change, in which with trade and automation, many occupations are either off-shored or cease to exist due to automation altogether. Lazear (2009) provides a useful framework for occupational specificity, called skill-weights approach, which assumes that occupations use different skills with different weights attached, so called skill bundles. The skill bundles of occupations have different distances to the skill bundle of the labour market on average. The further away

a skill bundle is from the average of the labour market, the more specific that occupation is and the more costly it is for a person with such skills to change occupations. Skill bundles that are similar to the labour market on average, mean that people in such occupations should find it easy to switch occupation. Geel, Mure and Backes-Gellner (2011) operationalise the skill-weights approach for German occupation using skills from a German employment survey and construct a measure for occupational specificity using total absolute rank differences in skills in an occupation compared to the labour market on average. They find that the more specific an occupation is, the higher the apprentice training cost for the firm and the lower the occupational mobility. Gathmann and Schönberg (2010) use tasks rather than skills in the same employment survey, and they construct an angular distance measure of task-specific human capital. Eggenberger, Rinawi and Backes-Gellner (2018) look at skill requirements in training curricula for Switzerland. They find a clear trade-off between higher wages in more specific occupations but lower occupational mobility and therefore higher risk of unemployment. I construct a measure for occupational specificity using information on occupational skills and tasks in order to rank occupational groups by their skill-specificity and show that manufacturing and craft occupations are more specific than service or merchant occupations.

5.2.3 Impact of Economic Conditions on Young Individuals

We know from the literature that economic circumstances during childhood and adolescence impact individual education paths and later life outcomes. An extensive literature investigates the impact of the business cycle on individuals' behaviour and choices and how these effect lifetime outcomes. Because of the short-term character of booms and recessions, meaning that normally the economy will again return to its previous state, it would be optimal for individuals to not change their behaviour Nevertheless, there is ample evidence, that being exposed to a recession during youth, changes individuals' beliefs, education choices and later economic outcomes. In terms of beliefs, Giuliano and Spilimbergo (2014) find that individuals who experienced a recession when young believe that success in life depends more on luck than effort, support more government redistribution, and tend to vote for left-wing parties. Malmendier and Nagel (2011) find that recessions during adolescence lead to lower participation in the stock market, lower willingness to take financial risk and lower investments as well as increased pessimism about future stock returns. As far as the impact on education choices go, there is evidence that schooling decisions follow a countercyclical pattern because of reduced opportunity costs of education. When the economy is in a downturn and good job opportunities become scarce, going to school is less costly in terms of forgone earnings. This effect seems to outweigh the income effect of a recession (Dellas and Koubi, 2003). From this follows that enrolment to college increases with economic downturns (Betts and McFarland, 1995) and the likelihood of drop-outs decrease (Adamopoulou and Tanzi, 2017). College students also tend to choose high return majors such as STEM fields more often rather than business related studies when the economic situation is brittle (Liu, Sun and Winters, 2017; Blom, Cadena and Keys, 2015). Nagler, Piopiunik and West (2015) show that teacher quality increases during recessions, as labour market entrants face less certain outside

options vis-à-vis the relatively safe teaching profession. Despite higher college enrolment rates, lifetime labour market incomes are actually shown to be adversely affected by recessions. Graduating from high-school, college or graduate school during a recession can have long-lasting detrimental effects on lifetime earnings and other outcomes (Raaum and Røed, 2006; Oreopoulos, von Wachter and Heisz, 2012; Kahn, 2010; Oyer, 2006), predominantly caused by a lower quality of first job placement. Also cyclical skill-mismatch has been found as a reason for adverse effects of graduating during a recession (Liu, Salvanes and Sørensen, 2012). On the other hand, Altonji, Kahn and Speer (2016) show that higher paying majors are sheltered from the negative effects of a recession. This chapter contributes to the literature in that it looks at the effect of a more permanent change in economic conditions on education choices and later labour market outcomes, namely structural change. If the business cycle impacts individuals' choices, then a more permanent, though slower change is expected to also have an impact.

5.2.4 Structural Change and Trade Shocks

Structural change Structural change, *i.e.,* the slow change from a predominantly manufacturing to service based economy, started in Germany like in most Western countries in the 1970s. It can be said to be driven by both automation and trade, in that routine tasks become automated and goods get produced where it is cheapest to do so. Structural unemployment is then caused by a permanent mismatch between the skills of the workforce and the changing mode of production in the economy, because the demand for skills changes. Automation of production processes implies that routine tasks, which are programmable, become increasingly redundant while more computational, communication and other generally transferable skills are more sought after (e.g. Autor, Levy and Murnane, 2003; Autor, Katz and Krueger, 1998). This phenomenon is referred to as skill-biased technological change. Autor, Levy and Murnane (2003) argue that as computers take over routine tasks, they increase demand for workers who perform 'non-routine' tasks that are complementary to the automated processes. Since computation is such a central element of technological change, this chapter also tests whether individuals choose occupations with high computer use. Computer use in occupations comes with many general skills, that are transferable to other occupations, such as programming, communication or analysis. Spitz-Oener (2006) finds that occupations with high computer use see most pronounced changes in skill requirements. The author generally shows that task complexity within occupations has increased since the nineteen-eighties and the need for manual routine tasks has plummeted in Germany.

Trade shocks Other than automation, international trade is often said to be the other important factor driving structural change. Between 1993 and 2013, Germany has seen an over 20 percent decline in manufacturing employment (e.g. Dauth, Findeisen and Suedekum, 2017). In the literature, the unexpected and rapid rise in Chinese productivity and the associated rise in trade with China and the rest of the world, has been keenly exploited as an exogenous shock

in global trade. In their seminal paper, Autor, Dorn and Hanson (2013) analyse the impact of Chinese import competition on US local labour markets and show that trade with China explains one third of the decline in manufacturing in local labour markets. Identification stems from differential initial industry structures across local labour markets. They also instrument trade volumes between the US and China with trade volumes between China and other high income countries. They find that local markets more exposed to Chinese import competition see higher unemployment, lower labour force participation, and reduced wages. In another paper, Autor et al. (2014) analyse the impact of industry trade exposure on labour market outcomes of incumbent workers. They find that workers more exposed to trade with China through their industry of employment exhibit lower cumulative earnings and employment and higher receipt of disability insurance.

In Germany, import competition from China has also led to decreases in manufacturing employment. Dauth, Findeisen and Suedekum (2014) estimate the local labour market effects of trade with "the East" for Germany. They not only use trade with China, but also investigate trade with Eastern Europe, which for Germany constituted another unexpected and sharp rise in trade after the fall of the iron curtain. The results for Germany differ substantially from those of the US, since Germany has a total current account surplus and more balanced trade with China and Eastern Europe than the US. They find that net export exposure (exports minus imports) has actually slowed down the decline in manufacturing employment and regions specialized in export industries have seen increases in employment. Nevertheless, they also find that regions specialized in import competing industries have seen substantial employment losses both in manufacturing and in other industries. They find that a 10-year change in local import exposure of 1000 Euro per worker reduced manufacturing employment relative to overall employment by 0.19 percentage points. In another paper, Dauth and Suedekum (2016) show that large import exposure, driven by large initial shares of import manufacturing, sped up structural change, i.e the decline in manufacturing employment. This finding represents the implicit first stage to the reduced form regression of import competition on occupation choices in this chapter.

Generally, manufacturing employment has been on a secular decline in Germany. Dauth, Findeisen and Suedekum (2017) show that the transition from manufacturing to services is fuelled by new labour market entrants as well as unemployed who re-enter the labour market. They show that net exports pulled labour market entrants more into manufacturing and into export-oriented firms more generally. There is no direct switching between employment in manufacturing and service without first undergoing unemployment. These estimates are based on county aggregates. In another paper at the individual incumbent worker level, Dauth, Findeisen and Suedekum (2018) find that imports reduce earnings and induces workers to leave their industry. This chapter contributes to the literature, as it analyses the impact of imports competition on individuals, focusing on regional import exposure for young labour market entrants and their choice of vocational education occupation.

5.3 Empirical Set-Up

This section describes the empirical model and identification strategy, how trade shocks and occupational specificity are constructed, the various data sources used, and provides a descriptive overview of the data.

5.3.1 Empirical Model

An individual growing up in a county exposed to stronger imports during his adolescence, experiencing unemployment in manufacturing, lay-offs or plant closures, may choose a different vocational occupation than otherwise. The regression analysis is a reduced form regression of local import exposure on education choices, with local structural change (reductions in manufacturing employment) being the conceptual first stage or channel driving the results. The regression estimation is therefore:

$$y_i^{r,t} = \beta_1 \Delta ImportExposure_{rt}^{Ger \leftarrow C+EE} + \beta_2 \Delta ExportExposure_{rt}^{Ger \rightarrow C+EE} + \beta_3 X_i + \beta_4 X_r + \gamma_{t*s} + \epsilon_i \quad (5.1)$$

where $y_i^{r,t}$ is individual i's outcome in terms of vocational occupation type, task content or later labour market outcomes (which depends on county r and year t but does not vary by them), $\Delta ImportExposure_{rt}$ is the import exposure per worker in county r and year t when individual i is 15, $\Delta ExportExposure_{rt}$ is export exposure per worker in county r and year$t$ when individual i is 15, X_i are individual controls, X_r are county controls, γ_{t*s} are year by state dummies, and ϵ_i is an error term. Import and export exposures vary per year on the level of 402 counties. Standard errors are clustered at the county level. The model is estimated using OLS linear probability models for most outcomes which are binary[4]. β_1 therefore denotes our coefficient of interest, namely the increase in the probability of an individual having outcome y, when import exposure increases by 1000 Euro per worker in county r at year t when individual i is 15.

5.3.2 Identification

The increases in trade with China and Eastern Europe can be said to have been exogenous from the point of view of German regions, as they stemmed from the respective domestic increases in productivity and competitiveness. China's accession to the WTO in 2001 and the fall of the iron curtain in Eastern Europe and its subsequent transition to democracy and market oriented economies in 1990, were mostly exogenous supply shocks to the world economy.

[4] Only the variables age at apprenticeship start and income are not binary

From the point of view of a German region with high shares of employment in an industry in which China or Eastern Europe started having a comparative advantage in and therefore started exporting to Germany, import competition is as good as random. Identification comes from different initial industry specializations across counties. Take the example of two counties with similar shares of employment in manufacturing in 1980. One county has high shares of employment in the textile industry, while the other specialists in the automotive industry. The county with high textile specialization experiences a strong import shock from China, as textile is one of the industries China has become competitive in. This county may experience closures of textile manufacturing plants and mass lay-offs, with subsequently high shares of unemployment. The other county with the automotive industry on the other hand, actually benefits from trade with China due to increasing export opportunities, as Germany retained the competitive advantage in the car industry. The local trade shocks are constructed by using a shift-share measure (Bartik, 1991), where national industry trade is apportioned by initial local industry employment structures (explained in detail in Section 5.3.3).

Nevertheless, a major concern with trade shocks is that employment and imports may be positively correlated with unobserved shocks to domestic product demand, in which case the effect on manufacturing employment, *i.e.,* the implicit first stage to the reduced form regression in Equation 5.1 would be underestimated. This is particularly problematic here, as the choice of vocational education occupation is of central interest, and push and pull factors stemming from labour demand should be shut down as much as possible. To isolate the supply-driven component of imports and exports from China and Eastern Europe, I use imports and exports to and from other high income countries with China and Eastern Europe (following e.g. Autor, Dorn and Hanson, 2013; Dauth, Findeisen and Suedekum, 2014). For the exclusion restriction to hold, it is important that while those third group countries should be similar to Germany, they should not be exposed to the same demand shocks as Germany nor should their trade flows with China and Eastern Europe affect counties in Germany other than through exogenous increases in imports. This is why no Euro area country or immediate neighbouring country is included. I use trade with Australia, Canada, Japan, Norway, New Zealand, Sweden, Singapore and the UK as instruments for trade with Germany, as in Dauth, Findeisen and Suedekum (2014). This way, the exogenous part of increased Chinese and Eastern European competitiveness is extracted, shutting down factors that affect imports and regions at the same time, such as demand shocks. Trade flows of these third group high income countries with China and Eastern Europe are therefore used to instrument trade flows of Germany with China and Eastern Europe. I run the following two-stage least squares regressions, to instrument both import and export exposure.
First stages:

$$\begin{aligned}\Delta InstImportExposure_{rt}^{Ger \leftarrow C+EE} = \zeta_1 \Delta ImportExposure_{rt}^{Other \leftarrow C+EE} + \\ \zeta_2 \Delta ExportExposure_{rt}^{Ger \rightarrow C+EE} + \zeta_3 X_i + \zeta_4 X_r + \gamma_{t*s} + v_i\end{aligned} \tag{5.2}$$

$$\Delta InstExportExposure_{rt}^{Ger \rightarrow C+EE} = \zeta_1 \Delta ImportExposure_{rt}^{Ger \leftarrow C+EE} + \zeta_2 \Delta ExportExposure_{rt}^{Other \rightarrow C+EE} + \zeta_3 X_i + \zeta_4 X_r + \gamma_{t*s} + v_i \quad (5.3)$$

Where $\Delta InstImportExposure_{rt}^{Ger \leftarrow C+EE}$ and $\Delta InstExportExposure_{rt}^{Ger \rightarrow C+EE}$ represent the instrumented import and export exposure, respectively and $\Delta ImportExposure_{rt}^{Other \leftarrow C+EE}$ and $\Delta ExportExposure_{rt}^{Other \rightarrow C+EE}$ denote the respective trade flow of China and Eastern Europe to and from the third group high income countries. The second stage can be written as:

$$y_i^{r,t} = \beta_1 \Delta InstImportExposure_{rt}^{Ger \leftarrow C+EE} + \beta_2 \Delta InstExportExposure_{rt}^{Ger \rightarrow C+EE} + \beta_3 X_i + \beta_4 X_r + \gamma_{t*s} + \epsilon_i \quad (5.4)$$

For the estimation to produce the true, unbiased causal effect of growing up in a county with high exposure to import competition on the vocational occupation choice and the subsequent career path, other than the instruments being valid, several further identifying assumptions need to hold. First, it needs to hold that counties developed along similar trends in terms of apprenticeships before import exposure. One wants to make sure, that regions more exposed to imports later were not on a differential trend in terms of vocational education initially anyway, since the effect of import exposure on vocational occupation would then capture other underlying factors that had nothing to do with import competition. Figure 5.1 plots shares of apprentices over the whole local working population for different quantiles of import exposure in 2000. As the "treatment" of import exposure is continuous, *i.e.,* represents a treatment intensity, counties are split into five quantiles of 20 percent along their 2000 import exposure from both China and Eastern Europe. In Figure 5.2 one can see, that imports from China and Eastern Europe are mostly flat until 1990, therefore one would not want to see differences in trends of apprenticeship numbers in counties. Unsurprisingly, there exist differences in levels, since import exposure is based on the initial industry structures (explained in detail below in 5.3.3), meaning that counties with high initial shares of manufacturing are later more exposed to import competition and also tend to have higher shares of apprentices among their working population. However, Figures 5.1a and 5.1b show no evidence that counties saw differential trends in (a) total shares of apprentices, nor in (b) manufacturing apprentices. For numbers of manufacturing apprentices, the quantiles move further apart, but there are no large differences in general trends.

Another assumption that needs to hold, is that there are no large adjustments via interregional migration due to trade shocks. In the analysis, individuals are fixed to a county and assumed to be "treated" by their county trade shock according to the first county in which they ever appear in the social security records. For the majority of individuals, this is the year they start their apprenticeship. These individuals are "treated" by the trade shock in their county in the year

they are 15 years old, *i.e.,* their year of birth plus 15 years. Therefore, an important assumption of the analysis is that individuals do not change counties for their apprenticeships and more importantly, that this does not happen differentially due to trade shocks. This also implies that there is no differential migration of the families due to trade shocks. First one should note that apprenticeships are an inherently local market and mobility for apprentices in Germany is very low (Stockinger and Zwick, 2017). Furthermore, interregional adjustments through migration are generally sluggish in Germany. For example, the rate of German interregional migration was at only at 1.2 percent in 1995 (Tatsiramos, 2009). Lastly, Dauth, Findeisen and Suedekum (2014) find no effects of import and export exposure on population shifts, showing that regional adjustment in Germany does not pose a major concern.

5.3.3 Local Labour Market Import Exposure

Import exposure, as used in Equation 5.1 is defined as follows:

$$\Delta ImportExposure_{rt} = \sum_{j} \frac{L_{rjt-10}}{L_{jt-10}} \frac{\Delta Imp_{jt}^{GER \leftarrow C+EE}}{L_{rt-10}} \tag{5.5}$$

where $\Delta ImportExposure_{rt}$ can be thought of as the change in per worker imports in 1000 Euro in county r at time t. L_{rjt-10} is employment in county r, industry j at time $t-10$, L_{jt-10} is employment in industry j at time $t-10$ nationally and L_{rt-10} is employment in county r at time $t-10$. $\Delta Imp_{jt}^{C+EE \leftarrow GER}$ are changes in import volumes between t and $t-10$ in industry j from China and Eastern Europe."Eastern Europe" includes Bulgaria, Czech Republic, Hungary, Poland, Romania, Slovakia, Slovenia and all countries of the former USSR[5]. The total 10-year change in industry j's imports from China and Eastern Europe to Germany is apportioned to county r according to county r's share in national industry j employment. The measure closely follows Dauth, Findeisen and Suedekum (2014), but extends their measure by introducing yearly varying import exposures with 10-year rolling window changes. I use the initial industry structure at the beginning of each period $(t-10)$. For example, trade exposure in 2000 refers to the change in import between 1990 and 2000 using the 1990 industry structure to apportion national industry trade volumes. In the robustness checks, I define alternative industry baselines, such as fixing them at 1990 (1993 for Eastern Germany) for all years as well as lagging them by 10 years. The results are robust to using either. Export exposure per worker is defined along the same lines, using export volumes from Germany to China and Eastern Europe.

Figure 5.3 shows import exposure for each region in 1990, 2000, 2003 and 2014 from import exposure from both China and Eastern Europe. With the availability of regional industry data, 2003 (2004 for instruments) import exposure for formerly German Democratic Republic

[5] Russia, Belarus, Estonia, Latvia, Lithuania, Moldova, Ukraine, Azerbaijan, Georgia, Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, Uzbekistan.

(GDR) counties is the earliest year possible. I analyse Eastern European and Chinese import shocks jointly in most analyses[6]. The maps show that there is considerable variation in import exposure both across regions and time. Average import exposure per worker increases from 840 Euro 1990 (*i.e.,* the increase between 1980 and 1990), to 5814 Euro in 2000 and 7082 Euro in 2007, where it peaks and then slowly ebbs down (which is natural, seeing as these are changes, the level is steadily increasing as we saw in Figure 5.3.) The 10-year rolling window changes used in this chapter exploit the fact that trade volumes have increased steadily and smoothly since the 1990s, while the other papers in the literature usually only look at one or two different time intervals.

The instrumental variable of third group high income country import exposure used in Equation 5.2 is constructed in the same manner as import exposure in 5.5:

$$\Delta InstImportExposure_{rt} = \sum_{j} \frac{L_{rjt-11}}{L_{jt-11}} \frac{\Delta Imp_{jt-11}^{\sum Other \leftarrow C+EE}}{L_{rt-11}} \tag{5.6}$$

where now $\Delta Imp_{jt-11}^{\sum Other \leftarrow C+EE}$ are imports from China and Eastern Europe to other high income countries. For the instrument import and export exposure lagged industry employment shares are used, at $t-11$ (for change in trade between t and $t-10$) to limit potential reverse causality in terms of employment due to anticipation in future trade exposure. Again, export exposure is defined analogously.

5.3.4 Occupational Specificity Measure

To establish whether an occupation is general or specific, I construct a skill-specificity measure based on various elements from Eggenberger, Rinawi and Backes-Gellner (2018); Gathmann and Schönberg (2010); Geel, Mure and Backes-Gellner (2011). The measure leans on Lazear (2009)'s skill-weights approach, which assumes that occupations use different skills with different weights attached (skill bundles) that makes them more or less general. Using information from a German employment survey on required skills and tasks performed in the individuals' occupation, I construct a specificity measure for each occupation, by comparing skill-weights in every occupation with skill-weights for the labour market on average. Table A5.1 reports the tasks and skills covered in the survey. Both tasks and skills are combined, in order to use more available information and because skills and tasks are highly complementary.[7] The

[6] For the main regression I do also analyse Eastern European and Chinese import shocks separately

[7] Despite Acemoglu and Autor (2011)'s distinction between *tasks* and *skills* in that a *skill* is a unit of work activity that produces output, while a *skill* is a workers endowment of capabilities, in this chapter I treat them as interchangeable. You cannot perform a task without having the skill and stating that one requires a skill for the daily job, implies that one performs the task attached to the skill. The way in which the survey is asked, the task requirements and skills hardly overlap, as one focuses more on operational tasks and the other on conceptional skills. Using both provides broader information about each occupation.

occupational specificity measure is a skill-distance measure in that a higher skill distance implies a lower overlap in the skill bundle from one occupation to the general labour market skill bundle. In a first step, the 31 tasks and skills are aggregated at the occupation level, which provides the skill-weights for each occupation. These are normalized to sum to one dividing by their sum, in order to take out skill-level effects. Then, tasks and skills are aggregated for the labour market on average, using the survey weights to ensure skills weights are aggregated to be representative of the labour market. I then construct an angular distance measure, similar to Gathmann and Schönberg (2010); Eggenberger, Rinawi and Backes-Gellner (2018):

$$SpecDist_{jl} = 1 - \frac{\sum_{i=1}^{n} x_{ji} * x_{li}}{\sqrt{\sum_{i=1}^{n} x_{ji}^2 * \sum_{i=1}^{n} x_{li}^2}}$$

where i is a skill, x is the skill weight of skill i in occupation j and l denotes the general labour market. To obtain skill distance rather than similarity of skill bundles, the angular distance is reversed by subtracting it from one. The larger the skill distance, the more specific and specialized an occupation is and therefore the lower the transferability of skills to another occupation. While highly specialized occupations may come with a wage premium, they are also riskier, because if an individual becomes unemployed who was in a highly specific occupation, he or she will find it more difficult to find a new occupation to which the specific skill bundle can be applied.

Figure 5.4 ranks the four main outcome occupation groups by their skill specificity. Manufacturing occupations are the most specific, followed by craftsmen. Service occupations and merchants are less specific, meaning that the skills in those occupations are closer to those of the labour market on average.

5.3.5 Data Sources

Various high quality data sources are required to implement this empirical analysis, including individual administrative data, administrative firm data, data on trade flows, as well as information on skills and tasks within occupations, among other data sources.

Individual Social Security Data The main data on individual vocational occupations and career paths stem from the German Social Security system. The Sample of Integrated Labour Market Biographies (SIAB)[8] is a representative two-percent random sample drawn from the population of individuals subject to social security (*i.e.,* employed, officially job seeking etc.; it excludes self-employed and civil servants) in Germany, assembled by the Institute for

[8] This study uses the weakly anonymous Sample of Integrated Labour Market Biographies (Years 1975-2014). Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

Employment Research (IAB) (Antoni, Ganzer and vom Berge, 2016). The data provide detailed administrative information, on individuals' occupations, employment status, earnings and importantly on where a person works and lives on the county level. Since the apprenticeship in the vocational education system is subject to social security, I observe individuals in this educational track at the start of their labour market career. I assume that the county I first observe them in, is the county the individuals grew up in, since apprenticeship markets are highly localized and individuals rarely leave their parental home for their apprenticeship. Each individual is kept only once, in order to observe the type occupation the individual first enters for his or her apprenticeship. Further life labour market outcomes are reported as cumulations. Individuals are included when they are aged 15 between 1990 and 2014 in western counties, and between 2003 and 2014 for eastern counties. This is because ten years earlier are the earliest years for which I can observe initial industry structures in the counties to construct the trade shocks. The person is "treated" at the county where she is first observed in the data for the year she is 15 years old. I choose age 15, because it is the year before individuals usually enter vocational education when they finish the middle track school. Here, the person has observed changed imports over 10 years since the age of 5, and has been "treated" by the import shock in the sense that she has been exposed to the structural change and declining employment in manufacturing in her home county. She has witnessed increased levels of structural unemployment in her local labour market, perhaps even of her parents or friends (note that unfortunately, I have no information on family ties in the data).

Administrative Firm Data To calculate yearly county-level per worker trade shocks by apportioning the national industry trade shock to the local employment share of that industry, detailed county-level data on employment in each industry is required. I use the Establishment History Panel (BHP) (Schmucker et al., 2016)[9], which is a 50 percent sample of all firms in Germany, providing yearly information on the number of employees, industry classification and county of operation. I use the years 1980-2014 for Western Germany and 1993-2014 for Eastern Germany.[10]

Trade Volumes I use trade volumes from the United Nations' Comtrade database, which provides extensive information on bilateral trade volumes, following Autor, Dorn and Hanson (2013) and Dauth, Findeisen and Suedekum (2014). Trade volumes between Germany and China, Germany and Eastern Europe, as well as between China and Eastern Europe and the eight high income countries for the instrument are being used. Product-level (SITC Rev.3) Comtrade trade volumes are mapped into 3-digit German Industry Classifications, version

[9] This study uses the weakly anonymous Establishment History Panel (Years 1975 2014). Data access was provided via on site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and remote data access.
[10] 1993 is the first year for which data for Eastern Germany was reliably recorded post reunification.

93 (Federal Statistical Office, 2003) using a crosswalk[11]. I identify 93 manufacturing sectors, dropping industries related to fuel, oil and gas. Trade volumes are converted into 2010 Euro This data is then aggregated to yearly (1980-2014) import and export volumes for each manufacturing industry. It is then merged to the administrative firm data, for the trade shock to be apportioned by regional industry employment shares.

Data on Computer Use, Skills and Tasks To classify the extent of computer use within occupation, I use four waves of a survey lead by the Federal Institute for Vocational Education and Training (BIBB) on individuals' employment careers and occupation (the BIBB/IAB Qualification and Occupational Career Surveys 1992 and 1999 and BIBB/BAuA Employment Survey 2006, 2012). I aggregate the question on whether working with a computer is a daily task on the job on the level of 3-digit Occupation Classification 1993, and merge the occupation averages to the occupations in the SIAB social security data. For the skill specificity measure, I further use the BIBB/IAB Qualification and Occupational Career Survey 1999 wave, which provides extensive details on the tasks and skills required in an individual's occupation. I use the 1999 survey wave because it inquires on a larger set of skills and tasks compared to other waves, and because it represents a central year of when occupational decisions are taken in my sample. Unfortunately, the survey waves are rather inconsistent in the tasks and skills inquired over time, which makes comparing occupational specificity difficult across years.

Miscellaneous Data Sources Further, numbers of students and graduates stem from the Regional Statistical Data Catalogue of the Federal Statistical Office and the statistical offices of the Länder. Lastly, information on regional supply and demand ratios stem from the Federal Institute for Vocational Education and Training. This data is collected at the job centre level, a labour market region which is comprised of 2 to 4 counties. The data is available on occupational level only between 2004 to 2011. Total supply-demand apprenticeship ratios are available from 1998 to 2011. I also use data from the German Socio-Economic Panel to look at the impact of parental occupations[12].

5.3.6 Descriptive Overview

Figure 5.2 plots total import volumes from China and Eastern Europe over time, showing large, but fairly smooth increases over the past two decades. It illustrates why having yearly 10-year rolling window changes in import exposure as treatment provides added value compared to using only one or two non-overlapping 10-year increases. Table 5.1 reports summary statistics for all variables; import and export exposures, individual and regional characteristics as well as all outcomes. Average import exposure over the entire time span is 4180 Euro 46 percent of the sample is female and 92 percent are German citizens. Table **??** shows that the states

[11] The crosswalk was kindly provided by Wolfgang Dauth
[12] Socio-Economic Panel (SOEP), data for years 1984-2013, version 30, SOEP, 2013, doi:10.5684/soep.v30.

in which I observe the individuals at age 15 are distributed as one would expect in terms of general populations and given the fact that Eastern German states are only included as of 2003. The years in which individuals are "treated" by import exposure at the age of 15 range from 1991 to 2013, with again quite an even distribution and the early 2000s being represented the most.

Outcomes The chapter looks at a number of different outcomes, to describe which kind of vocational training occupations individuals exposed to import competition enter, and what their labour market outcomes are. The outcomes can be classified into three categories: (1) occupational groups, describing the type of occupation, (2) occupational task characteristics, describing what tasks the job entails, and (3) labour market outcomes, describing how individuals do during and after their vocational education. The occupational groups are dummy variables for whether an occupation is in manufacturing, represents a craft occupation, is in services, or is a merchant occupation.[13] Import-intensive manufacturing industries are identified as a manufacturing industry exposed to imports above the median. 35.5 percent of individuals in the sample are in manufacturing, 24.6 percent in craft occupations, 57.1 percent in service occupations, and 18.2 percent in merchant occupations. These occupations can be ordered according to their occupational specificity, as shown in Figure 5.4. The specificity measure ranges between 0 and 1, with one being highly specific, meaning that there is no overlap in the skill bundles of that occupation and the general labour market, and 0 meaning the occupation is very general, with perfect overlapping skill bundles. Average skill specificity in the sample is 0.118; manufacturing has a skill specificity of 0.20, craft occupation of 0.165, service of 0.136 and craft occupations of 0.129. Manufacturing and craft occupations being the more skill-specific, provide less potential for switching occupations, because the skills in these occupations are not as transferable. Therefore, they provide less sheltering from the forces of structural change, such as trade and automation. Note that these occupational groups do not sum to 1, as they are not mutually exclusive. While manufacturing and services are mutually exclusive, crafts and merchant occupations are subsets of both these groups. 12.8 percent of individuals work in import manufacturing industries.

Considering what is known about increasing skill requirements and task-complexity within occupations (Spitz-Oener, 2006), I further look at what the chosen occupations entail in terms of tasks. First, the extent of computer use in an occupation from the survey of the

[13] Manufacturing and Service occupations are identified based on occupational grouping of the German Classification of Occupations 1988 (Federal Employment Agency, 1998). Crafts occupations are identified based on the German Trade and Crafts Code, using the same procedure as in Lergetporer, Ruhose and Simon (2018). Merchant occupations are identified using the official classification of vocational training occupations of the Federal Institute for Vocational Education and Training.

Federal Institute for Vocational Education and Training is used[14]. This measure changes within occupations over time. 42 percent of occupations have frequent computer use. Further, I use Blossfeld (1987) to classify whether an occupation is "manual" (24 percent on average), "easy manual' (3 percent on average) or "qualified manual" (21 percent on average).

Finally, the following labour market outcomes are considered: the individuals' earnings during the apprenticeship (24 Euro gross daily earnings on average), what earnings are one, five and ten years after finishing the apprenticeship (24.58, 50.83, 70.62 Euro gross daily earnings on average, respectively) as well as the earnings growth rates over five (127 percent) and ten years (162 percent). Moreover, I also look at the age at which an individual starts the apprenticeship (19 on average), whether the person is employed the first year after the apprenticeship (63 percent), whether the person starts working in a different county after the apprenticeship (29 percent), how many occupational switches the person performs in their career (2.6 times on average), how many years the person is unemployed (0.77) and how many times she moves counties (1.25 times on average).

5.4 Impacts of Import Exposure on Vocational Occupation Choice and Labour Market Outcomes

This section presents and discusses results from the main regression on the three groups of outcomes: (1) occupational type, (2) occupational tasks and (3) labour market outcomes. It then checks for threats to identification in terms of endogenous sample selection and labour demand. Further, results from robustness checks and heterogeneous effects across gender are reported.

5.4.1 Skill-Specific Versus General Skills Occupation Type

Table 5.2 reports the effects of local labour market import exposure on the choice of vocational occupation type. All regressions control for export exposure and include individual and regional controls as well as state by year fixed effects, meaning that the effects are identified within year and state. Panel A reports OLS results, panel B reports 2SLS IV results for the same outcomes. The coefficients can be interpreted as the effect of a 1000 Euro increase in per worker import exposure on the likelihood of the outcomes in question. Column 1 shows that higher import exposure increases the likelihood of an individual to enter a manufacturing VET occupation. OLS gives a coefficient of 0.14 percent increase per 1000 Euro per worker increase

[14] The computer use on the job variable is taken from the occupation averages of four waves of the BIBB/IAB Qualification and Occupational Career Survey (1992, 1999) and BIBB/BAuA Employment Survey (2006, 2012). The averages for the survey year are used for occupations in the SIAB data for five years surrounding the survey year, such that the averages from 1992 are applied to occupations in the years 1990-1995, from 1999 to 1996-2001, from 2006 to 2003-2008 and from 2012 to 2009-2014. The results are not sensitive to changing around how these years are attributed.

in import exposure and the 2SLS IV coefficient gives a larger coefficient of 0.23 percent likelihood increase. Column 2 reports the coefficients on the likelihood of an individual entering a craftsmen VET occupation. The coefficients are positive, and the 2SLS coefficient implies a 0.19 percent increase in the likelihood of entering a craft occupation for a 1000 Euro per worker increase in import exposure. Columns 3 and 4 report the effects on entering a service and merchant occupation. Contrary to the previous columns, these coefficients are negative, implying a reduction in the likelihood of entering a service or merchant occupation. Column 5 finally shows effect of import exposure on entering an import manufacturing industry, *i.e.,* one of the industries that cause the trade shock. Perhaps unsurprisingly but disappointingly, adolescents enter more import-intensive industries that are prevalent in their labour market. This implies that they expose themselves to even more import competition in the future, and may be subject to uncertain employment prospects.

Since outcomes in Columns 1-4 of Table 5.2 are ordered from left to right by their occupational skill specificity, it becomes quickly evident that the effect of import exposure induces individuals to enter more specific occupation groups (manufacturing and crafts) and less the more general occupation groups (service and merchant). Additionally, individuals choose to work more in the manufacturing industries, that will expose them even more to import competition and therefore risk of future unemployment. The results imply that despite import competition and resulting local structural unemployment as shown in Dauth, Findeisen and Suedekum (2014), (1) individuals go more into occupations that are threatened by import competition and (2) into occupations that do not provide easily transferable skill bundles to facilitate occupational mobility in case of future unemployment. I control for manufacturing employment per county, which means that the effects are not purely reflecting the fact that individuals enter whatever industry structure is prevalent in their county. With these vocational education occupations, individuals expose themselves even more to imports in the future instead of sheltering themselves from it, which may have detrimental effects on their later labour market outcomes.

In terms of the magnitude of the effects, the average import exposure per worker is 4180 Euro, which means the marginal effect given by the coefficients should be multiplied by 4.18 to get the average effect. In the case of manufacturing, the likelihood was increased on average by (4.18*0.23 percent $\approx$) 1 percent. The difference between counties at the 75th and 25th percentile of import exposure is 3591 Euro (Table A5.2 reports mean import exposures across different years and quantiles) in 2000, giving a difference in effects of 0.86 percentage points. Comparing the size of 2SLS IV and OLS coefficients, OLS coefficients tend to be consistently smaller (in absolute terms) than 2SLS coefficients, pointing to the fact that there is a positive correlation between import demand shocks and labour demand, which means that OLS underestimates the true effects.

In Appendix Table A5.3, I present results for considering import exposure from China and Eastern Europe separately. The results show that the overall, *i.e.,* combined effects of import exposure from China and Eastern Europe in the main effects are predominantly driven by

China, not Eastern Europe. This might be explained by the fact that for Eastern Europe, import and export exposure are highly correlated with a correlation coefficient of 0.77. This suggests that there is much inter-industry trade and that therefore more employment opportunities may have been retained for industries exposed to Eastern European imports, and therefore do not pose much of a shock. On the other hand, Chinese import and export only have a correlation coefficient of 0.19, meaning that Germany does not export to China in the same industries as China exports to Germany. This can imply that only the import shock from China actually impacted individual's vocational education and labour market outcomes. Contrary to my results, Dauth, Findeisen and Suedekum (2014) find that trade with Eastern Europe caused much stronger industry employment displacement effects than trade with China, due to the fact that trade with Eastern Europe increased earlier, as well as in other industries in which German counties had more initial specialization

5.4.2 Occupation Tasks: Computer Use and Manual Labour

The previous section establishes that individuals enter more skill-specific occupations and more import exposed manufacturing industries. To further understand what that means in terms of the skills and tasks required within the occupations an individual choose, Table 5.3 reports results for computer use within occupations and whether the occupations is a manual, easy manual or qualified manual occupation. The table reports IV results. Column 1 reports the probability of entering an occupation with above median computer. Increased import competition reduces the probability of an individual entering an occupation with computer use and the size of the coefficient (0.2 percent) is similar to that of entering manufacturing. Computer use is of such central importance in technological progress and at the heart of the skill-biased technological change idea (Card and DiNardo, 2002) and those able to use a computer can learn to control machines, analyse or communicate, even as manufacturing becomes increasingly automated. Despite the globalization forces and structural change the individuals in this sample are exposed to, they do no enter professions that may teach them important IT skills such as communication, data analysis or coding. This would act as an insurance against unemployment, in case the industry in which the individual is employed, is subject to further import competition or automation.

Column 2 shows the impact of import exposure on entering a manual occupation. Again, the coefficient is very much in line with the size of the coefficient on manufacturing, which is unsurprising since manufacturing is inherently manual in nature. In Columns 3 and 4, manual work is further split up into easy manual and qualified manual occupations. The results show that, at least, import exposure pulls individuals more into qualified manual labour, and there is a null effect for easy/unqualified occupations. This finding is reassuring, since apprentices go into vocational education to learn a somewhat skilled occupation. This means that while growing up in import competition exposed counties pulls individuals more into manual, less computerized vocational occupations, these occupations are at least those requiring qualifications and skills, and are not just simple manual labour.

5.4.3 Later Labour Market Outcomes

This section discusses the effects of import exposure on individual labour market outcomes. The upper panel of Table 5.4 reports effects on (log daily gross) earnings at different stages. Being exposed to 1000 Euro per worker higher import exposure in their county than elsewhere when 15 (for those who enter vocational education subsequently) affects individuals negatively, in that they earn 0.14 percent less during their apprenticeship, not significantly less a year after finishing their apprenticeship, 0.44 percent less five years after finishing their apprenticeship and 1.1 percent less ten years after finishing their apprenticeship[15]. Comparing these marginal effects to the average increase in import exposure of 4180 Euro, the average loss in income 10 years post apprenticeship due to import exposure amounts to 4.6 percent, which is a substantial reduction in income. Column 6 shows that 1000 Euro import exposure leads to 5.7 percentage points lower earnings growth over 10 years, which is also quite a sizeable negative effect. These effects are independent of which occupations individuals choose, the only restriction being that individuals enter vocational education. The effects can be compared to the literature on adverse effects of growing up in a recession, or growing up in a poor neighbourhood Oreopoulos, von Wachter and Heisz (2012) find that graduating from college in a recession causes earnings losses for individuals for over ten years. An unemployment rate of 5 percent implies an earnings loss of 9 percent initially, which slowly fades away over a ten year period. Here by contrast we see that being exposed to import competition and corresponding structural unemployment, causes losses in lifetime earnings that become larger over time.

The bottom panel of Table 5.4 shows the effects of import exposure on some additional labour market outcomes, which may provide potential channels explaining these adverse income effects. The results show, that individuals in more import exposed counties enter apprenticeships at an earlier age. This may be because individuals leave school earlier and enter VET with a lower degree. I find no evidence for this in Section 5.4.4 however, where I check for differential selection into education tracks. Another explanation is that individuals find apprenticeship places faster or do not take time off, but rather that their path ahead is clear and does not deviate from what is and has been prevalent in the local labour market.

Column 8 of Table 5.4 indicates that import exposure makes individuals more likely to be employed the year immediately after finishing their apprenticeship. This result is in line with the finding from Hanushek et al. (2017*a*), who show that "skill-specific" education helps with the transition from schooling to the labour market. The result here suggests that this also seems to hold for general versus skill-specific occupations *within* vocational education. There is no significant effect on the number of years of unemployment, which shows that although individuals take wage cuts, they are not more likely to be unemployed. Lastly, the results show that individuals are less mobile in terms of regional (inter-county) mobility, but instead more

[15] Duration of apprenticeships is not differentially affected, so these effects do not stem from differential firm tenure post apprenticeship

mobile in terms of occupational mobility. These results are in line with Dauth, Findeisen and Suedekum (2014) who show that there are no adjustments through interregional migration due to trade shocks.

Note that the above results just show the effect of import exposure on labour market outcomes, but ideally one would be interested in how the choice of VET occupation affects labour market outcomes, in particular, whether individuals are sheltered from trade and automation. In other words, does the hypothesis that individuals in service and merchant occupations are more sheltered and therefore experience less adverse earnings outcomes due to import exposure? It would be very difficult to provide causal evidence on the effect of vocational occupation choice on labour market outcomes, because occupation choice is an endogenous variable that is correlated with unobserved individual characteristics such as talent or motivation. A regression linking vocational occupation choice and labour market outcomes would suffer from selection bias, because a highly motivated individual, may take the rational occupation choice and have a high income, but that same individual may have fared just as well in any other occupation and the seeming positive relationship would be due to the unobserved factor motivation.

Nevertheless, Table 5.5 presents suggestive evidence showing the effects of import exposure and import exposure interacted with the vocational education occupation choice on 10-year earnings growth. Column 1 shows the results when import exposure is interacted with whether an individual chooses a manufacturing occupation. The level effect of import exposure is insignificant, while the interaction term shows a decline of 8 percentage points on 10-year earnings growth for 1000 Euro import exposure if an individual pursued VET in a manufacturing occupation. This implies that the adverse effect of import exposure is entirely driven by individuals in manufacturing occupations and is zero for all other occupations.

In Column 2, both the level effect as well as the interaction term of import exposure with craft occupations is negative. Contrary to this, in Columns 3 and 4 the interaction terms for service and merchant occupations respectively are positive and of similar magnitude as the level effect of import exposure, implying that the adverse effect on ten year earnings growth cancels out. The results although not causal, suggest that indeed more general occupations in services and as merchants have a sheltering effect from the negative effects of import exposure during adolescence.

An obvious channel as to why individuals still enter these occupations that affect them adversely would be their parents. Table A5.4 shows evidence from the German Socio-Economic Panel, which allows to link family ties and also asks about youth's occupational aspirations. Results in Panel A columns 1 and 2 show that the general results for occupation choices hold also in this data (though the effects are not significant due to small sample sizes). Aspirations of young individuals however, are affected in the opposite directions, and as one would have expected in the first place, namely choosing less manufacturing and more service occupations when exposed to import competition. Therefore, there seems to be a mismatch between aspi-

rations and actual choices. Panel B shows the impact of parental occupations on vocational occupation choices. Columns 1 and 2 show that while parental occupation is an important driver in adolescents' occupation choices meaning that individuals enter the same occupation as their parents, columns 3 and 4 show that having a father that worked in manufacturing and growing up in regions exposed to import competition, actually decreases the probability of individuals to enter a skill-specific manufacturing job. This suggests that potential first-hand negative experiences of job or income loss due to import competition within a family, may work to dissuade individuals from taking up skill-specific occupations.

5.4.4 Not an Endogenous Sub-sample

This chapter focuses on young individuals who self-select into apprenticeships, rather than going to university. Since this is a non-random subgroup of individuals, there must not be any differential and therefore endogenous selection into this subgroup because of trade exposure; as this would introduce a bias into the estimations. Table 5.6 Column 1 shows the effect of imports at time t on transitions from elementary school to higher school tracks at $t-5$. Since the "treatment" of import exposure concerns 15 year-olds, these same individuals should not have selected into academic track or middle track school differentially at the age of 10. As the coefficients show, there is no evidence of this. Columns 2 and 3 show the effects of import exposure at time t on 7th graders in academic and middle track schools at $t-3$ and also show no effect. Column 4 presents the effect on graduates from the middle track at time t, again showing no effect. Most importantly, there is also no effect on total numbers of apprentices at t+1 from trade. This robustness check shows, that there was no differential selection into different education levels due to the trade shock. With no educational upgrading, it is clear that keeping the level of education fixed at the vocational level and investigating individual vocational education occupation choices, is the relevant research question and therefore relevant level of analysis.

5.4.5 Choice or No Choice?

So far, it is unclear whether the occupational paths on which young individuals embark can be called "choices", or whether they are entirely driven by labour demand. Since "choosing" an apprentice occupation differs importantly from choosing a university major in the sense that it strongly depends on local availability of a firm offering an apprentice positions in such an occupation, this is a major concern. Unavoidably a certain portion of the type of vocational occupation individuals enter, is due to their local industry structure. Nevertheless, any county will also have service and merchant related apprenticeship positions on offer, for example as accountants, tax consultants or procurement specialists. One wants to know whether limitations in apprenticeship choice are systematically related to trade shocks: if the same firms, which are hit by import competition now also offer less apprentice positions, or employ more apprentices as a way of having cheaper labour, any of the findings may be purely driven by the labour demand side and have nothing to do with individual choices. Knowing which is

the driver makes an important difference for policy implications, *i.e.,* whether an information intervention in schools, or a policy aimed at the firm side would be effective in teaching more transferable skills to young adults.

The results speak against the fact that individuals have no real choice in their apprenticeship and have to take what is available on the local labour market, because the effects show that young adults still go into manufacturing and import manufacturing despite the firms being exposed to import competition and may see higher unemployment and firm closures. Moreover, it has been found that firms, at least in the short run, do not adjust the number of apprentice places according to the business cycle (Luethi and Wolter, 2018), which speaks for the fact that numbers of apprenticeship positions should be fairly stable. To further investigate this issue, I look at local supply-demand relations of apprenticeship positions. These statistics provide information, on exactly how many apprentice places were offered, how many new contracts were signed, how many candidates looked for an apprenticeship and how many were left without a spot. These data are available at the level of labour market regions of job centres, which are constituted of two to four counties. While there are 402 counties, there are 176 job centre labour market regions, which are sometimes referred to as German commuting zones. These data are available for all occupations aggregated from 1998 to 2011, while between 2004 and 2011 they are also available on the occupation level. Table 5.7 shows results for the effect of import exposure (aggregated up to the labour market regions), on outcomes concerned with supply-demand-ratios of apprenticeship positions for all occupations together. The supply-demand ratio in Column 1, is calculated by adding up all apprenticeship positions offered (new apprenticeship contracts (*i.e.,* matches) plus unfilled positions) and dividing them by all apprenticeship positions searched (new apprenticeship contracts plus unsuccessful candidates). A supply-demand ratio of 1 means that there is perfect clearing on the apprenticeship market; a number larger than one indicates excess supply, a number less than one indicates excess demand of apprenticeship positions. Column 2 looks at the numbers of unfilled apprentice positions, Column 3 at the number of successfully signed new contracts and Column 4 at the number of unsuccessful apprenticeship candidates in the given year. There are no significant effects of import exposure on any of these measures. These null-effects are robust to trying different timings of the import shock, *i.e.,* taking the lagged import shock, for example.

In Table 5.8, unsuccessful candidates by different occupational groups are analysed For Columns 1 and 2, vocational education occupations are split among manual or office-based. For Columns 3, 4 and 5, I use the available information of which “chamber” the vocational education is administered by (Chamber of Crafts and Trade; Chamber of Industry and Commerce or Public Services). There are no effects on the number of unsuccessful candidates for the occupational categories of manual, office, craftsmen nor for industry and commerce. This shows, that there is no acute shortage of office jobs even in import-exposed regions. This means that there is no evidence that young individuals searching for apprenticeship places who really wanted to get an office job were forced to take a manufacturing apprenticeship.

The only coefficient which is significant is public service, meaning that there is an oversupply of candidates for public service apprenticeships compared to the amount of places offered. While this shows that there is increased interest in public service, or perhaps just a shortage of apprenticeship position where import exposures are stronger, it is unlikely that this effect on unsuccessful public service apprenticeship places drives our main results, since the share of apprenticeships in public service is below 4 percent (BIBB, 2016). This analysis provides evidence that local apprenticeship markets cleared well even in import exposed regions and that the results are indeed likely to be driven by individual choices, rather than only by labour demand.

5.4.6 Robustness Check: Alternative Measures of Import Shocks

To make sure that the results are not solely produced by the definition of import exposure I choose, Table 5.9, presents results of alternative definitions of import shocks. Coefficients are shown for a selection of outcomes: manufacturing occupation, crafts occupation, import industry, computer use and 10-year earnings growth.[16]

Each cell in Table 5.9 refers to a separate regression. There are four different import exposure definitions, "baseline" being the same as in the main analysis, namely the 10-year rolling window changes between t and $t-10$, with $t-10$ as base year for the industry employment structures. "Cumulative" refers to 10-year rolling window cumulated import volumes apportioned by initial ($t-10$) regional industry structures. "Current year" refers to the current year total import exposure apportioned by initial regional industry structures at $t-10$. "Fixed baseline" refers to 10-year changes, like in baseline, but with fixed initial industry structure at 1980 for the Western Germany and with 1993 for Eastern Germany. The alternative measures of import exposure produce very similar results to the baseline, meaning that the results do not hinge on just the 10-year year rolling window changes and $t-10$ industry structure that are used in the main analysis. Using cumulative import shocks, *i.e.,* adding up all the import volumes over ten years naturally produces a smaller coefficient per 1000 Euro per worker, because the shock is numerically a lot larger (by a factor a little less than tenfold). The current-year import exposure gives very similar effects as the baseline in both significance and magnitude, indicating that the baseline results are driven by the large increases in trade volumes in later years, not by the starting levels, which where close to zero in all regions. Fixing the baseline at 1990 or 1993 also gives very similar results though with a little smaller effect sizes. Not allowing the initial industry structure to vary at all over 20 years gives probably cleaner in terms of endogenous adaptation of counties but also less realistic representations of the real trade shock. However, the results are still very similar that this does not give reason for concern.

[16] The table shows only a selection of outcomes, the alternative measures work similarly well for all outcomes. The results are available upon request.

In a another robustness check, I investigate whether "shocking" individuals with trade shocks at different ages changes the the results. The effects same identical to the estimates of assignment the trade shock at the age of 15, or at 13 or 14 and 16 and 17.[17] Since the trade "shock" constitutes a ten year change in import exposure and individuals are exposed before and after the same, it is not surprising that the results are the same. I choose to assign the treatment at the age of 15, because it is usually a year or two before an adolescent in middle school tracks choose their apprenticeship occupation.

5.4.7 Heterogeneities Across Gender

Males and females make inherently different labour market decisions, in particular in the middle-skill section of vocational education, where more than in high-skilled jobs, occupations are strongly fragmented by gender. Whether women react differently than men to growing up in an import exposed county, is therefore an interesting question. I therefore estimate the following regression equation:

$$\begin{aligned} y_i^{r,t} = \beta_1 \Delta ImportExposure_{rt}^{Ger \leftarrow C+EE} + \beta_2 \Delta ExportExposure_{rt}^{Ger \rightarrow C+EE} + \\ \beta_3 female_i + \beta_4 \Delta ImportExposure_{rt}^{Ger \leftarrow C+EE} x female_i + \\ \beta_5 X_i + \beta_6 X_r + \gamma_{t*s} + \epsilon_i. \end{aligned} \tag{5.7}$$

If the individual is female, β_1 + β_4 is the effect of import exposure and only β_1 if the individual is male. Table 5.10 reports selected results from heterogeneity analyses of interacting 10-year changes in import exposure with the individual being female.[18] The top panels shows heterogeneous effects for occupation type and tasks. There is no differential effect of import exposure for women on entering manufacturing, crafts or service occupations. While there are of course large level differences of men and women as can be seen from the female coefficient, exposure to import competition does not induce a different behaviour from men for those occupation categories. However, females exposed to import competition enter merchant occupations more and also occupations with more computer use. Females also choose relatively less manual but more qualified manual occupations when exposed to imports. These results suggest that females shelter themselves more from import exposure, because they do chose slightly more general occupations with more computer use and less manual labour. However, this is not reflected in the labour market outcomes of females. The lower panel of Table 5.10 shows heterogeneous effects of labour market outcomes. Import exposure affects females more adversely than men. In terms of earnings, females are worse off during the apprenticeship, one year after and ten years after finishing the apprenticeship. They are also less likely to be unemployed the year after finishing their apprenticeship and are

[17] Results available upon request.
[18] The variables were demeaned before building their interactions.

unemployed for more years throughout their careers. They are however, more mobile in terms of both occupational as well as regional mobility.

The results suggest that females are more adversely affected by import competition than men in terms of labour market outcomes, a finding that is also found in the graduating in a recession literature (e.g. Hershbein, 2009) and an important aspect for policy implications.

5.5 Concluding Remarks

In this chapter, I investigate the impact of growing up in a German region exposed to import competition from China and Eastern Europe. Looking at the choice of vocational occupation as the relevant point of labour market entry and to keep the educational level constant, the chapter provides causal evidence on the effect of local import shocks on (1) the type of vocational occupation, (2) the task content of the occupation, and (3) the effect on further life labour market outcomes. The chapter uses individual-level longitudinal social security data and other high quality data sources such as administrative firm data, bilateral trade data and data on apprenticeship position for this empirical investigation.

The results show that import competition perpetuates vocational occupation choices of individuals, rather than leading to adjustments into more general and service oriented occupations. First, I find that greater exposure to import competition pulls individuals more into manufacturing occupations, more into craftsmen occupations and import industries in particular, and less into service and commerce occupations. The results imply that individuals do not adjust away from the predominant industry structure of the county they grew up in, and therefore do not protect themselves from future further forces of globalization through more import competition. Secondly, I find that the task content of occupations individuals choose, does not teach them general and transferable skills. I find that increased import exposure makes adolescents less likely to enter occupations with high computer use, and more likely to enter manual occupations. Lastly, I find that individuals exposed to import competition in their adolescence who enter vocational education, are adversely affected on the labour market in later life. They earn less 5 and 10 years after their apprenticeships if finished and also see less earnings growth. They are more mobile in terms of occupational mobility, but less mobile regionally. While not causal, I demonstrate that these adverse labour market outcomes are at least partly related to choices of vocational occupation types. Analysing occupational choices within vocational education is the right level of analysis for Germany, as there is no differential selection into different schooling tracks (academic versus non-academic track) in response to import shocks. Furthermore, looking at supply-demand ratios of apprenticeship positions, I show that the results are not purely labour market demand driven. The results are also robust to various import definitions.

I find that the effects are very heterogeneous across genders. Women, as opposed to men are more likely to enter service oriented and merchant based occupations when exposed to local

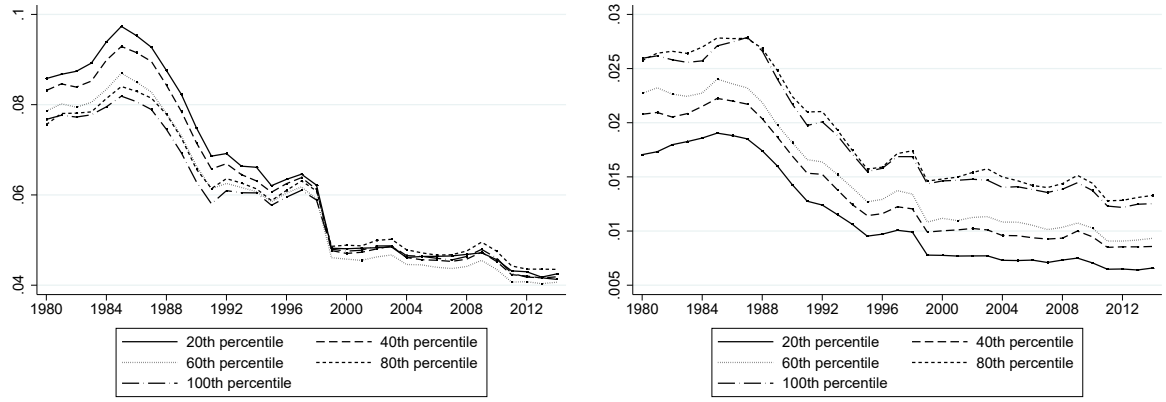
import competition during adolescents. They also choose occupations more computer use. However, women are nevertheless still adversely affected by import competition in terms of later labour market outcomes.

This chapter has contributed to the existing literature in several ways. It is the first study to look at the effect of local import exposure on individuals who grew up in exposed regions. It uses yearly variation in import exposure in addition to regional variation. It is also the first study to bring together the impact of trade shocks and occupation choice at labour market entry. It extends the literature on general versus skill specific education and applies it horizontally to vocational education by combining it with occupational skill-specificity measures. It also contributes to the literature on the impact of economic conditions such as recessions on schooling decisions and later life outcomes, by showing the effect of structural change on personal vocational occupation choices and later life outcomes.

The chapter suggests that the adjustment of occupational choices into more service-oriented occupations in response to import exposure does not take place at the level of young individuals growing up in import exposed regions. Rather, initial industry structures seems to be perpetuated by young labour market entrants, in that they are still more likely to choose manufacturing and import industries when having been exposed to more import competition at the age of 15. In terms of policy implications this calls for better informational access when individuals choose their apprenticeship positions. More job fares or better information about the 320 different possible occupations requiring vocational education may pose avenues for potential policies targeted at young individuals.

Figures and Tables

Figure 5.1 : Apprentice Shares by Quantiles of Import Exposure in 2000

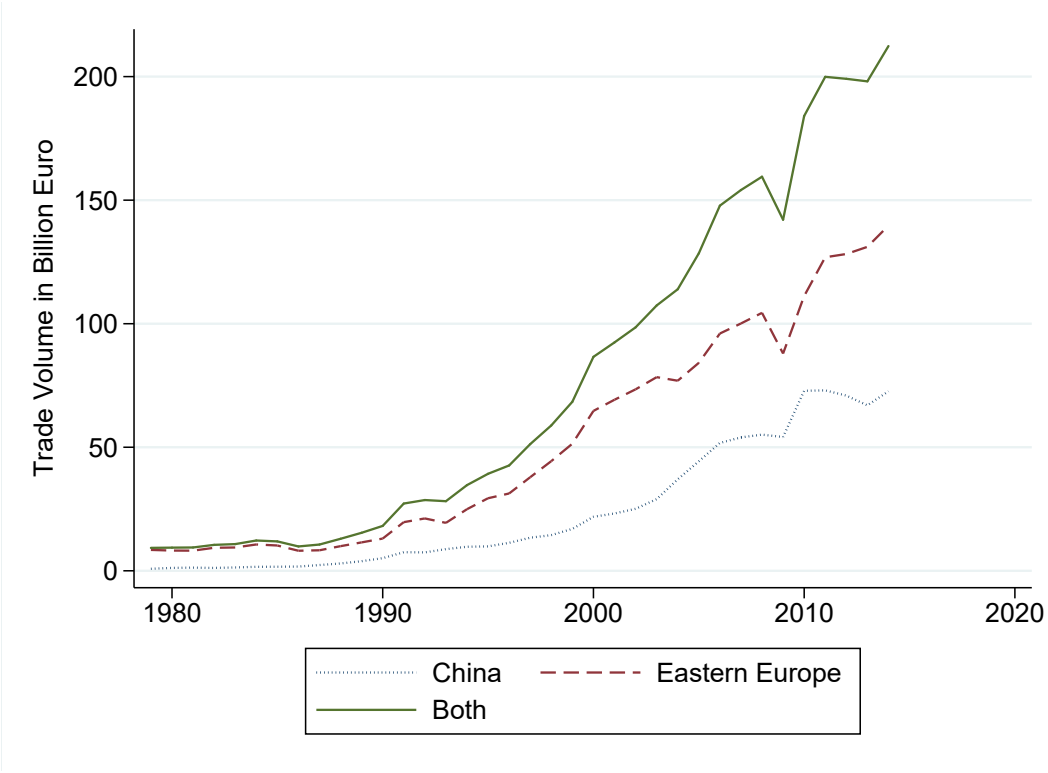


(a) All apprentices

(b) Manufacturing apprentices

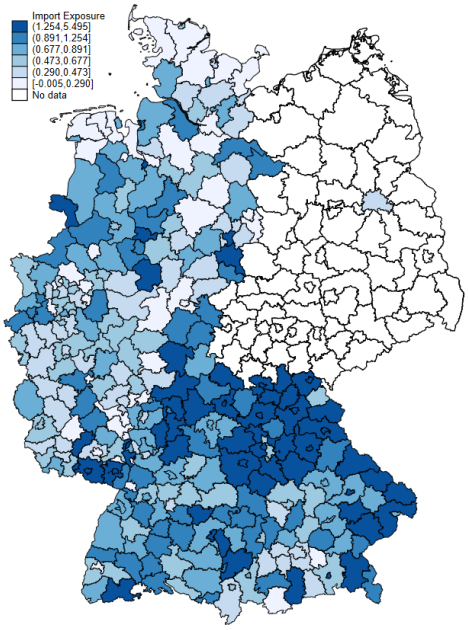
Note: Shares of apprentices among all workers within a county. Counties divided into 5 quantiles along import exposures in 2000. Figure (a) refers to apprentices in all occupations, Figure (b) to apprentices in manufacturing occupations.
Source: Establishment History Panel.

Figure 5.2 : Total Trade Volumes in Billions of 2010 Euros

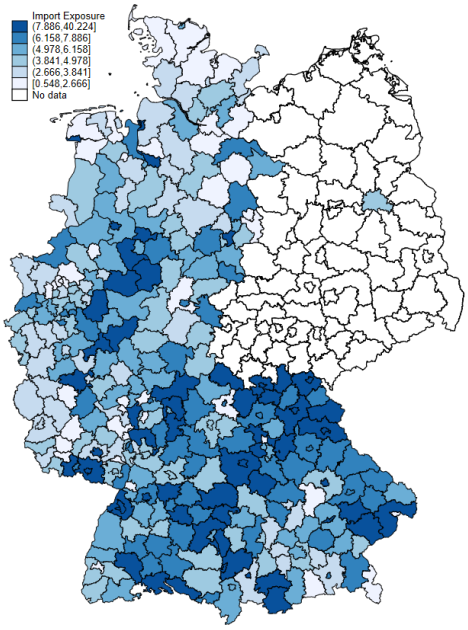


Note: Import volumes from China, Eastern Europe and the two combined in 2010 billions of Euros.
Source: UN Comtrade Data

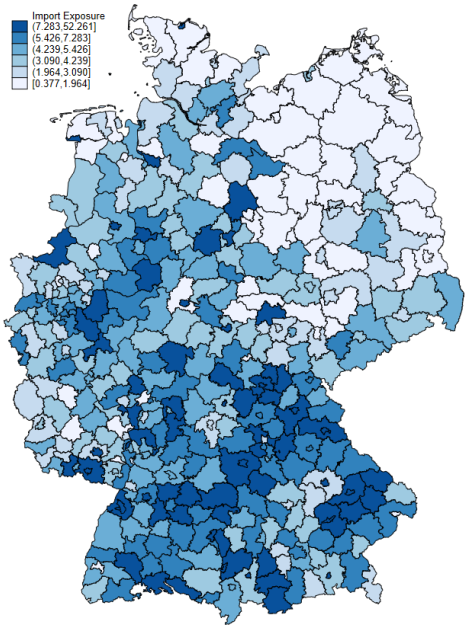
Figure 5.3 : 10-year Changes in Import Exposure Per Worker



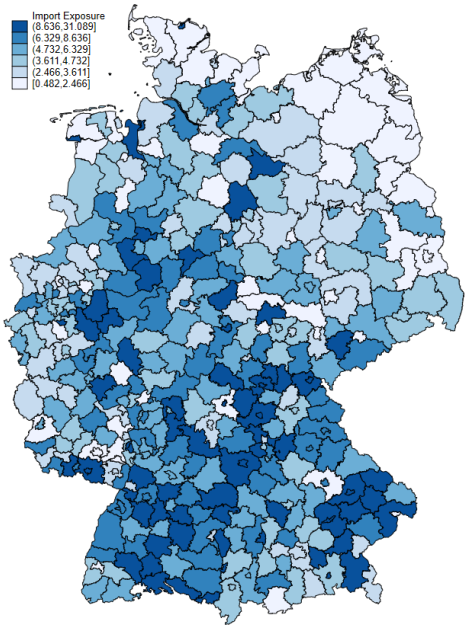
(a) 1990: change from 1980-1990



(b) 2000: change from 1990-2000



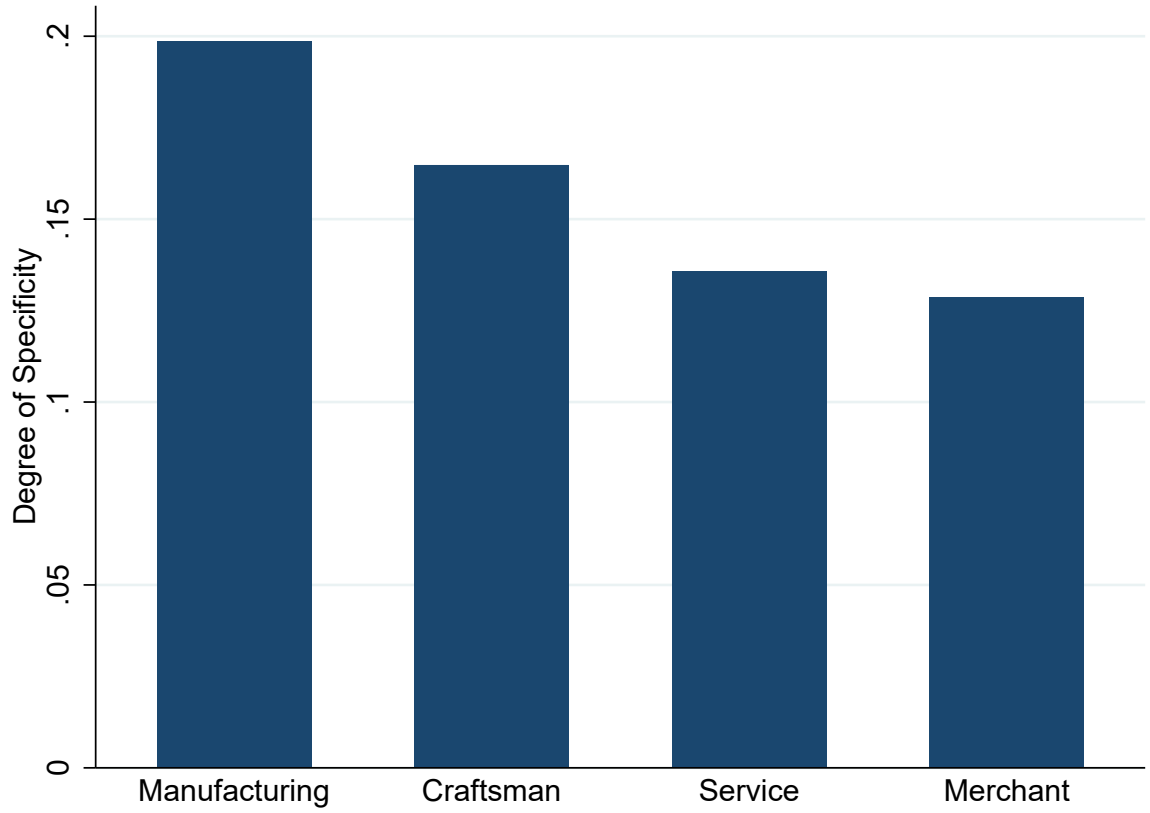
(c) 2003: change from 1993-2003



(d) 2014: change from 2004-2014

Note: 10-year changes in import exposure per worker in 1000 Euros. Figures (a) and (b) exclude former Eastern German counties due to data availability.
Source: UN Comtrade Data and Establishment History Panel, own calculations.

Figure 5.4 : Occupational specificity by Occupation Group



Note: The figure shows the average skill-specificity measures in the four occupational groups manufacturing, craftsmen, services and merchants. The skill-specificity is an angular distance measure representing the distance in skill bundles between an occupation and the average labour market; see Section 5.3.4. Source: BIBB/IAB Qualification and Occupational Career Surveys 1999, own calculations.

Table 5.1 : Summary Statistics

Var	Mean	Std Dev	Var	Mean	Var	Mean	Std Dev
Trade Exposure			Year when 15		Occupation Category		
Import Exposure Overall	4.1804	4.1113	1991	0.0490	Manufacturing	0.3535	
Import Exposure Eastern Europe	2.5618	2.5784	1992	0.0468	Craftsmen	0.2462	
Import Exposure China	1.6186	2.4284	1993	0.0466	Service	0.5713	
Export Exposure Overall	4.3094	4.0889	1994	0.0471	Merchant	0.1822	
Export Exposure Eastern Europe	3.4701	3.1157	1995	0.0484	Import Industry	0.1278	
Export Exposure China	0.8393	1.3090	1996	0.0483			
Individuals			1997	0.0478	Skill specificity	0.1183	0.0460
Female	0.4580		1998	0.0465	Tasks		
German	0.9283		1999	0.0467	Computer Use	0.4230	
State			2000	0.0448	Manual	0.2479	
Schleswig-Holstein	0.0376		2001	0.0472	Easy Manual	0.0307	
Hamburg	0.0208		2002	0.0583	Qualified Manual	0.2172	
Lower Saxony	0.1112		2003	0.0589	Labour Market		
Bremen	0.0095		2004	0.0559	Gross daily Earnings		
Northrhine-Westphalia	0.2304		2005	0.0555	During	24.5801	10.1441
Hesse	0.0780		2006	0.0476	One year after	50.8276	28.4435
Rhineland-Palatine	0.0560		2007	0.0429	5 years after	70.6205	35.7110
Baden-Wurttemberg	0.1533		2008	0.0382	10 years after	80.8797	43.9291
Bavaria	0.1893		2009	0.0311	Growth after 5 year	1.2744	6.5158
Saarland	0.0152		2010	0.0219	Growth after 5 year	1.6256	7.4187
Berlin	0.0372		2011	0.0155	Age at apprenticeship	19.3283	2.3581
Brandenburg	0.0105		2012	0.0062	Employed first year after	0.6335	
Mecklenburg-Vorpommern	0.0089		2013	0.0006	Different County after	0.2936	
Saxony	0.0202				Occupation switches	2.5803	2.1433
Saxony Anhalt	0.0111				County Switches	1.2553	1.4486
Thuringa	0.0108				Years unemployed	0.7666	1.6684

Note: N= 196,025. *Source:* Individual social security (SIAB) data.

Table 5.2 : Effects of Import Exposure on Vocational Education Occupation Types

Panel A: OLS					
	(1)	(2)	(3)	(4)	(5)
	Manufacturing	Craftsmen	Service	Merchant	Import Manufacturing
Import Exposure	0.0014*	0.0015**	-0.0011**	-0.0009**	0.0016***
	(0.0008)	(0.0006)	(0.0005)	(0.0004)	(0.0006)
N	196025	196025	196025	196025	196025
R-squared	0.007	0.286	0.104	0.297	0.021
Panel B: IV					
	(1)	(2)	(3)	(4)	(5)
	Manufacturing	Craftsmen	Service	Merchant	Import Manufacturing
Import Exposure	0.0023**	0.0019**	-0.0016**	-0.0009**	0.0023***
	(0.0011)	(0.0008)	(0.0007)	(0.0006)	(0.0007)
N	180000	180000	180000	180000	180000

Note: Panel A refers to OLS regressions, Panel B to 2SLS IV regressions. The F-statistic for the first stage regression in the 2SLS (Panel B) is 127.86. The outcome in column 1 is a dummy for whether the occupation an individual enters in in manufacturing, in column 2 a crafts occupation, in column 3 a service, in column 4 a merchant occupation. The outcome in column 5 is a dummy for whether the occupation in a import-intensive manufacturing industry. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* Individual social security (SIAB) data.

Table 5.3 : Effects of Import Exposure on Occupation Task Characteristics

	(1) Computer use	(2) Manual	(3) Easy Manual	(4) Qualified Manual
Import Exposure	-0.0020***	0.0029**	-0.0003	0.0015**
	(0.0006)	(0.0012)	(0.0002)	(0.0007)
N	180000	180000	180000	180000
F-Stat 1^{st} stage	129.61	127.86	127.86	127.86

Note: The outcome computer use stems from the BIBB occupation survey with four waves between 1992-2012 and refers to a dummy indicating whether the majority of individuals in an occupation state they use computers often or very often in their job. Outcomes in columns 2-4 refer to dummies whether the classification is “manual”, “easy manual” or “qualified manual”, as classified by Bloosfeld. All results stem from 2SLS IV regressions. The unit of observation is the individual, observed in the data once. The individual is “treated” by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * p$<$0.10, ** p$<$0.05, *** p$<$0.01. *Source:* Individual social security (SIAB) data.

Table 5.4 : Effects of Import Exposure on Earnings and Other Labour Market Outcomes

	Gross daily Earnings					
Panel A	(1) During apprenticeship	(2) 1 year after apprenticeship	(3) 5 years after apprenticeship	(4) 10 years after apprenticeship	(5) Growth after 5 years	(6) Growth after 10 years
Import Exposure	-0.0014**	-0.0013	-0.0044**	-0.0110***	-0.0112	-0.0579***
	(0.0006)	(0.0011)	(0.0018)	(0.0033)	(0.0082)	(0.0192)
N	180000	150000	80000	44000	77000	43000
Fstat 1st Stage	127.86	91.79	233.42	315.33	229.64	311.79
	Other Labour Market Outcomes					
Panel B	(7) Age at apprenticeships start	(8) Employed first year after apprenticeship	(9) Years unemployed	(10) Times county switched	(11) Occupation switches	
Import Exposure	-0.0100**	0.0012**	0.0008	-0.0044**	0.0050**	
	(0.0046)	(0.0006)	(0.0017)	(0.0022)	(0.0025)	
N	180000	180000	180000	180000	180000	
F-Stat 1^{st} stage	127.86	127.86	127.86	127.86	127.86	

Note: Each cell refers to a separate regression. Outcomes in panel A columns 1-4 refer to gross daily earnings, during, one year upon completing the apprenticeship, 5 and 10 years after the apprenticeship, respectively. The outcome in column 5 (6) refers to growth between the first year and the fifth (tenth) year after finishing the apprenticeship. In Panel B, the outcome in column 7 refers to the age at which an individual begins his apprenticeship, column 8 is a dummy for whether the individual is employed the year after finishing the apprenticeship, column 9 refers to the number of years an individual is unemployed throughout his life, column 10 to the number of times the individual moves county and 11 the number of times the individual switches occupations. All results stem from 2SLS IV regressions. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01. *Source:* Individual social security (SIAB) data.

Table 5.5 : 10-Year Earnings Growth by Vocational Occupation Choice

	(1) Manufacturing	(2) Craftsmen	(3) Service	(4) Merchant	(5) Import Manuf
Import Exposure	-0.0113	-0.0333*	-0.0896***	-0.0884***	-0.0637***
	(0.0208)	(0.0191)	(0.0219)	(0.0224)	(0.0193)
Import Exposure x Occ Category	-0.0866***	-0.0662***	0.0780***	0.0665***	0.0516
	(0.0219)	(0.0248)	(0.0192)	(0.0197)	(0.0720)
N	43000	43000	43000	43000	43000
F-Stat 1^{st} stage	205.56	197.36	201.11	200.65	207.17

Note: The results refer to the effects of import exposure on 10-year earnings growth, and the interaction of having chosen the respective occupation type. All results refer to 2SLS IV regressions. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01. *Source:* Individual social security (SIAB) data.

Table 5.6 : Non-Endogenous Subsample: Selection into School Tracks

	4th grade transitions	7th grade		Graduates		Apprentices
	Academic track	Middle school	Academic track	Middle school	Academic track	
	(1)	(2)	(3)	(4)	(5)	(6)
Import Exposure	-0.0005	-0.00057	-0.00017	-0.00015	-0.00001	0.0011
	(0.0012)	(0.00051)	(0.00021)	(0.00028)	(0.00026)	(0.0016)
N	422	6320	6320	7318	5748	8736
R-squared	0.235	0.024	0.561	0.277	0.549	0.077

Note: Data for this analysis is at the county-year level for 402 counties across varying amounts of years, according to data availability. The outcomes represent shares of students over the total at the respective level. Import exposure is used at the respectively correct time to test non-differential selection into subsamples due to the treatment. In column (1) it is tested whether the trade shock at t+6 (referring to the change of t-(t-10) in trade exposure), lead pupils to differentially select into the academic track high-school after fourth grade, at t. In columns 2 and 3 the outcomes are shares of seventh graders in the middle track and academic track high-school at t, and the import shock refers to t+3. Columns 4 and 5 refer to graduates from middle school (with import at t) and academic track high-school (with trade shock at t-4). All regressions include year and individual fixed effects and also control for export exposure. Robust standard errors clustered on county level in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01. *Source:* Regional Statistical Data Catalogue of the Federal Statistical Office and the statistical offices of the Länder.

Table 5.7 : Supply-Demand Relations for Apprenticeship Positions for All Occupations

	(1) Supply-demand	(2) Non-filled positions	(3) New contracts	(4) Unsuccessful applicants
Import Exposure	-0.0000	-0.6203	-30.6334	0.2827
	(0.0004)	(1.0808)	(22.0862)	(1.1758)
N	4793	4793	4793	4793
R-squared	0.193	0.193	0.463	0.712

Note: Analysis on the level of 176 job centre labour market regions. Outcomes refer to supply and demand of apprentice positions in labour market regions. Column 1 refers to the suppy-demand ratio of apprenticeship positions, with all supplied apprenticeship positions (new contracts and unfilled positions) over all demanded positions (new contracts and unsuccessful candidate). Column 2 refers to non-filled positions, column 3 to new apprenticeship contracts agreed and column 4 to the number of unsuccessful applicants. Years 1998-2011 are included. All regressions include year and labour market region fixed effects. All regressions include state-by-year fixed effects. Robust standard errors clustered on labour market region level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* Federal Institute for Vocational Education and Training.

Table 5.8 : Unsuccessful Apprenticeship Candidates by Occupation Category

	(1)	(2)	(3)	(4)	(5)
	Manual	Office	Craftsmen	Industry and Commerce	Public Service
Import Exposure	0.2569	0.2589	0.1456	0.3794	0.0176**
	(0.2437)	(0.2941)	(0.1927)	(0.4094)	(0.0077)
N	1168	1168	1168	1168	1168
R-squared	0.548	0.532	0.554	0.534	0.458

Note: Analysis on the level of 176 job centre labour market regions. Outcomes refer to supply and demand of apprentice positions in labour market regions. The outcome are unsuccessful apprenticeship applicants in each labour market region. Years 2004-2011 are included. All regressions include state-by-year fixed effects. Robust standard errors clustered on labour market region level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* Federal Institute for Vocational Education and Training.

Table 5.9 : Robustness Check: Using Different Import Exposure Definitions

	(1) Manufacturing	(2) Craftsmen	(3) Import Industry	(4) Computer	(5) 10 Year Earnings Growth
Baseline	0.0014*	0.0016**	-0.0043***	-0.0017***	-0.0652***
	(0.0008)	(0.0007)	(0.0016)	(0.0005)	(0.0202)
Cumulative	0.0002**	0.0003***	-0.0010***	-0.0002***	-0.0093***
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0026)
Current Year	0.0013*	0.0017***	-0.0057***	-0.0016***	-0.0586***
	(0.0007)	(0.0006)	(0.0017)	(0.0004)	(0.0159)
Fixed Baseline	0.0014	0.0012*	-0.0033**	-0.0014***	-0.0573***
	(0.0009)	(0.0006)	(0.0015)	(0.0005)	(0.0204)

Note: Each cell refers to separate regression. The alternative import exposure measures are: *Baseline* refers to our ten year rolling window changes in trade exposure such as in the main results. *Cumulative* refers to rolling window ten year cumulated import volumes apportioned by initial (t-10) regional industry structures. *Current year* refers to the current year total import exposure apportioned by initial regional industry structures at t-10. *Fixed baseline* refers to ten year changes, like in baseline, but with fixed initial industry structure at 1980 for the West and with 1993 for the East. All regressions are OLS. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* Individual social security (SIAB) data.

Table 5.10 : Hetereogeneity Analysis by Gender

	(1) Manufacturing	(2) Craftsmen	(3) Service	(4) Merchant	(5) Computer	(6) Manual	(7) Qualified Manual
Imp Exp	0.0013*	0.0015**	-0.0012**	-0.0010**	-0.0016***	0.0016*	0.0012**
	(0.0008)	(0.0006)	(0.0005)	(0.0005)	(0.0004)	(0.0009)	(0.0005)
Female	-0.5085***	-0.2708***	0.5317***	0.5182***	0.1505***	-0.4997***	-0.3161***
	(0.0063)	(0.0039)	(0.0062)	(0.0068)	(0.0020)	(0.0065)	(0.0043)
Imp Exp x Female	-0.0008	-0.0005	0.0011	0.0014*	0.0014**	-0.0018*	0.0021***
	(0.0008)	(0.0006)	(0.0007)	(0.0007)	(0.0005)	(0.0009)	(0.0007)
	(8) Earnings during apprenticeship	(9) Earnings 1 year after apprenticeship	(10) Earnings 10 years after apprenticeship	(11) Employed after apprenticeship	(12) Occupation switches	(13) Years unemployed	(14) Times counties switched
Imp Exp	-0.0011**	-0.0014	-0.0128***	0.0005	0.0040**	0.0010	-0.0027
	(0.0005)	(0.0008)	(0.0035)	(0.0006)	(0.0019)	(0.0015)	(0.0020)
Female	-0.0448***	-0.1539***	-0.5632***	-0.0073**	-0.3848***	-0.0568***	-0.0415***
	(0.0030)	(0.0079)	(0.0148)	(0.0036)	(0.0127)	(0.0103)	(0.0088)
Imp Exp x Female	-0.0009*	-0.0040***	-0.0121**	-0.0024***	0.0212***	0.0132***	0.0137***
	(0.0005)	(0.0011)	(0.0054)	(0.0007)	(0.0061)	(0.0036)	(0.0030)

Note: This tables reports the coefficients of interest from interactions; import exposure, female, and their interaction. The results refer to OLS regressions. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* Individual social security (SIAB) data.

Appendix

Appendix A5.1 Appendix Tables

Table A5.1 : List of Tasks and Skills from BIBB Survey used for Skill Specificity Measure

Tasks	
	Teach
	Consult
	Measure examine
	Monitor
	Repare
	Sell, Buy
	Organise
	Marketing
	Evaluate Information
	Negotiate
	Develop
	Produce
	Tend to people
Skills	
	Maths
	German
	Presentation
	Foreign Languages
	Sales, Marketing, PR
	Design
	Programme application
	Software Development
	Computer literacy
	Other technical knowledge
	Labour Law
	Other legal knowledge
	Management
	Finance
	Controlling
	Protection of Labour
	Medical Science
	Other Skills

Note: The skills and tasks stem from the BIBB/IAB Qualification and Occupational Career Surveys 1999.

Table A5.2 : Change in Import Exposure Per Worker by Quantiles and Years

	Percentiles			
Overall	25th	50th	75th	100th
1990	0.2546	0.5360	0.8616	1.7166
2000	2.3595	4.2749	5.9510	10.7129
2008	2.1580	4.1540	6.2708	13.3473
2014	2.1811	4.0019	6.0587	10.8712
Eastern Europe	25th	50th	75th	100th
1990	0.0832	0.2070	0.3230	0.6704
2000	1.7070	3.0035	4.2051	7.9448
2008	0.8035	1.7979	3.1105	6.5117
2014	1.0852	2.1264	3.5001	6.6666
China	25th	50th	75th	100th
1990	0.1257	0.2836	0.5075	1.1694
2000	0.5148	0.9946	1.5671	3.3654
2008	0.9402	1.8356	3.0902	7.8451
2014	0.8376	1.5215	2.3445	5.0357

Note: Table refers to mean 10 year changes in per worker trade exposure from Eastern Europe, China and both in 1000 Euro. Division into quantiles is different by each respective year to show total increases.

Table A5.3 : Results for Occupation Categories for Eastern Europe and China, IV and OLS

Panel A: OLS	(1)	(2)	(3)	(4)	(5)
Eastern Europe	Manufacturing	Craftsmen	Service	Merchant	Import Manufacturing
Import Exposure	0.0017	0.0021*	-0.0013	-0.0064	0.0067*
	(0.0017)	(0.0012)	(0.0011)	(0.0039)	(0.0035)
N	196025	196025	196025	180000	180000
R-squared	0.295	0.104	0.295	0.2798	0.021
Panel B: IV	(1)	(2)	(3)	(4)	(5)
Eastern Europe	Manufacturing	Craftsmen	Service	Merchant	Import Manufacturing
Import Exposure	0.0122**	0.0103***	-0.0067*	-0.0064	0.0025*
	(0.0048)	(0.0036)	(0.0039)	(0.0039)	(0.0036)
N	180000	180000	180000	180000	180000
F stat First Stage	17.51	17.51	17.51	17.51	17.51
Panel C: OLS	(1)	(2)	(3)	(4)	(5)
China	Manufacturing	Craftsmen	Service	Merchant	Import Manufacturing
Import Exposure	0.0017*	0.0026**	-0.0016**	-0.0014**	0.0033
	(0.0010)	(0.0012)	(0.0007)	(0.0006)	(0.0023)
N	196025	0.1000	196025	196025	196025
R-squared	0.292	180000	0.295	0.286	0.021
Panel D: IV	(1)	(2)	(3)	(4)	(5)
China	Manufacturing	Craftsmen	Service	Merchant	Import Manufacturing
Import Exposure	0.0037**	0.0026**	-0.0030***	-0.0025***	0.0051*
	(0.0017)	(0.0012)	(0.0011)	(0.0010)	(0.0029)
N	180000	180000	180000	180000	180000
F stat First Stage	15.34	15.34	15.34	15.34	15.34

Note: These results refer results for separate regression by Eastern Europe and China, OLS and IV regressions. The treatment in Panel A (OLS) and B (IV) refers to Import Exposure per worker (in 1000 Euro) at county level from Eastern Europe, in Panel C (OLS) and D (IV) from China. The outcome in column 1 is a dummy for whether the occupation an individual enters in in manufacturing, in column 2 a crafts occupation, in column 3 a service, in column 4 a merchant occupation. The outcome in column 5 is a dummy for whether the occupation in a import-intensive manufacturing industry. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) at county level (402 counties) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the county, and dummies for whether the individual is female or non-German. All regressions include state-by-year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* Individual social security (SIAB) data.

Table A5.4 : Evidence on Youths Aspirations and Effects of Parental Occupations

	Adult occupations		Youths' aspirations	
Panel A	(1)	(2)	(3)	(4)
	Manufacturing	Service	Manufacturing	Service
Import Exposure	0.0010	-0.0010	-0.0008	0.0010
	(0.0031)	(0.0023)	(0.0013)	(0.0014)
N	4302	4302	2090	2090
R-squared	0.194	0.207	0.249	0.290

	Adult occupations			
Panel B	(1)	(2)	(3)	(4)
	Manufacturing	Service	Manufacturing	Service
Import Exosure	0.0015	0.0002	0.0032	-0.0022
	(0.0030)	(0.0031)	(0.0028)	(0.0029)
Father Manufacturing	0.1142***	-0.0841***	0.1316***	-0.1081***
	(0.0174)	(0.0181)	(0.0186)	(0.0194)
Imp Exp x Father Manuf			-0.0046**	0.0064***
			(0.0021)	(0.0018)
N	2762	2762	2762	2762
R-squared	0.219	0.214	0.221	0.217

Note: This table presents results from the German Socio-Economic panel. The household survey allows to link families together. In Panel A columns 1 and 2, the outcome refers to the vocational occupation choice of all adults in the survey. The outcomes in Panel A columns 3 and 4 refer to occupational aspirations of 17 year olds in the household. Outcomes in Panel B refer again to adult occupations. Import Exposure treatment is aggregated up to 96 German planning regions. The unit of observation is the individual, observed in the data once. The individual is "treated" by the import shock in the county she is first observed in, in the year she is 15. The treatment refers to import exposure per worker (in 1000 Euro) from both China and Eastern Europe. All regressions also control for the respective export exposure. All regressions control for the following covariates: manufacturing employment in the region, and dummies for whether the individual is female or non-German. All regressions include state and year fixed effects. Robust standard errors clustered on county level in parenthesis. Significance level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Source: German Socio-economic panel data

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Experimental Economics, Paris School of Economics

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PwC Advisory, Internship — Jul 2011 - Oct 2011
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Scholarships

- Bernt Rohrer Foundation Publication Stipend for PhD Students in their final year (add-on stipend of 500 Euro monthly), October 2017-October 2018
- German Academic Exchange Service (DAAD): 9 months scholarship for PhD students visiting a US Department, September 2016 - May 2017

Teaching

- Supervisor for undergraduate students writing Bachelorthesis, winter semester 2017/2018 and 2018/2019 LMU
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Refereeing

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