Individual and Regional Returns to Higher Education: Empirical Evidence for Germany

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Chapter 1

Introduction

Investments in education have on average large individual and social returns and have been found to be one of the major drivers of economic development and growth. Following the pioneering work of Becker (1962, 1964), the economic literature on the returns to education has mushroomed over the past 50 years. Microeconomic studies have found that the average individual wage returns to an additional year of schooling range from 5% in Scandinavian countries to 9% in Anglo-Saxon countries (see, e.g., Harmon et al., 2003). Many studies have focused on accounting for a possible "ability bias" when establishing the causal link between education and earnings. However, studies comparing identical twins or exploiting institutional features of the school system in an instrumental variable setting find estimates that are relatively similar to those of standard OLS earnings regressions (Card, 1999). While some of these earnings returns might simply be due to job market signaling of higher ability (Spence, 1973), empirical research finds very little evidence that signaling plays an important role (Lange and Topel, 2006). Education must thus increase individual productivity. In fact, macroeconomic studies also find that education has large productivity effects. A one year increase in the average education is found to increase GDP per capita by 3 to 6% and GDP growth by 1 to 3 percentage points (see, e.g., Sianesi and Van Reenen, 2003). The overall returns to education are generally larger than private ones, because they might appear as profits rather than wages (Gennaioli et al., 2013). Moreover, research has highlighted that a large portion of the effects of education on economic development can to be attributed to positive human capital externalities.[1] First, human capital might lead to innovations and new technologies that increase economic

[1] Throughout the dissertation I use the term "education" and "human capital" interchangeably, although human capital is in theory a broader concept than education.

growth (Lucas, 1998; Romer, 1990). Second, a higher level of human capital in the economy might increase the sharing of knowledge and make learning from others easier and faster (Moretti, 2004b). Third, educated workers might also increase the pecuniary returns of less-educated workers (Acemoglu, 1996). Social returns to education do not only show up in terms of productivity spillovers, but also in terms of better health and lower crime rates (Lochner, 2011). All these results provide a rationale for the tremendous increase in the level of education across the world. In OECD countries the share of individuals aged 25 to 34 with a tertiary degree increased from 26% in 2000 to 41% in 2014 (OECD, 2015a). The presence of human capital spillovers and positive social returns to education also suggests that governments should subsidize education. While the average public investment in education in OECD countries amounted to 4.8% of the GDP in 2000, by 2012 this figure had increased to 5.2% (OECD, 2015a).

Most studies on the returns to education have treated education as unidimensional. However, as the average level of education has dramatically increased, educational decisions are about the type of education to pursue as well as about the quantity. When deciding whether to increase investment in education, policymakers are typically confronted with trade-offs concerning the allocation of funding to different types of education. Similarly, individuals would like to know on what type human capital they should invest in. Is it better to spend resources on reducing the number of secondary school dropouts or on boosting tertiary education?[2] What are the returns to academic versus vocational education? What are the returns and career options of different fields of study? Many of these questions have still not been sufficiently addressed by economists. While there has been some research on heterogeneous returns depending on school quality, much less is known about the returns of different levels of education (Card, 1999; Sianesi and Van Reenen, 2003). Moreover, only recently there has been a growing body of research on the returns to different university subjects (Altonji et al., 2016). There are typically huge differences in earnings between graduates of different fields of study. For instance, in the US the gap between the most and less well-paid university subjects is as large as the wage differences between university and high school graduates (Altonji et al., 2016).

[2] Clearly such a question might have an impact on inequality and thus requires a political response. Nevertheless, more precise information regarding the returns to different educational investments would allow policymakers to base their choices on actual evidence.

This dissertation aims at contributing to the literature on the returns to higher education by investigating the individual payoffs of different fields of study in Germany, as well as the impact of universities on the labor market and on economic development. Particular attention is devoted to the outcomes of STEM (Science, Technology, Engineering, and Mathematics) education. These fields of study are of special concern for many governments, because of their direct link to innovation and economic growth. Indeed, STEM education is found to lead to larger human capital exernalities than other fields of study (Winters, 2014). In this respect Germany is a very interesting case, since it is the one of the OECD countries with the largest share of graduates in natural sciences and engineering (OECD, 2015b). Many of these graduates study at technical colleges (*Fachhochschulen*), which provide a type of education that is more practice-oriented and includes periods of practical training so that students can gain work experience during their studies. This dissertation focuses on two further aspects. First, not only I do analyze the wage payoffs of education, but also at the match between educational qualifications and job requirements. Second, I devote particular attention to regional disparities and to educational payoffs at the local level.

With an increase in the level of education and in the degree specialization, the effects of education on productivity will very much depend on the extent to which skills acquired through formal education are used on the job. A higher specialization during formal education is likely to increase uncertainty about future payoffs. Given that education is typically acquired before entering the labor market, an unexpected shift in labor demand could lead to a mismatch in the skills acquired by individuals during education and those required on the job. For instance, there is evidence that technological changes have replaced many manual and cognitive routine tasks over the past 30 years (Autor et al., 2003). This has of course affected those workers that specialized in such tasks on the job, but it might be especially harmful to those that also specialized in such skills during their education (e.g. accountants) because of higher investment costs such as foregone earnings and tuition fees. Even in the absence of a major shift in skill demand, labor market frictions such as imperfect information and costly job search might lead to a mismatch between workers' education and job requirements (Sattinger, 2012). If human capital depreciates fast, a job mismatch for a short period of time might have a persistent effect on future earnings and productivity.[3] Thus, the increase in educational attainment and specialization provides a rationale for investigating job match quality, since its importance in explaining wage differences might have increased. Moreover, from the worker's point of view the quality of the job match might also increase utility independently of wages, for instance if individuals have a preference for the subject they have specialized in. Besides wages, I will thus also look at measures of job match quality as outcome variables.

Job match quality can be measured in different ways. Most studies in the education economics literature have focused on the match between qualifications held by workers and job requirements. Job requirements are either based on workers' self-assessments or imputed at the occupational level based on the observed education in the workforce. Measures of overqualification and relatedness of job requirements to completed education are found to be strongly correlated to wages (Leuven and Oosterbeek, 2011; Robst, 2007). Such measures of mismatch have also been found to be negatively associated with job satisfaction (Hersch, 1991). In this dissertation I employ qualification mismatch measures based on workers' self-assessments.

Education is deemed to be one of the main factors of economic development not only at the national level, but also at the regional level. Regions that have invested disproportionately into education, or where advanced educational institutions are established by the national government, experience a higher GDP per capita and larger economic growth compared to other regions within the same country (Gennaioli et al., 2014, 2013). Nevertheless, there is little literature on the impact of different types of institutions, as well as on the channels explaining the economic impact of educational institutions at the regional level. Valero and Van Reenen (2016) find some initial evidence on the importance of the impact of universities on regional economic growth. This might be simply due to the larger supply of skilled workers who are on average more productive. However, there are other channels that are especially relevant at the local level. First, human capital externalities have a geographic dimension and are found to decrease quickly with distance (Fu, 2007; Rosenthal and Strange, 2008). Several papers have documented the presence of important innovation spillovers from university research in terms of commercial patents, which are especially large at the local level (see, e.g., Belenzon and Schankerman, 2013; Jaffe, 1989; Jaffe et al., 1993). Moreover, Kantor and Whalley (2014a) find that universities

[3] Liu et al. (2016) show for instance that graduating during a recession has persistent negative effects due to a higher skill mismatch when graduates enter the labor market.

increase local wages, especially when universities are more research-intensive and firms are technologically closer to universities. Second, higher education institutions might affect regional economic growth through a local multiplier effect. An increase in the supply of high skill labor might increase the demand for local goods and services (Moretti, 2010). These developments are not only likely to increase economic growth, but also population growth through migration from other regions.

There is a strong complementarity between densely populated regions and skills that goes beyond the fact that regions with a higher concentration of skills might attract new migrants and grow in size. The reverse is also true: large cities disproportionally attract high-skilled workers. One reason might be that returns to education are generally found to be larger in urban areas (see, e.g., Becker, 1964). However, while gross wages are higher in cities there is generally no evidence that real wages are higher (Glaeser and Mare, 2001). Urban amenities, such as theaters or good restaurants, might be more valuable to high-skilled individuals (see, e.g., Glaeser and Gottlieb, 2009). It might also be that knowledge spillovers and thick labor markets matter mostly for high-skilled individuals. Gould (2007) finds that there is an urban wage premium in the US only for white-collar workers while there is no premium for blue-collar workers. There is evidence that highly skilled couples locate in big cities because of the likelihood of finding good job matches for both partners (Costa and Kahn, 2000). But it might also be easier for single highly educated individuals to find a job that is a good match for the skills acquired in large cities.

This dissertation consists of three essays. The *second chapter*, coauthored with Ulrich Zierahn, investigates the individual payoffs of different fields of study in Germany. In particular, we compare graduates from STEM subjects to graduates from two other groups, Business & Law and Social Sciences & Humanities. Graduates in natural sciences and engineering are often found to earn higher wages and to have a lower risk of overqualification. However, STEM subjects are also typically associated with more challenging programs, as the drop-out rate is higher in these subjects (see Heublein et al., 2012, for Germany). Moreover, there is evidence from the US that STEM students have better results in both mathematics and verbal pre-college tests (Arcidiacono, 2004). It is thus unclear whether the positive individual returns to STEM tertiary education are to be attributed to the human capital acquired at university or reflect positive individual characteristics of STEM students, such as higher ability, ambition or motivation. We employ an instrumental variable strategy to deal with selection into fields of study. Using data from the German Socio-Economic Panel, we find that selection matters when STEM graduates are compared to the Business & Law group, while it plays only a minor role for the difference between STEM and Social Sciences & Humanities graduates. Workers with a STEM degree are on average less likely to be overqualified compared to graduates from other fields. When controlling for selection, the difference with Business & Law graduates disappears, while the difference with Social Sciences & Humanities graduates gets even larger. Moreover, STEM graduates earn on average similar wages to Business & Law graduates in our sample and about 25% higher wages than Social Sciences & Humanities graduates. The wage difference relative to the latter group persists when controlling for selection. On the contrary, we find evidence that that Business & Law subjects lead to higher payoffs than STEM fields.

Our results hint to a positive selection of students into STEM subjects. This result has important policy implications given that governments are often concerned that the aggregate supply of STEM graduates might be too low. Policies promoting STEM over Business & Law subjects could negatively affect individual wage payoffs. There is evidence that individual preferences and subject-specific abilities play a major role when choosing a field of study (see, e.g. Arcidiacono et al., 2012; Hilmer and Hilmer, 2012; Stinebrickner and Stinebrickner, 2014). Therefore, if more STEM graduates are desired, governments should consider policies focusing on students at a younger age, such as fostering the development of math skills early in school.

In the *third chapter* I investigate regional differences in qualification mismatch depending on the size of the local labor market. There is ample evidence that wages are higher in more densely populated regions (see, e.g. Glaeser and Mare, 2001, for the US; Lehmer and Möller, 2010, for Germany; and Combes et al., 2008, for France). This should reflect some forms of agglomeration economies, such as better matches between workers and firms. Using data from the German Socio-Economic Panel, I find that male workers who live in denser regions are both less likely to be overqualified for their job and less likely to work in a different field than the one they are trained for. The impact on overqualification is robust to the inclusion of an extensive set of control variables and is relatively large. An increase of 10% in the regional employment density is associated with a decrease of

1-1.5% in the overqualification incidence. The impact of horizontal mismatch is slightly smaller and less precise. I then employ different empirical strategies to account for the potential sorting of talented workers into more urbanized areas. I get very similar results for overqualification when I focus on individuals who never leave the place where they grew up, when I estimate fixed-effects regressions obtaining identification through regional migrants, or when I instrument current density with historical population data. Finally, I investigate the extent to which lower qualification mismatch in large agglomerations contributes to the urban wage premium. I find that overqualification accounts for about 6% of the impact of regional employment density on hourly wages, while the contribution of horizontal mismatch appears to be insignificant.

Overqualification entails only one specific aspect of job-worker match, namely the match in formal qualifications obtained through education.[4] Other types of matches have been shown to play an important role, such as assortative matching between highly productive firms and workers (Dauth et al., 2016). With the expansion of higher education, the contribution of job-worker matches to regional wage inequalities might have gained importance.[5] If this is the case, regional disparities in population might grow over time, since workers might have a greater incentive to move to larger cities. There is indeed some evidence that regional disparities in population have grown in West Germany over the past 40 years.[6]. However, it is not clear what policy response is needed to deal with the rising importance of agglomeration economies (Glaeser and Gottlieb, 2008). Subsidizing unproductive areas is not an efficient way to transfer resources to poor people (Kline and Moretti, 2014). After all, national policymakers should care about people not regions. Policymakers at the national level should thus probably accommodate these changes, for instance by ensuring that enough housing is available in growing cities (Glaeser and Gottlieb, 2008). Local policymakers instead might wish to increase regional income and population. Polices that attract or train high-skilled workers might be the most efficient way to achieve those goals (Glaeser and Gottlieb, 2008). The last chapter of the dissertation analyses the local impact of university openings.

[4] Note that skill mismatch is a broader concept than overqualification including, for instance, skills acquired on the job by accumulating experience. Skill mismatch is thus likely to explain a larger part of the urban wage premium. However, skill mismatch is very difficult to measure accurately.

[5] This is a policy relevant topic which would require further research.

[6] Using regional data for West Germany, I estimate that urban regions (as defined by the Federal Office for Building and Regional Planning) have experienced a 15% higher employment growth and a 12% higher population growth between 1980 and 2010.

The *fourth chapter*, coauthored with Christina Gathmann and Verena Lauber, studies the effects of universities on the local economy. In particular, we investigate how the opening of technical colleges in Germany during the 1980s and 1990s has affected local employment and wages of workers with different qualifications using administrative data. Our empirical strategy combines a matching procedure with a time-varying difference-in-differences approach to find suitable control regions for regions that had a college opening. The opening of a technical college substantially increases the regional student population. This results in a large positive shock of high-skilled labor in the district when the first cohorts complete their studies. We find that high-skilled employment in the region increases by 12% eight to nine years after the opening. We also find evidence that new colleges also raise the employment of workers without a college degree suggesting complementarities in the local production function. Moreover, wages of high-skilled workers do not decrease in the medium-run indicating a shift in local demand, especially for high-skilled workers. The employment and wage effects remain if we exclude all employees working in education, suggesting that a college opening has an impact on the local economy beyond additional jobs in teaching and research. The large increase in employment and the lack of a drop in wages point to sizable adjustments on the labor demand side. We find no large increase in employment at new establishments, suggesting that most of the adjustments happen in incumbent firms either through changes in the output mix or through changes in the technology used in the production process.

While our analysis focuses on the short- to medium-run effects of a college opening, the insights gained have important implications for regional policy. First, opening a new college is indeed an effective strategy to increase the skill level of the regional workforce. Second, we do not find evidence of adjustment costs in terms of lower wages or employment of such a large shock in high-skilled labor. Whether establishing new universities is a cost-effective strategy to boost local economic development ultimately depends on the size of the positive human capital externalities in the longer run. This is an important topic for future research.

Chapter 2

Field of Study, Qualification Mismatch, and Wages: Does Sorting Matter?

2.1 Introduction

There is a widespread belief in the public debate that Science, Technology, Engineering and Mathematics (STEM) are important drivers of innovations and play a key role for economic growth (Aktinson and Mayo, 2010). There is also some scientific evidence that the social returns to STEM education exceed the private benefits. For example, Winters (2014) finds that human capital externalities are especially high for STEM graduates. Hence, it is often claimed in western economies that the number of STEM graduates is too low and that policy makers should engage in increasing it.[1] This gave rise to recent policy initiatives for

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[1] For the US, see for example National Academies (2010) and ManpowerGroup (2013). For Germany, see for example the report of Anger et al. (2013) on skilled labour in STEM fields.

promoting STEM fields, for example in the US[2] or in Germany[3]. Since STEM graduates earn on average higher wages (Arcidiacono, 2004; Daymont and Andrisani, 1984; Grogger and Eide, 1995; James et al., 1989) and face a lower risk of overqualification (Dolton and Vignoles, 2000; Frenette, 2004; McGuinness, 2006) than non–STEM graduates, policies pushing pupils to study STEM subjects might also positively affect individual careers.[4] Moreover, such policies could enhance the efficiency of the tertiary education system by providing the graduates who are demanded by the labor market, if these policies would really reduce the risk of overqualification.

The positive effects of these policies on individual careers and on the skill match in the economy critically depend on the assumption that differences in overqualification and wages between subjects are attributable to the subjects studied rather than individual characteristics (sorting). However, there is evidence that STEM subjects are associated with more challenging studies and require higher ability (Arcidiacono et al., 2016; Betts and Morell, 1999; Rask, 2010). This is reflected in higher drop-out rates for students in STEM subjects. For example, the drop-out rate for mathematics and science in Germany is 39% and 48% for engineering, as compared to 24% in economics, social sciences and law.[5] Further, there is evidence from the US that pupils choosing STEM subjects have better results in both mathematics and verbal pre-college tests (Arcidiacono, 2004). Therefore, students in STEM subjects are likely to differ in their personal characteristics, such as ability, as compared to other students. This implies that the effect of the university field of study on wages and overqualification rates cannot be interpreted in a causal way unless controls for all relevant personal characteristics are appropriately included (Altonji et al., 2012). Employing a dynamic discrete choice model for the US, Arcidiacono (2004) finds indeed that most of the differences in wage returns to fields of study decrease after controlling for ability sorting. Policies promoting STEM subjects might thus push pupils into fields of study which do not fit their abilities. This would negatively affect

[2] For example, the National Science and Mathematics Access to Retain Talent (SMART) Grant or the "Educate to Innovate" initiative of the Obama administration.

[3] For example, the program "Komm Mach MINT" seeks to increase the number of female students in STEM fields of study (`http://www.komm-mach-mint.de/`). Other programs, which are often, but not always, publicly sponsored, are listed in the website of the STEM gateway "MINT Zukunft schaffen": `www.mintzukunftschaffen.de`.

[4] Overqualification corresponds to a "vertical" educational mismatch denoting the possession of a higher qualification than the one necessary for the job. In the paper, we use the term skill mismatch and overqualification interchangeably. In particular, we refer to overqualified employees as (skill) mismatched employees and vice versa.

[5] Heublein et al. (2012), numbers for bachelor students based on the alumni year group 2010 in Germany.

the efficiency of these policy measures concerning the aim of providing adequate STEM graduates. Moreover, the overall effects of such policies on individual careers are unclear and could be even adverse.

Against this background, the present paper tests the hypothesis that higher wages and lower risk of overqualification of STEM graduates compared to other graduates are at least partly driven by differences in unobserved characteristics. We compare STEM graduates to graduates of Business & Law subjects and of Social Sciences & Humanities. The analysis is based on data from the German Socio–Economic Panel (GSOEP) for employed male graduates, which includes detailed information about the current job, such as a subjective evaluation of the required qualifications, and information on parental background and the educational career. To ensure that the results are not affected by the transition process from university to the labor market, we rely on employed individuals who graduated at least five years prior to the survey. We use an instrumental variable approach to control for the selection of individuals into subject groups when estimating the effects of the subjects on wages and the risk of overqualification. Our exclusion restrictions are the difference in mathematics and German grades from the last school report (using average school grades as a control variable) and a binary variable indicating whether the individual played a music instrument in youth. Making use of linear and non-linear IV techniques, we find that selection matters for differences in the risk of overqualification and wages when STEM graduates are compared to the Business & Law group, while it plays only a minor role for the difference between STEM and the Social Sciences & Humanities graduates.

The rest of the paper is organized as follows. In Section 2.2 we provide a concise review of the literature. In Section 2.3 we discuss the role of sorting into subjects and how we take account of sorting in our econometric specification. In Section 2.4 we describe our data and we provide definitions, as well as summary statistics, for the variables included in the analysis. Section 2.5 contains the results of our estimations, while Section 2.6 concludes.

2.2 Literature Review

The approach of this paper is motivated by Job Assignment Models (c.f. Sattinger, 1993). In those models there exist different types of workers and different types of jobs. Workers choose jobs based on utility maximization and there is a certain technology which links workers with specific characteristics to jobs with specific characteristics. The quality of the match between workers and jobs affects the work productivity and thus leads to different labour market outcomes. There is growing empirical evidence in favor of Job Assignment Models (McGuinness, 2006). While the empirical literature building on these models usually focuses on the wage effects of overqualification, we are interested in the effects of worker characteristics on the quality of job matches. In particular, we are interested in how the field of study affects overqualification and wages as two key aspects of the quality of job match. We interpret them as outcomes of the assignment problem. There exist two strands of literature, which already deal with these issues.

The **first** strand of literature has grown rapidly in recent years with an increasing attention towards the variation of economic returns to university education by field of study. These studies provide evidence that STEM graduates earn higher wages than graduates in art, humanities and social sciences (Arcidiacono, 2004; Daymont and Andrisani, 1984; Grogger and Eide, 1995; James et al., 1989; Robst, 2007). The differences in the returns to higher education across subjects are found to be substantial and possibly larger than differences in returns to college quality (James et al., 1989). However, most studies do not account properly for selection into fields of study and simply use OLS with a large set of control variables. Selection might be a substantial problem, since students are likely to choose particular subjects based on the heterogeneity of returns. Moreover, omitted variables that influence both the choice of the fields of study and earnings may also lead to biased estimates. It is therefore not clear whether we can consider in this case OLS estimates as estimates of the causal impact of university subjects (Altonji et al., 2012). In particular, there is evidence that individuals choosing STEM fields perform better in both cognitive and verbal tests than individuals choosing other fields (Arcidiacono, 2004). Thus, OLS estimates are likely to overestimate the earnings returns to STEM subjects. Only few analyses attempt to address selection into fields of study. Webber (2014) uses a simulation approach and various assumptions about selection on unobservables to argue that large disparities in lifetime earnings between fields of studies remain even after addressing selection. Arcidiacono (2004) employs a dynamic discrete choice model and finds that most of the differences in wage returns to fields of study persist after controlling for selection, albeit they decrease in size. While structural models are very valuable to understand how individuals make sequential educational choices and can account for educational costs, they generally impose strong simplifications. In their recent review of the literature Altonji et al. (2012) state that "given the complexity and pitfalls of estimation based on dynamic structural models, we expect careful studies using IV strategies or OLS with rich controls to continue to play a critical role in the literature going forward". To the best of our knowledge, the only papers using an instrumental variable or a regression discontinuity approach to deal with selection into fields of study are Berger (1988) and Hastings et al. (2013). While Berger is mainly interested in estimating the impact of expected future earnings on subject choice, the author investigates also the wage returns to fields of study in the US. He focuses on five subject groups and uses some individual and family background characteristics, such as father's occupation, ethnic origin and parental education, as exclusion restrictions in the subject choice equation. However, the validity of these instruments might be questioned, since it is likely that family background characteristics could affect skills and earnings directly and not only through the subject choice channel. Hastings et al. instead estimate the returns to fields of study in Chile and use the thresholds in the centralized Chilean admission system to apply a regression discontinuity approach. They find large and significant differences in returns by field of study with higher returns for business, law and technical fields than for arts, architecture and humanities.

Apart from earnings returns, graduates in STEM fields seem to be also better off with respect to non–wage labour market outcomes than their peers graduating in other subjects. There is a general public perception that these subjects enable a better link to the labour market and that the skills provided by these curricula are more useful for future jobs. The **second** strand of literature therefore investigates how skill mismatch for graduates varies across fields of study (see Berlingieri and Erdsiek, 2012, for a survey of the literature on skill mismatch in Germany). Most of these studies have focused on the discrepancy between the type of qualification (i.e. possessing a higher qualification than required) among graduates of different fields of study (Dolton and Vignoles, 2000; Frenette, 2004; Green and McIntosh, 2007).[6] Except for graduates of some specific majors, such as education or medicine, for whom this type of mismatch is close to zero, graduates majoring in STEM fields appear to have a lower risk of overqualification than graduates in humanities and social sciences (Büchel and Matiaske, 1996; Dolton and Vignoles, 2000;

[6] Another literature investigates the role of fields of study for horizontal mismatch, i.e. mismatch between the content of the field of study and occupational content (see for example Robst, 2007).

Fehse and Kerst, 2007; Frenette, 2004; McGuinness, 2006). Similarly to most of the studies on wage returns to university subjects, these studies focus on estimates from simple logit or probit regressions. However, the potential bias is likely to be larger for studies on qualification mismatch, since they typically fail to include fundamental control variables, such as high school grades. In fact, high school grades are found to be key predictors of both university subject choice and earnings (Rose and Betts, 2004). Since overqualification is typically associated to lower earnings, failure to take into account high school grades (or other proxies for ability) is likely to lead to an overestimation of the differences in job mismatch across fields of study. Moreover, to the best of our knowledge, no previous study on overqualification has tried to address directly the selection problem. It is still not clear if the cross–subject differences in qualification mismatch persist after controlling for the selection into different fields of study and, if so, how large is the bias when predictors of subject choice are omitted.

Further, as this paper addresses the role of sorting into subjects, it is related to studies which focus on the role of individual characteristics for the choice of the field of study. In the literature, individual characteristics such as gender and parents education (Boudarbat and Montmarquette, 2009), tastes and motivations (Hilmer and Hilmer, 2012) or expected earnings (Arcidiacono et al., 2012; Beffy et al., 2012; Freeman and Hirsch, 2008) have been found to be important predictors for subject choice. Notably, differences in ability affect the subject choice of college majors (Turner and Bowen, 1999). Moreover, Arcidiacono et al. (2012) show that students subject choice depends on the subject-specific abilities of the individuals. This suggests that differences in labor market outcomes between graduates of different subjects might be affected by the differences in omitted or unobserved individual characteristics.

In summary, graduating in a STEM subject is related to on average higher wages and a lower risk of overqualification. We hypothesize that this relationship is partly driven by sorting into fields of subject. That is, we expect that, if differences in (unobserved) individual characteristics are taken into account, the positive effect of graduating in STEM versus other fields on wages and the negative effect on the risk of overqualification decrease.

2.3 Econometric Methods

In this paper, we apply instrumental variable (IV) techniques to control for the selection into subjects when estimating the effects of graduating in a subject on the risk of overqualification and wages. We first present the approach of our paper for modeling the probability of overqualification and wages, before we discuss our choice of instrumental variables. Finally, we present the implementation of our approach.

2.3.1 Approach

The aim of this paper is to estimate the effect of fields of study on wages and the probability of overqualification for graduates. Ideally, we would estimate the probability of an individual to work in a matched vs. mismatched job with a probit model,

$$Pr(\text{overqualified}_i = 1) = Pr(\beta X_{1,i} + \sum_{j=1}^{2} \phi_j \text{subject}_{ji} + \epsilon_i > 0) \; ; \; j = 1, 2 \tag{2.1}$$

where overqualified indicates whether individual i is overqualified (0 for non–overqualified and 1 for overqualified) and subject_{ji} is a dummy variable equal to 1 if the individual i graduated in the university subject j, taking into account other relevant covariates $X_{1,i}$. We distinguish between three groups of subjects j: STEM ($j = 0$) as the base category, Business & Law ($j = 1$) and Social Sciences & Humanities ($j = 2$).

Analogously, we are interested in the effect of the field of study on wages, using log wages as the dependent variable. Ideally, we would estimate log wages in a linear specification:

$$\log \text{wage} = \delta X_{1,i} + \sum_{j=1}^{2} \varphi_j \text{subject}_{ji} + \varepsilon_i \; ; \; j = 1, 2 \tag{2.2}$$

The key problem for analyzing the effects of subjects on wages and overqualification is that the choice of the subject itself depends on observable and unobservable characteristics of the individuals. Assume that the utility U_{ji} of individual i to study subject j is a function of observable characteristics $X_{2,i}$ and unobservable characteristics η_{ji}, $U_{ij} = \theta_j X_{2,i} + \eta_{ji}$. Individual i will study subject j when his utility from studying this subject is higher than the utility from any other subject k and the probability that he will study subject j is

$$Pr(\text{subject}_{ji} = 1) = Pr(\theta_j X_{2,i} + \eta_{ji} > \theta_k X_{2,i} + \eta_{ki}) \ ; \ j \neq k \tag{2.3}$$

where $X_{2,i}$ represents covariates which influence the subject choice and might overlap with X_1.[7] There might be unobserved characteristics of the individuals that influence both, the choice of the subject and the probability of a mismatch resp. wages. For example, individuals choosing STEM university subjects might have on average a higher ability than individuals choosing other subjects. There might also be other unobserved characteristics which could affect both, the choice of the field of study and labor market outcomes, such as motivation or ambition. These qualities are highly rewarded in the labour market and potentially decrease their probability of overqualification in the job resp. increase their wages. If this is the case, there will be a non–zero correlation between the error–terms of the equations, i.e. between η_{ji} and ϵ_i resp. ε_i.[8]

Then subject$_{ji}$ contains η_{ji}, which is correlated with ϵ_i and ε_i. Therefore, the estimation of the effect of subjects on the risk of overqualificaton resp. wages is inconsistent for β_1 and ϕ_j resp. δ_1 and φ_j. Hence, subject$_{ji}$ is a multinomial endogenous variable. In order to account for the potential endogeneity, we apply instrumental variable techniques. Through the instrumental variable approach we can estimate the effect of the fields of study on labor market outcomes excluding the effects of unobserved heterogeneity of the graduates. By comparing the estimates with and without the instrumental variables, we can visualize to what degree differences between fields of study are driven by unobserved heterogeneity. However, the precise underlying mechanisms cannot be identified. For example, unobserved heterogeneity might affect labor market outcomes through differences in ability, motivation or ambition between the graduates of different fields of study. Even though we cannot identify the precise underlying mechanisms, by distinguishing between

[7] In the analysis we include in $X_{1,i}$ and $X_{2,i}$ demographic characteristics, family background and educational background characteristics. The two matrices differ with respect to two instrumental variables, which are included only in $X_{2,i}$. The detailed list and description of the variables included in the analysis is presented in Section 2.4.

[8] The sign of the correlation depends on the definition of the reference groups. In our case, graduating in STEM and being non–overqualified are the reference groups, so that we expect a positive correlation between η_{ji} and ϵ_i. This is because we expect that unobservables which are associated with a higher probability to graduate in STEM subjects are also associated with a lower probability of being overqualified. Analogously, we expect a negative correlation between η_{ji} and ε_i because we expect that unobervables which are associated with a higher probability to graduate in STEM subjects are associated with higher wages.

the effects of unobserved heterogeneity and the direct effects of the fields of study, we can analyze whether policy measures aimed at improving individuals' labor market outcomes should focus on pre-university characteristics or on directly altering the relative number of students in different subjects. Further, we can discuss how promoting STEM subjects might affect labor market outcomes of those who are pushed in these subjects.

2.3.2 Instrumental Variables

The key problem for our empirical analysis is that unobservable factors, such as ability, most likely not only affect the individual risk of overqualification and individual wages, but also the probability of graduating in a STEM vs. other subjects. We control for a wide range of individual characteristics. Further, we partly control for ability by using average school grades as a proxy. However, school grades only partly reflect ability, so that the unobserved variation of ability (which is not covered by school grades) still can lead to biased estimates for the effects of graduating in specific subjects. Moreover, there might be other unobservable characteristics which are linked to both, the choice of subjects and labor market outcomes, such as motivation or ambition. To address the selection problem, we employ two instrumental variables for the subject choice.

Our first instrumental variable is the difference of mathematics and German grades in the last year of school. We argue that, once we control for the average of high school grades in German and math as well as for other observables, the difference in grades has an effect on the job match and on wages only through the university subject. For example, assume that two individuals have the same overall ability, i.e. the same average math and German grades. The individual who has relative better math than German grades is more likely to choose a STEM subject, as this individual is likely to be more interested and has a comparative advantage in STEM topics. We argue that his comparative advantage in math does not have per se an effect on labour market outcomes. This might seem a strong assumption considering that there are studies stressing the role of high school mathematics grades on future earnings (Joensen and Nielsen, 2009; Murnane et al., 1995) and that, at least for the US, math courses might be more important for labour market outcomes than English courses (Rose and Betts, 2004). We conduct a series of robustness checks and informal tests to analyze whether this issue might be relevant in our case and to investigate if and in which direction our IV estimates might be biased. We conclude that, when controlling for broad field of study groups, mathematics grades do not have stronger effects on labour market outcomes than German grades. This is in line with novel evidence on returns to skills by Hanushek et al. (2015), who find that, contrary to the US, monetary returns to mathematical skills and to literacy skills are very similar in Germany. We are therefore convinced that the first instrument is valid.

Our second instrumental variable is a binary variable indicating whether the individual played a music instrument or was involved in other music activities in youth. Our argument is analogous to the above discussion. An individual, who played an instrument in youth is likely to have different interests compared to other individuals, hence choosing other subjects in university — holding constant all other variables. Once we control for average high school grades and other observables, such as family background characteristics, we argue that playing versus not playing an instrument affects wages and job match only through the university subject. Again, this might seem a strong assumption as there is literature arguing that playing an instrument in youth relates to outcomes such as skills, personality or educational achievement, even though such analyses usually do not detect causal effects (Hille and Schupp, 2015). Schellenberg (2004) provides evidence for a causal, albeit very small, effect on educational outcomes and IQ, while Hille and Schupp (2015) provide evidence for causal effects on skills, school grades and personality. Nevertheless, those effects are measured against the alternative of no extracurricular activity and for samples drawn from the whole population. As Schellenberg (2004) notes, other extracurricular activities might have very similar effects. Note that we focus on university graduates only, who are a homogeneous group, and that we rely on a rich set of covariates. If the alternative of playing an instrument is another activity with similar effects on skills, then differences in skills between those who played an instrument in the youth and those who did not are likely to be small and ambiguous. In a sample of graduates, who typically come from families with a higher social status, such as in our sample, this is more likely to be the case. Hence, we believe that there is only little room left for the variable "played an instrument in youth" to directly affect the remaining variation in overall ability, as we control for the most relevant variables which affect labour market outcomes and as we solely focus on graduates. Anyhow, we check whether the variable is a valid instrument using several strategies. First, we restrict our analysis to bivariate comparisons of subjects (STEM versus Business & Law and STEM versus Social Sciences & Humanities), so that we can check whether the two instruments are invalid using overidentification tests. The tests indicate that the instruments are not invalid. Second, we apply analogous robustness checks and informal tests as for the first instrument to analyze the validity of the second instrument. All tests suggest that the second instrument does not directly affect labour market outcomes when controlling for a rich set of control variables and broad field of study groups. We are therefore convinced that both instruments are valid.

2.3.3 Implementation

In order to implement our empirical approach, we rely on a probit specification for the risk of overqualification in equation (2.1) and a linear specification for log wages in equation (2.2). The key challenge is to account for the endogeneity of the subject choice. We therefore estimate the subject choice in equation (2.3) as a multinomial probit model. We then have two econometric models, one for subject choice and overqualification and one for subject choice and wages.

Having a probit specification for the risk of overqualification and a multinomial probit specification for the subject choice, we assume that the errors of both parts of the model follow a multivariate normal distribution, so that we can estimate a joint model of subject choice and overqualification using simulated maximum likelihood. Analogously, if we rely on a normally distributed error for our wage equation, we can estimate wages and subject choice in a joint model using simulated maximum likelihood. We rely on the Geweke-Hajivassiliou-Keane (GHK) algorithm for implementing the simulated maximum likelihood of these two mixed-process (MP) models (Geweke, 1989; Hajivassiliou and McFadden, 1998; Keane, 1994).

The two implementations are recursive models, where the endogenous variable of the multinomial probit part (the subject choice) appears as an explanatory variable in the probit part (risk of overqualification) resp. the linear part (log wages). Only the equation for the risk of overqualification resp. wages is fully specified. For the subject choice equations, we rely in both cases on instrumental variables to address the endogeneity problem. Hence, we apply a limited-information maximum likelihood estimator for the two models (Roodman, 2011).

In the multinomial probit model for the subject choice, we choose STEM as the base category, so that we can compare how choosing Business & Law or Social Sciences &

Humanities over STEM affects wage and overqualification.[9] This implies that the base category is not included in equations (2.1) and (2.2) and that we use $\theta_0 = 0$ to define the base category in the multinomial probit in equation (2.3). The error terms in the multinomial probit equation are assumed to be independent and identically distributed implying that the relative probability of choosing one subject over another is assumed to be independent of the availability of the third subject. We impose this assumption, which is generally called independence of irrelevant alternatives (IIA), because we do not have alternative-specific variables.

As we have two endogenous variables (in both, the model for wages and overqualification), we require two instrumental variables. We compare our main specifications, the MP models, with corresponding single-equation models for the risk of overqualification (probit model) and wages (ordinary least squares, OLS). We do so in order to compare how taking account the endogeneity affects the results, so that we can visualize the role of sorting into subjects for wages and overqualification.

Actually, we do not need any instruments to technically identify these models, since the non–linearity is already sufficient for technical identification. Moreover, the non–linearity will contribute to the identification of the model even if we do include instruments, such that it is hard to distinguish whether identification is due to the instruments or the non–linearity (Altonji et al., 2005). We therefore compare our basic specification to linear specifications where the identification solely relies on the instruments. In particular, we model the probabilities of choosing Business & Law versus STEM resp. Social Sciences & Humanities versus STEM as individual linear probability models (LPMs).[10] Based on these, we apply two-stage least squares (2SLS) approaches for estimating the effects of the choice of subjects on wages and the risk of overqualification (with a LPM-specification for overqualification).

We compare these implementations to other specifications to check the robustness of our results. First, we compare our results to the approach proposed by Deb and Trivedi (2006,

[9] Choosing STEM as the base category might seem counterintuitive, given that we are mainly interested in the results for STEM. However, note that if we would choose e.g. Business & Law as the base category, we were unable to discuss how choosing Social Sciences & Humanities instead of STEM (or vice versa) would affect wages and overqualification. Therefore, in order to discuss how choosing STEM versus Social Sciences & Humanities and STEM versus Business & Law affects wages and overqualification, we have to choose STEM as the base category.

[10] In addition to the strong assumptions implied by the LPM, this specification further treats the two subject choices as independent processes. Therefore, this implementation does not take into account the correlation of the two subject choice equations.

2009), where the correlation of the error terms of the multinomial treatment equation (subject choice) and the outcome equation (overqualification resp. wages) is modeled by introducing a latent variable which enters both parts of the model (henceforth DT). Second, for wages we apply the two-step procedure proposed by Wooldridge (2010, p.939), where we use the predicted probabilities from separate probit models for the subject choices (Busines & Law versus STEM and Social Sciences & Humanities versus STEM) as instruments in the outcome equation.[11] The advantage of this IV estimator (henceforth ATEIV) is that it is more efficient than the standard 2SLS, if the probit model is a better approximation for the first stage than the linear model (Newey, 1990).

2.4 Data and Descriptive Statistics

2.4.1 Data Source and Key Variables

The sample used is drawn from the German Socio–Economic Panel (GSOEP), a panel data set for the years 1984-2011 consisting of about 20,000 individuals living in Germany (see Kroh, 2012, for details). We focus on highly educated males surveyed in the years 2001 to 2011, for whom there is information about the subject of their tertiary degree. We restrict the analysis to male graduates, since female labour market participation in Germany is strongly influenced by child care and family responsibilities. The investigation of females therefore requires a different econometric approach that takes into account selection out of the labour market. On the contrary, more than 96% of male graduates under 55 in our SOEP sample are employed.[12] The 11 GSOEP waves include 4,081 male adults aged between 26 and 65 with a university degree (including universities of applied studies) and for 2,252 there is information on the field of study.[13] Of these, 2,064 are employed in one of the 11 waves. We select one observation per individual such that the time since graduation is minimized, but is at least 5 years. Moreover we drop individuals graduating before the age of 20 or after the age of 35, because the likelihood to obtain a

[11]This implementation, just like the LPM models from above, does not take into account the correlation of the two subject choice equations.

[12]Of these, less than 2% are respectively unemployed and non–employed and the shares differ only slightly across the three subject groups considered. Because of early retirement provisions, the share of employed workers is lower for male graduates aged 55 to 65. Table 2.A.6 shows that our main results are not driven by this age group.

[13]Note that the high number of lost observation is mainly due to the fact that this type of information was not asked in the biography questionnaire before 2001

university degree at a later stage of life might be higher for particular subject groups and we want to hinder that this could possibly affect our results. We end up with a sample of 896 individuals, for whom we have information on all variables relevant for our analysis. Most of the reduction of the sample is due to missing values for high school grades, which are available for about 75% of employed graduates.[14]

To analyze differences across fields of study, we divide graduates into three broad groups: STEM, Business & Law and Social Sciences & Humanities. Graduates in the fields of medicine and education (or teaching) are omitted from the analysis, because of the specificity of the link between education and occupation.[15] We consider as STEM fields mathematics, natural sciences (physics, chemistry and biology), computing, engineering and architecture.[16] The Business & Law group comprises law, management, public management, managerial engineering and economics. All other subjects including other social sciences, arts and humanities are grouped in the Social Sciences & Humanities category. Table 2.A.1 provides a detailed overview of the fields of study included in each subject group category.

Overqualification is measured based on workers' self–assessment about the educational requirement of the job. More precisely, the following question is asked in the GSOEP questionnaire: "what type of education or training is usually necessary for this type of work?" We consider an individual to be overqualified if a graduate reports that her job requires a vocational degree or a no degree at all.[17] The measure, which is widespread in the overeducation literature, has the drawback of relying on the subjective individual self–assessment. Nevertheless, several authors have claimed that the measurement errors are probably less severe for this measure than for measures based on the distribution of educational qualification within occupations – i.e. "realized matches" on the qualification required by the job. This is because the latter is the result of demand and supply forces

[14]Again, this is because this information was only asked in the biography questionnaire starting in 2001. Therefore, we do not have information for individuals entering the GSOEP before 2001 if they already completed high school.

[15]In Germany, students graduating in medicine and teaching have to take a state examination at the end of their studies. For each discipline, these state examinations are a prerequisite for holding a civil service job or a job regulated by the state. Since graduates of these subjects cannot be overqualified if they act within their profession, they face a very low overqualification risk.

[16]We follow the classification provided by the German Institute for Employment Research (IAB) for MINT subjects (the German acronym for STEM). See for example, IAB (2010). Re-defining architecture as a Social Sciences & Humanities field does not qualitatively alter the results.

[17]Note that we do not distinguish between university and university of applied science (*Fachhochschule*) degrees, although the variable allows such a distinction.

and it ignores variation in required schooling across jobs within an occupation (Leuven and Oosterbeek, 2011).

Hourly wages are measured through the self-reported monthly gross income divided by monthly working hours. We calculate real wages based on the CPI deflator using 2010 as the base year. In order to ensure that outliers are not driving the main results we trim wages excluding the 1st and the 99th percentile (individuals receiving a hourly wage lower than EUR 5 or higher than EUR 100) and we employ the standard logarithmic form for the wage regressions.

Concerning high school grades, we have data on the mathematics and the verbal (German) score from the last school report. These two subjects are the only compulsory courses for the high school diploma in most federal states in Germany. Grades are measured using the 6 points scale typical for the German system. We reverse the order of the grades in order to ease the interpretation of the regression results, so that 6 is the highest grade and 1 the lowest. We construct the variable *Grade:average*, which equals the individual average of the two grades, and the variable *Grade:difference*, resulting from subtracting math grades from verbal ones. The latter will be positive for students with a comparative advantage in math, negative for those better in German and equal to zero for students receiving the same grade for both subjects.

The school grades play an important role for the entrance of pupils into the university system in Germany. At the end of school education in the upper secondary level, pupils can earn the Abitur or Fachabitur, which qualifies them for general (Abitur) or subject-specific (Fachabitur) higher education entrance. University students typically have a general higher education entrance qualification, whereas the share of subject-specific higher education entrance qualification is higher for students at universities of applied sciences (Fachhochschule). In specific subjects where the number of applicants typically exceeds the number of available places at universities, the allocation of places to applicants is centrally organized at the national level and based on the final grade of the Abitur. Students whose final Abitur grade is not sufficient can queue for a place at a university whereas their queuing time depends on their grade. Universities also have individual university-specific entrance limitations for other subjects, which are typically based on the final Abitur grade and which are specific to the subjects. In these subjects, individuals require a minimum grade (numerus clausus) for registering as a student. For many subjects, however, no entrance limitation exists and anyone with a higher education entrance qualification can register as a student.

2.4.2 Descriptive Results

Table 2.1 presents the mean and standard deviation for relevant variables. STEM subjects represent the largest field of study group (53%) followed by Business & Law (31%) and Social Sciences & Humanities (16%). The sample composition across fields of study reflects the fact that the STEM subjects are typically male-dominated subjects, differently from subjects of the other fields. 34% of the sample studied at an university of applied science (*Fachhochschule*, abbr. *FH*) for their highest degree. More than two thirds of graduates have an 'Abitur' high-school degree, which allows direct access to university. The great majority of those who don't have such a degree graduated then from an university of applied science (meaning that the variables 'Abitur' and 'FH degree' are negatively correlated). About 16% have a high-school degree providing direct access only to universities of applied sciences (Fachhochschulreife). Almost 30% of graduates did a professional apprenticeship, which is done in general before starting university. FH graduates are more likely to have done such an apprenticeship. Instead of actual experience we include potential experience, specifically time since graduation, which is independent from the unemployment spells. The average time since graduation is 19 years, while the average age is 46 years. The majority of the individuals was born in the 50s and the 60s.

Table 2.2 shows summary statistics of the dependent variables and other main variables by the three field of study groups employed. In our sample graduates from the Business & Law group earn on average slightly more than STEM graduates. Graduates from Social Sciences & Humanities have on average the lowest earnings. Concerning overqualification, STEM graduates face a lower risk of being mismatched (12%) followed by the Social Sciences & Humanities (14%) and Business & Law (18%). As regards average grades from the last school report, STEM graduates received on average better grades than the other two groups. This supports our hypothesis that STEM graduates might have on average higher ability, meaning that there is some ability sorting into fields of study. The figures on the difference between math and German grades anticipate the results of the first stage regressions. STEM graduates had a comparative advantage in math at school, while Social Sciences & Humanities graduates had a comparative advantage in German. Graduates of

Table 2.1: Summary Statistics

	mean	sd	min	max
Subject group				
STEM fields	0.53	0.50	0	1
Business & Law	0.31	0.46	0	1
Social Sciences & Humanities	0.16	0.36	0	1
Dependent variables and other main variables				
Real hourly wage (log)	3.15	0.45	1.55	4.54
Overqualified	0.14	0.35	0	1
Grade: average	4.68	0.72	2.5	6
Grade: difference	0.26	1.08	-4	4
Played music in youth	0.37	0.48	0	1
Demographic characteristics				
Christian	0.60	0.49	0	1
Migration background	0.07	0.25	0	1
Parental education and employment				
Father: higher educ.	0.28	0.45	0	1
Mother: higher educ.	0.09	0.29	0	1
Mother non employed (age 15)	0.45	0.50	0	1
Pre-graduation characteristics				
FH degree	0.34	0.48	0	1
Apprenticeship	0.27	0.44	0	1
University access (Abitur)	0.69	0.46	0	1
FH access (Fachhochschulreife)	0.16	0.36	0	1
Birth year				
Born before 1950	0.19	0.40	0	1
Born in the 1950s	0.32	0.47	0	1
Born in the 1960s	0.29	0.46	0	1
Born in 1970 or after	0.19	0.39	0	1
High school federal state				
Schleswig-Holstein & Lower Saxony	0.11	0.3	0	1
Hamburg & Bremen	0.03	0.18	0	1
North Rhine-Westphalia	0.21	0.41	0	1
Hesse, Rhinel.-Palatinate & Saarl.	0.13	0.34	0	1
Baden-Wuerttemberg	0.14	0.34	0	1
Bavaria	0.14	0.34	0	1
Berlin	0.02	0.12	0	1
East Germany or outside Germany	0.22	0.41	0	1
Post-graduation characteristics				
Married	0.90	0.29	0	1
Urban region	0.56	0.50	0	1
Job in West Germany	0.70	0.46	0	1
Time since graduation	19.15	9.92	5	43
Time since grad. squared	465.3	404.3	25	1849
Survey year				
Surveyed in 2001 or 2002	0.27	0.44	0	1
Surveyed in 2003, 2004 or 2005	0.33	0.47	0	1
Surveyed in 2006, 2007 or 2008	0.15	0.36	0	1
Surveyed in 2009, 2010 or 2011	0.25	0.43	0	1

The summary statistics refer to the final sample of 896 observations.

the Business & Law group had on average very similar German and math school grades. The Social Sciences & Humanities group presents the highest share of individuals playing an instrument or being involved in other music activities in youth (47%), followed by STEM and Business & Law graduates (37% and 33% respectively).

Table 2.2: Relevant Variables by Field of Study

	STEM		Business & Law		Social Sciences & Humanities	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Real hourly wage (log)	3.16	0.44	3.24	0.47	2.94	0.43
Overqualified	0.12	0.33	0.18	0.38	0.14	0.35
School grade: average[a]	4.82	0.7	4.54	0.73	4.54	0.73
School grade: difference	0.56	0.99	0.01	1.1	-0.27	1.05
Played music in youth	0.37	0.48	0.33	0.47	0.47	0.5

[a]Note that we reverse the order of grades typical to the German system, so that 6 is the highest and 1 the lowest grade.

2.5 Results

2.5.1 Impact of University Subjects on Overqualification

Our first aim is to investigate the impact of the field of study on the probability of being overqualified. To do so, we explicitly model the subject choice to address the selection into the three broad subject groups considered. We include the difference in math and German high school grades and whether the individual played a musical instrument in youth as instrumental variables for the subject dummies (see Section 2.3). In order to highlight the relevance of modeling the sorting into subjects, we compare the results of the instrumental variable model with a simple linear probability model and a probit model for the probability of overqualification where we do not account for sorting.

The results from the overqualification regressions are shown in Table 2.3. The first two columns present the results of the linear probability model, where the overqualification dummy is regressed on the fields of study dummies and a large set of demographic, family background, pre-graduate education and geographic control variables. We show the coefficients for the subject groups Business & Law and Social Sciences & Humanities, leaving STEM fields as the comparison group. While the specification in column (2)

Table 2.3: Effect of Fields of Study on the Risk of Overqualification

	LPM		MP LPM[a]	Probit	MP Probit[b]
	(1)	(2)	(3)	(4)	(5)
Panel A: Overqualification equation					
Business & Law	0.084***	0.075***	0.021	0.068***	-0.075
	(0.027)	(0.027)	(0.035)	(0.025)	(0.081)
Social Sc. & Humanities	0.057*	0.046	0.500***	0.045	0.210*
	(0.034)	(0.034)	(0.034)	(0.033)	(0.114)
Grade: average		-0.045**	-0.026	-0.048***	-0.051***
		(0.018)	(0.020)	(0.016)	(0.019)
Demographic charact.	Yes	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes	Yes
Panel B: Subject choice equation					
Business & Law fields					
Grade: difference			-0.120***		-0.122***
			(0.017)		(0.017)
Played music in youth			-0.084**		-0.086**
			(0.040)		(0.039)
Grade: average			-0.074**		-0.076***
			(0.029)		(0.028)
Social Sc. & Humanities					
Grade: difference			-0.098***		-0.116***
			(0.015)		(0.015)
Played music in youth			0.048		0.043
			(0.032)		(0.034)
Grade: average			-0.075***		-0.076***
			(0.024)		(0.023)
Control variables					
Demographic charact.			Yes		Yes
Parental education			Yes		Yes
Pre-graduation charact.			Yes		Yes
Post-graduation charact.			Yes		Yes
School state dummies			Yes		Yes
Observations	896	896	896	896	896
R-sq.	0.111	0.118			

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01. The coefficients in Panel B and in columns (4) and (5) are average marginal effects. [a] MP model with linear specification for the overqualification equation; [b] MP model with probit specification for the overqualification equation.

includes the average of math and German grades from the last school report, this variable is omitted in the model of column (1). Graduates in STEM fields appear to be less likely to be overqualified than graduates in other fields. According to the first specification the coefficient is of about 8% for Business & Law graduates (significant at the 99% confidence level) and of about 6% for Social Sciences & Humanities graduates (significant at the 90% confidence level). The point estimates decrease by about 1 percentage point in the second specification, when we control for school grades. High school grades have a negative and significant effect on the overqualification probability and appear to be an important omitted variable in the first specification. Since graduates in STEM fields had on average better grades at school, the difference in the overqualification risk with respect to other graduates decreases when grades are controlled for.

Column (3) of Table 2.3 shows the results of the instrumental variable model with a linear probability model for the overqualification equation. Panel A shows again the coefficients for the main variables from the overqualification equation. Panel B shows the coefficients of the instrumental variables and the average school grades for the subject choice equation. The coefficients in Panel B are average marginal effects. In Section 2.4 we already anticipated the relationship between the instrumental variables and the subject dummies. Panel B shows that this relationship is strong also in the model including control variables (i.e. the other instruments). First of all, the coefficients for the difference in grades are negative and strongly significant for both Business & Law and Social Sciences & Humanities. This means that individuals with a comparative advantage in mathematics are more likely to choose STEM subjects than the other two subject groups. Second, the coefficient for playing music in youth is negative and significant for Business & Law and positve but not significant for Social Sciences & Humanities, meaning that individuals playing music in youth are less likely to choose Business & Law subjects and are more likely to choose other Social Sciences & Humanities subjects. Third, the average of mathematics and German high-school grades has a strong impact on the field of study chosen. Students with higher grades select themselves into STEM fields. We test the two instruments for joint significance in the multinomial probit model using the likelihood ratio (LR) statistic. The test statistic is of 81, which is much larger than the 99 % critical values for $\chi^2(4)$. Thus the result confirms that the instruments are relevant.[18]

[18]We also estimate a simple 2SLS model considering the two subject dummies as independent variables in order to compute the Angrist and Pischke multinomial F-test of excluded instruments (Angrist

Turning to the results of the subject equation (Column (3), Panel A) the coefficient for Business & Law graduates decreases now to 2% and becomes insignificant. It appears thus that the differences between graduates of this group and STEM graduates are almost entirely explained by selection into subject groups. Conversely, the coefficient for Social Sciences & Humanities increases strongly suggesting that graduating in Social Sciences & Humanities increases the risk of overqualification relative to graduating in STEM disciplines. Thus the results point towards the presence of individual unobservable characteristics, which simultaneously increase the chance of studying Social Sciences & Humanities and lower the risk of overqualification. It remains unclear, which characteristics these might be. Nevertheless, the group of Social Sciences & Humanities is both small and heterogeneous, and this might affect the results. Columns 4 and 5 show the results for the simple probit regression and the instrumental variable model with a probit model for the overqualification equation, respectively. All the coefficients shown are average marginal effects, so that we can compare these to the coefficients of the previous models. Comparing the outcomes from the model in column (5) to the simple probit, we can find a similar pattern as the one found with the linear models (contrasting the linear probability model with the model in column (3)). In the non-linear instrumental variable model the Business & Law coefficient becomes even negative (see column (5)). The Social Sciences & Humanities coefficient increases, albeit to a lower extent than in the linear instrumental variable model (see column (3)). These results are robust with respect to the specification used, as the 2SLS and DT specifications lead qualitatively to the same results (Table 2.A.2).

2.5.2 Impacts on Wages

Our second aim is to investigate the effect of subjects on wages. The main model we estimate is the MP model following the procedure described in Section 2.3, which allows us to take into account the non-linearity of the first stage regression. The instruments used are the same as for the overqualification models and the model is analogous to the one presented in column (3) of Table 2.3. Again, we compare our main model to two OLS models with and without the inclusion of the average high-school grades.

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and Pischke, 2008). The test statistic equals 18.6 for Business & Law and 23.6 for Social Sciences & Humanities, much above the rule-of-thumb value of 10.

Column (1) of Table 2.4 shows the results of the OLS regression when grades are omitted. Graduates in Business & Law subjects appear to receive hourly wages that are very similar to those of STEM graduates. The difference in earnings between the two groups of graduates found in the descriptive table thus disappears when we include control variables. Differently, graduates in Social Sciences & Humanities earn about 25% less than the above groups and the coefficient is highly significant. Column (2) shows the results of the same model when average grades are controlled for. As expected, the coefficient of school grades is positive, but is not significant at standard confidence levels. Nevertheless, it seems to be important to control for grades since the point estimates for the subject group dummies change slightly. Consistently with ability sorting into STEM fields, the negative coefficient of Social Sciences & Humanities decreases, while the Business & Law coefficient increases.

Column (3) of Table 2.4 shows the results of the MP model. Again, Panel B shows the coefficients of the instrumental variables and the average school grades for the subject choice equation. The coefficient shown in Panel B are average marginal effects. Since the subjects choice equations are the same as for the overqualification results, the coefficients are almost the same. Coefficients for the log wage equation are shown in Panel A. The coefficient for Business & Law subjects is larger compared to the OLS coefficient, but remains insignificant because of the larger standard errors of the IV model. The difference in hourly wages between Business & Law graduates and STEM graduates increases to about 10%. Similarly to the overqualification regressions, selection into subjects appears to play a role when we compare these two groups, although the effect is still insignificant. However, note that the effect becomes significant if we exclude self-employed individuals or individuals with migration background (see Table 2.A.6). Conversely, the coefficient for Social Sciences & Humanities decreases slightly in the MP model. Thus, selection does not seem to matter much for the difference between the STEM and the Social Sciences & Humanities group. The results are robust with respect to the specification employed, as we get comparable results with the ATE IV, 2SLS and DT specifications (Table 2.A.3). The coefficient for Social Sciences & Humanities are larger in the ATE IV and DT compared to the MP specification and insignificant in the ATE IV and 2SLS specifications, but they again remain close to those of the OLS models, such that our interpretation remains unaffected.

Table 2.4: Effect of Fields of Study on Log Hourly Wages

	OLS		MP
	(1)	(2)	(3)
Panel A: Wage equation			
Business & Law	-0.003	0.002	0.102
	(0.029)	(0.030)	(0.105)
Social Sciences & Humanities	-0.249***	-0.243***	-0.245**
	(0.037)	(0.038)	(0.115)
Grade: average		0.025	0.031
		(0.020)	(0.021)
Demographic charact.	Yes	Yes	Yes
Parental education	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes
Panel B: Subject choice equation			
<u>Business & Law fields</u>			
Grade: difference			-0.122***
			(0.017)
Played music in youth			-0.088**
			(0.040)
Grade: average			-0.076***
			(0.029)
<u>Social Sciences & Humanities</u>			
Grade: difference			-0.117***
			(0.015)
Played music in youth			0.036
			(0.034)
Grade: average			-0.070***
			(0.024)
<u>Control variables</u>			
Demographic charact.			Yes
Parental education			Yes
Pre-graduation charact.			Yes
Post-graduation charact.			Yes
School state dummies			Yes
Observations	896	896	896
R-sq.	0.353	0.355	

Standard errors in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The coefficients in Panel B are average marginal effects.

2.5.3 Robustness Checks

In Section 2.3 we highlighted that there are studies showing that mathematics skills are particularly important for labour market outcomes (Joensen and Nielsen, 2009; Murnane et al., 1995). Therefore, there might be reasons to be suspicious about the validity of the difference in grades instrument. For example, if mathematics grades would have a larger positive effect on wages than German grades (other than through the field of study chosen), this might lead to biased IV estimates. Since the grade difference is negatively correlated with the Business & Law and Social Sciences & Humanities dummies, coefficients of the two subject dummies would be underestimated in the 2SLS wage equations. By the same token, if mathematics grades would matter more than German grades for overqualification, this would lead to an overestimation of the two subject dummies in the 2SLS overqualification equation. Therefore, if mathematics grades matter more than German grades for labour market outcomes, the IV models would underestimate the bias of a possible ability sorting into STEM subjects. We perform an informal test to investigate whether math and German grades have different impacts on the dependent variables. Table 2.A.4 shows the results of OLS regressions for overqualification and wages highlighting the impact of high school grades. For each dependent variable, we show the results from two specifications - with and without the inclusion of subject dummies. The coefficient for average grade is, as expected, negative for the overqualification regressions and positive for the wage regressions. Conversely, the coefficient of the difference in grades (i.e. of the instrumental variable) is never significant in any specification. When subject dummies are not controlled for, the coefficient is small and negative in the overqualification regression (column (1)) and small and positive in the wage regression (column (3)). From these regressions mathematics grades seem to be slightly more important than German grades for labour market outcomes, even if the effect is close to zero and far from being significant. However, when we control for subject dummies the coefficient approaches zero in the overqualification regression (column (2)) and reverses sign in the wage regression (column (4)). We receive similar results when regressing wages (resp. overqualification) on math and German grades, i.e. both grades have effects of similar sizes on wages (resp. overqualification). This informal test suggest that the difference in grades does not directly affect labour market outcomes, i.e. that it is a valid instrument. This is in line with recent evidence, which shows that returns to mathematical and literacy skills are very similar in Germany (Hanushek et al., 2015).

The exogeneity of the other exclusion restriction i.e. playing music in youth could be also called into question. We believe that in our setting the potential concerns are minimized for the reasons outlined in Section 2.3.2. Nevertheless, we perform the same informal test also with this second instrumental variable. The results are shown in Table 2.A.5. The coefficient for playing music in youth is never significant in any specification. The coefficient is positive in the overqualification equation and is about 2% in both specifications (with or without subject dummies). Conversely, it is negative and about 3% in the wage equation without subject control variables, but drops to the half (1.4%) when subject dummies are included. All coefficients are very small and far from being significant. Thus, playing music in youth does not seem to have a direct effect on overqualification and wages, i.e. we are confident that it is a valid instrument.

Since our model is just-identified, we cannot directly implement a test of overidentifying restrictions in our context. A simple solution is to drop one of the three subject categories, estimate a GMM IV model with the binary subject dummy as endogenous variable and test our overidentifying instruments. We first drop the Social Sciences & Humanities category and estimate a model comparing STEM fields to Business & Law fields. The test statistic of the Hansen-Sargan test equals 0.017 (p-value of 0.90). Similarly, dropping the Business & Law fields and comparing STEM to Social Sciences & Humanities, we get a test statistic of 0.635 (p-value of 0.43). Finally, if we drop STEM fields and follow the same procedure we get a test statistic of 2.44 (p-value of 0.12). Therefore, in none of the three cases we would reject the null hypothesis that the two instruments are valid. The conditions under which the overidentification test can fail to detect invalid instruments seem implausible in our case (c.f. appendix 2.A.1).

Although we restrict our sample to employed males with at least 5 years of potential experience (measured as time since graduation), our sample is still very heterogeneous. Ideally, we would like to look at potential heterogeneous effects by interacting the instrument with relevant individual characteristics. However, because of the small sample size and the many control variables, this is impracticable. A feasible alternative is to exclude small sub-samples of individuals and estimate the main specifications with a restricted sample. This can also ensure that our results are consistent for the whole sample and are not driven by large effects for very specific graduates. Table 2.A.6 shows the results for sub-samples excluding self-employed, part-time workers and older cohorts, respectively. We included self-employed workers in our main specification, because we consider self-employment to be an outcome of the university subject choice. However, individuals with a propensity towards self-employment might choose specific fields especially because they allow them the possibility to be self-employed. Moreover, self-employed workers might respond differently to the question regarding the qualifications required for the job and this can affect the overqualification variable.[19] Results for the sub-sample of employees are shown in Panel A of Table 2.A.6. If anything, we can observe larger differences to the main model for the wage regression than for overqualification regressions. While the point estimates in the OLS do not differ too much to the main specifications, the difference to the coefficients in the IV model gets larger. On the one hand, the coefficient for Business & Law increases to 0.22 and is now statistically significant (at the 90% confidence level). On the other hand, the coefficient for Social Sciences & Humanities is of -0.13 (about 9 percentage points larger than the estimate in the OLS case). Panel B shows the results when we exclude part-time workers. Again, we did not control for part-time jobs considering them more an outcome of the study choice than an individual attitude. This argument is especially valid for involuntary part-time. On the contrary, voluntary part-time might be the outcome of individual attitudes such as family orientation. The results for the sub-sample of full-time workers are very similar to the main results. This is not surprising since there are only 24 males in our sample that work in part-time jobs. Panel C shows the results for a younger sub-sample, namely if we exclude individuals born before 1950. Also in this case the results do not differ qualitatively. Nevertheless, it has to be pointed out that ability sorting (or other similar unobservable biases) seem to play a smaller role for the difference in wages between STEM and Business & Law graduates when older cohorts are excluded.

2.6 Conclusions

In this paper, we analyze the effects of graduating in STEM fields and other subjects on wages and the risk of overqualification. Unobservable factors, such as ability, are likely

[19] A concern is that self-employed could underestimate the qualification required by the job. Indeed, self-employed workers are more likely to report to be overqualified in the job. The overqualification incidence is of 19% for self-employed and of 14% for other worker and this difference is significant (at the 99% confidence level) according to a simple t-test.

to affect not only wages and the risk of overqualification, but also the probability of graduating in a specific subject. We therefore apply instrumental variable techniques to control for the selection into subjects.

We find that the risk of overqualification for Business & Law graduates is about 7-8 percentage points higher compared to STEM graduates, even when controlling for individual, family, and other characteristics. However, once we control for the selection into subjects, no significant differences in the risk of overqualification remain between these groups. Moreover, controlling for individual, family and other characteristics we find almost no wage differences between Business & Law and STEM graduates. However, once we control for selection, we find that Business & Law graduates earn on average 10% more than STEM graduates.[20] These results hint to a positive selection of students into STEM subjects.

The results for Social Sciences & Humanities are less clear. Graduates in these subjects face a higher risk of overqualification, and it seems that this risk is even higher when controlling for selection into subjects. This would imply that studying Social Sciences & Humanities is associated with a higher risk of overqualification than studying STEM and that this is reduced on aggregate by the selection of students with favorable unobserved characteristics into Social Sciences & Humanities. Moreover, Social Sciences & Humanities graduates earn less than STEM graduates and controlling for selection does not significantly affect this wage gap. However, this group accounts for only 16% of the sample and contains very diverse subjects, so that these results could be influenced by the small sample size and the diversity of the subjects.

Our results are robust with respect to different specifications, variations of the sample, and in- or exclusion of control variables. Additionally, we provide tests to confirm the credibility of our instruments.

Our results indicate that it is not sufficient to compare average wages and average risks of overqualification between fields of study when one is interested in the individual returns to subject choices. Moreover, the results are highly relevant for education policy. They suggest that policies promoting STEM over Business & Law subjects could negatively affect wages while having no effect on the risk of overqualification for pupils, who would otherwise have chosen Business & Law. This does not imply that such policies are

[20] The effect is only significant when individuals with migration background or self-employed individuals are excluded from the sample.

ineffective. Assuming that there is a lack of STEM graduates, such policies could be an option to increase the aggregate supply of these graduates. The results, however, indicate that the individual level effects of such policies could be negative in terms of wages and that these policies might not reduce the incidence of overqualification. This negatively affects the efficiency of such policy measures with respect to their goal of increasing the supply of STEM graduates. Our results suggest that individual characteristics play an important role for the subject choice, as well as for labor market outcomes. This would imply that policy measures which aim at increasing the supply of adequate STEM graduates should additionally take into account individual characteristics by, for example, fostering the development of math skills early in school. In line with this, Stinebrickner and Stinebrickner (2014) argue that students' subject choices by and large fit their abilities, so that policies which aim at increasing the supply of science graduates should focus on providing pupils with the skills required by these subjects.

Our findings are based on a sample of German men born between 1940 and 1980. It would be very relevant from a policy perspective to check whether similar results are found for women and for younger cohorts. Moreover, further research is necessary to evaluate the role of policies promoting STEM fields for the aggregate supply of STEM graduates and to identify whether there is an excess demand for STEM graduates.

2.A Appendix

2.A.1 A Note on the Overidentification Test

Above we have reduced the sample to bivariate comparison of fields of study (for each potential comparison) so that we were able to apply overidentification tests. In non of the cases did the test reject the null hypothesis that the instruments are valid. However, an objection against the use of the overidentification test for detecting invalid instruments is that the test does not detect invalid instruments when the IV estimators using the full and the reduced set of instruments are similarly asymptotically biased (Wooldridge, 2010, p. 135). Parente and Santos Silva (2012) discuss this issue in the case of one endogenous explanatory variable and two instruments — as in the case of our bivariate comparison of fields of study — and argue that the overidentification test does not detect invalid instruments if $\gamma_2/\gamma_1 = \pi_2/\pi_1$, where π_1 and π_2 are the coefficients in the linear projection of the explanatory endogenous variable (field of study) on the instruments and γ_1 and γ_2 are the coefficients in the projection of the error of the outcome equation on the instruments. More generally, De Blander (2008) shows that the overidentification test does not detect invalid instruments if the instruments appear in the same linear combination in the linear projection of the error of the outcome equation on the instruments, as they appear in the linear projection of the endogenous explanatory variable (field of study) on the instruments.

In our case both instruments are negatively correlated with the Business & Law dummy, so that the test would fail if the instruments were directly linked to the error in the outcome equation and if the ratio of these effects would be about the same as for the instruments' effects on the endogenous explanatory variable. This requires that the instruments are linked with the same sign to the error in the outcome equation. For example, higher ability pupils might both have relative better math grades and might be more likely to play instruments, so that we would underestimate the wage-effect of the Business & Law dummy with both instruments, assuming that ability positively affects wages. Conversely, the first instrument is negatively and the second instrument is positively correlated to the Social Sciences & Humanities dummy, so that the test would fail if the instruments were directly linked to the error in the outcome equation and if the ratio of these effects would be about the same as for the instruments' effects on the endogenous explanatory variable. This requires that the instruments are linked with opposite signs to the error in the outcome equation. For example, higher ability pupils might have relative better math grades and might be less likely to play instruments, so that we would underestimate the wage-effect of the Social Sciences & Humanities dummy with both instruments. This implies that the test can fail in both cases only if the instruments are differently linked to the outcome equation for the two fields of study. In fact, in neither case the test rejects the null so that the test can only fail if the instruments differently directly affect wages (overqualification) for Business & Law vs. STEM than for Social Sciences & Humanities vs. STEM. This seems implausible and we are therefore confident that the test is reliable.

2.A.2 Tables and Figures

Table 2.A.1: Fields of Study Groups

Subject group	Specific fields of study
STEM	Mathematics, Physics, Astronomy, Chemistry, Pharmacology, Biology, Geosciences, Computer Science, Engineering (incl. Civil, Mechanical, Electrical and Traffic Engineering), Mining and Metallurgy, Architecture and Interior Design, Regional Planning, Surveying and Mapping
Business & Law	Law, Business Administration, Public Management and Governance, Economics, Managerial Engineering
Social Sc. & Humanities	Philosophy, History, Literary Studies, Linguistics, Philology, Cultural Studies, Theology, Psychology, Political Science, Social Sciences, Social Work, Geography, Landscape Conservation, Agricultural Sciences, Forest Management, Fine Arts, Design, Performance, Film and Television, Theater, Sport Science, Music, Musicology

Note that Education, Medicine, Dentistry and other health sciences are excluded from the sample.

Table 2.A.2: Effect of Fields of Study on the Risk of Overqualification - Different Specifications

	LPM	2SLS	DT
	(1)	(2)	(3)
Panel A: Overqualification equation			
Business & Law	0.075***	-0.096	0.019***
	(0.027)	(0.184)	(0.005)
Social Sc. & Humanities	0.046	0.203	0.262***
	(0.034)	(0.206)	(0.005)
Grade: average	-0.045**	-0.048**	-0.031***
	(0.018)	(0.021)	(0.002)
Demographic charact.	Yes	Yes	Yes
Parental education	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes
Panel B: Subject choice equation			
Business & Law fields			
Grade: difference		-0.061***	-0.562***
		(0.015)	(0.096)
Played music in youth		-0.091***	-0.424**
		(0.032)	(0.204)
Grade: average		-0.043*	-0.367**
		(0.024)	(0.143)
Social Sciences & Humanities			
Grade: difference		-0.061***	-0.738***
		(0.011)	(0.118)
Played music in youth		0.054**	0.294
		(0.026)	(0.223)
Grade: average		-0.038**	-0.453***
		(0.018)	(0.157)
Control variables			
Demographic charact.		Yes	Yes
Parental education		Yes	Yes
Pre-graduation charact.		Yes	Yes
Post-graduation charact.		Yes	Yes
School state dummies		Yes	Yes
Observations	896	896	896
R-sq.	0.118	0.023	

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Note that the coefficients shown in column (3) and column (4) are not average marginal effects and cannot be directly compared to the coefficients of the other models.

Table 2.A.3: Effect of Fields of Study on Log Hourly Wages - Different Specifications

	OLS	2SLS	ATEIV	DT
	(1)	(2)	(3)	(4)
Panel A: Wage equation				
Business & Law	0.002	0.152	0.069	0.200***
	(0.030)	(0.200)	(0.164)	(0.050)
Social Sc. & Humanities	-0.243***	-0.271	-0.217	-0.229***
	(0.038)	(0.206)	(0.153)	(0.077)
Grade: average	0.025	0.032	0.030	0.036*
	(0.020)	(0.022)	(0.022)	(0.021)
Demographic charact.	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Panel B: Subject choice equation				
Business & Law fields				
Grade: difference		-0.061***	-0.059***	-0.585***
		(0.015)	(0.014)	(0.098)
Played music in youth		-0.091***	-0.087***	-0.444**
		(0.032)	(0.031)	(0.209)
Grade: average		-0.043*	-0.037*	-0.371**
		(0.024)	(0.022)	(0.147)
F-test (excl. instr.)		18.6		
Social Sciences & Humanities				
Grade: difference		-0.061***	-0.058***	-0.825***
		(0.011)	(0.011)	(0.128)
Played music in youth		0.054**	0.052**	0.305
		(0.026)	(0.024)	(0.257)
Grade: average		-0.038**	-0.032*	-0.511***
		(0.018)	(0.016)	(0.179)
F-test (excl. instr.)		23.6		
Control variables				
Demographic charact.		Yes	Yes	Yes
Parental education		Yes	Yes	Yes
Pre-graduation charact.		Yes	Yes	Yes
Post-graduation charact.		Yes	Yes	Yes
School state dummies		Yes	Yes	Yes
Observations	896	896	896	896
R-sq.	0.355	0.330	0.351	

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Note that the coefficients shown in Panel B of column (4) are not average marginal effects and cannot be directly compared to the coefficients of the other models.

Table 2.A.4: Effect of High-School Grades on Overqualification and Wages

	Overqualification		Log hourly wages	
	LPM	LPM	OLS	OLS
	(1)	(2)	(3)	(4)
Grade: difference	-0.007	0.000	0.008	-0.008
	(0.012)	(0.013)	(0.012)	(0.012)
Grade: average	-0.051***	-0.046**	0.035*	0.026
	(0.018)	(0.018)	(0.020)	(0.020)
Business & Law		0.075***		-0.002
		(0.029)		(0.030)
Social Sc. & Humanities		0.046		-0.249***
		(0.035)		(0.039)
Demographic characteristics	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Observations	896	896	896	896
R sq.	0.110	0.118	0.319	0.355

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 2.A.5: Effect of Playing Music in Youth on Overqualification and Wages

	Overqualification		Log hourly wages	
	LPM	LPM	OLS	OLS
	(1)	(2)	(3)	(4)
Played music in youth	0.021	0.024	-0.029	-0.014
	(0.024)	(0.024)	(0.027)	(0.026)
Grade: average	-0.053***	-0.046**	0.038*	0.025
	(0.018)	(0.019)	(0.019)	(0.019)
Business & Law		0.077***		0.001
		(0.028)		(0.030)
Social Sc. & Humanities		0.044		-0.242***
		(0.033)		(0.038)
Demographic characteristics	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Observations	896	896	896	896
R sq.	0.110	0.119	0.320	0.355

Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

Table 2.A.6: Results for Restricted Sample

	Overqualification		Log hourly wages	
	Probit	MP Probit	OLS	MP
	(1)	(2)	(3)	(4)
Panel A: Excluding self-employed workers				
Business & Law	0.067**	-0.074	-0.003	0.224*
	(0.026)	(0.087)	(0.028)	(0.121)
Social Sc. & Humanities	0.032	0.197	-0.220***	-0.132
	(0.035)	(0.152)	(0.038)	(0.099)
Grade: average	-0.044**	-0.048**	0.035*	0.056**
	(0.017)	(0.019)	(0.020)	(0.022)
Demographic characteristics	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Observations	760	760	760	760
R sq.			0.401	
Panel B: Only full-time workers				
Business & Law	0.070***	-0.085	0.002	0.078
	(0.025)	(0.081)	(0.028)	(0.110)
Social Sc. & Humanities	0.045	0.253**	-0.260***	-0.297***
	(0.033)	(0.108)	(0.038)	(0.085)
Grade: average	-0.044***	-0.048**	0.021	0.024
	(0.016)	(0.019)	(0.019)	(0.020)
Demographic characteristics	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Observations	872	872	872	872
R sq.			0.380	

Standard errors in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$;
The coefficients in columns (1) and (2) are average marginal effects.

Table 2.A.6: Results for Restricted Sample (continued)

	Overqualification		Log hourly wages	
	Probit	MP Probit	OLS	MP
	(1)	(2)	(3)	(4)
Panel C: Younger cohorts				
Business & Law	0.071**	-0.090	-0.017	0.034
	(0.028)	(0.077)	(0.031)	(0.125)
Social Sc. & Humanities	0.067*	0.210*	-0.251***	-0.254***
	(0.036)	(0.109)	(0.042)	(0.108)
Grade: average	-0.047***	-0.049**	0.021	0.024
	(0.018)	(0.019)	(0.021)	(0.022)
Demographic characteristics	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Observations	722	722	722	722
R sq.			0.357	
Panel D: Excluding individuals with migration background				
Business & Law	0.059**	-0.111	0.009	0.173*
	(0.026)	(0.079)	(0.030)	(0.096)
Social Sc. & Humanities	0.040	0.161	-0.248***	-0.265**
	(0.034)	(0.127)	(0.040)	(0.115)
Grade: average	-0.043**	-0.050**	0.010	0.020
	(0.017)	(0.020)	(0.020)	(0.022)
Demographic characteristics	Yes	Yes	Yes	Yes
Parental education	Yes	Yes	Yes	Yes
Pre-graduation charact.	Yes	Yes	Yes	Yes
Post-graduation charact.	Yes	Yes	Yes	Yes
School state dummies	Yes	Yes	Yes	Yes
Observations	837	837	837	837
R sq.			0.365	

Standard errors in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$

The coefficients in columns (1) and (2) are average marginal effects.

Chapter 3

Local Labor Market Size and Qualification Mismatch

3.1 Introduction

There is ample evidence that workers earn higher wages in larger labor markets. For instance, Glaeser and Mare (2001) show that average wages in metropolitan areas with a big city (i.e. a city with more than 500.000 inhabitants) are about 33% higher than outside these areas. From an individual perspective, the higher cost of living in cities might explain why not all workers are willing to move to larger cities. However, the urban wage premium must reflect higher productivity in larger cities to explain why firms do not relocate to less urbanized areas. Duranton and Puga (2004) distinguish three mechanisms behind the higher productivity in larger cities: the sharing of facilities and risks, faster learning and knowledge diffusion and better matches between firms and workers. While the importance of better matches as a source of agglomeration economy is stressed from a theoretical perspective, there is little evidence of its empirical relevance (Puga, 2010). A major explanation for this is the paucity of data allowing to measure match quality in

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a comprehensive way. Previous studies have attempted to measure it indirectly through the share of occupational and industry changes (Bleakley and Lin, 2012) or through assortative matching in terms of worker and firm quality (Andersson et al., 2007; Dauth et al., 2016) and found evidence of better matches in more urbanized areas. The focus in this paper is on a direct measure of job match quality, namely the match between the formal qualifications earned by workers and the job requirements. This type of match has been found to be strongly related to wages and firm productivity (Kampelmann and Rycx, 2012; Leuven and Oosterbeek, 2011). More specifically, I look at the match between actual and required qualifications both in terms of level (vertical match) and in terms of content (horizontal match), since there is reason to expect both types of match to be better in thicker labor markets.

The question whether workers in more densely populated areas are less exposed to educational mismatch is also interesting in and of itself and relevant for the labor economics literature on skill mismatch. Does it actually pay off for individuals to move to larger cities in terms of better job matches and future career prospects? Previous studies have already investigated the impact of various regional labor market characteristics including the size of regional labor markets, regional unemployment rates and mobility restrictions on overqualification (Büchel and Ham, 2003; Jauhiainen, 2011). However, these studies aimed at analyzing several determinants of overqualification and not at establishing a clear - and possibly causal - link between the size of the local labor market and qualification mismatch. OLS regressions with standard control variables might lead to biased estimates in this context. On the one hand, since cities generally have a higher share of university graduates, one could suspect that they could be more attractive for individuals with higher unobserved ability. In fact, several papers have stressed the importance of addressing the spatial sorting of workers by individual skills in order to estimate the urban wage premium (Combes et al., 2008; Glaeser and Mare, 2001). On the other hand, the skill mismatch literature has found a positive correlation between measures of individual ability and overqualification (Leuven and Oosterbeek, 2011). Since more talented individuals could be both more likely to live in large cities and to have a better job match, the sorting of workers across areas could lead to an overestimation of the effect of city size on the job match. To address this potential bias, I estimate linear regressions of qualification mismatch on regional employment density including an extensive set of control variables that correlate with individual ability, such as information on parental background, school grades and personality traits.[1] I then follow three main empirical strategies to sequentially deal with the major empirical concerns and test whether the baseline estimates are robust for different sub-groups of individuals.[2] Firstly, by restricting the sample to individuals who have remained in the region where they grew up (non-movers) biases from the direct migration of more talented workers into cities can be addressed. Secondly, by estimating a fixed effects model on our panel of workers and obtaining identification through individuals migrating form one region to another, I can get rid of unobserved time-invariant heterogeneity (such as individual ability). Thirdly, I deal with potential reverse causality by instrumenting current employment density with historical population data from the 19th century.

The obtained estimates of employment density on overqualification are fairly similar across the different specifications. An increase of 10% in the regional employment density is associated with a decrease of 1-1.5% in the probability of being overqualified. On the contrary, most of the estimates of employment density on the horizontal mismatch measure are smaller and not statistically significant. Finally, I investigate the contribution of better qualification matches in explaining the wage premium in thicker labor markets. By including our mismatch measures to an OLS regression of log hourly wages on employment density (and other control variables), overqualification is found to explain only 6% of the impact of regional employment density on hourly wages, while the contribution of horizontal mismatch appears to be insignificant.

Two other recent studies analyze the effect of population or employment density on job mismatch for the US (Abel and Deitz, 2015) and France (Boualam, 2014).[3] Abel and Deitz (2015) find evidence of a moderate effect of population size and employment density on measures of vertical and horizontal mismatch for US college graduates. They also find that mismatch accounts for 5-8% of the urban wage premium. Boualam (2014) investigates the impact of employment density on a measure of horizontal match based on the distribution

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[1] I use regional employment density to measure the labor market size following previous studies (for a review of the literature on agglomeration economies see Combes and Gobillon, 2015 and Heuermann et al., 2010). The results do not change qualitatively when using population density or dummy variables for urban areas.

[2] This procedure is common in the urban wage premium literature, because of the difficulty of finding exogenous sources affecting the mobility of individuals across regions.

[3] Andini et al. (2013) also analyze the impact of population density on different measures of job matching, including the appropriateness of the educational qualification for the job. However, their coefficients are not statistically significant for the educational match, as well as for most of the other measures of matching.

of workers' fields of study within an occupation for French labor market entrants. While this measure of match quality is found to increase with employment density, it does not seem to explain differences in wages between thick and thin labor markets. The present paper provides at least three contributions. First, while the cited papers make use of cross-sectional data, I employ panel data that enables me to estimate fixed effects regressions to eliminate the time-invariant unobserved ability bias as in previous studies on the urban wage premium (Combes et al., 2008; Glaeser and Mare, 2001). Second, the survey data I use (i.e. the German Socio-Economic Panel) has extensive information on workers characteristics and biographies that might be very important to account for in the analysis to avoid potential omitted variable biases, such as detailed parental background information, high-school final grades and information on personality traits. Third, the data contains direct questions on the qualifications required by the job, allowing me to construct vertical and horizontal qualification mismatch variables based on workers' self-assessments. Measures based on worker's self-assessment are typically preferred over measures which infer the required qualification from the data at hand, since the latter not only depend on demand forces but also on qualifications supplied (Hartog, 2000).

The rest of the paper is organized as follows. Section 3.2 describes the data and presents descriptive evidence of the link between employment density and qualification mismatch. Section 3.3 contains the main results on the impact of employment density on overqualification and horizontal mismatch. While in Section 3.4 I attempt to disentangle the effect of labor market size from that of other characteristics of denser regions (such as specialization and skill structure) on the mismatch incidence, in Section 3.5 I investigate the contribution of qualification mismatch to the wage differential across regions. Finally, Section 3.6 concludes.

3.2 Data and Descriptive Statistics

3.2.1 Data Source and Key Variables

The sample used is drawn from the German Socio–Economic Panel (GSOEP), a panel data set for the years 1984-2012 consisting of about 20,000 individuals living in Germany (for details, see Kroh, 2012). I restrict the sample to males surveyed in the years 2000 to 2011 to avoid concerns about possible selection biases into labor force participation for women. The sample is further restricted to dependent workers employed full-time. The 12 GSOEP waves include 8,288 male adults aged between 16 and 64 with a university degree or a completed training qualification who are employed at least twice in the time framework considered. I end up with an unbalanced panel of 5,625 individuals (35,363 observations), for whom I have information on all variables relevant for our analysis.

I employ the regional employment density at the level of labor market regions as a measure of labor market size.[4] This is calculated by the number of employed individuals per square kilometer. Labor market regions are defined by the Federal Office for Building and Regional Planning to differentiate areas in Germany based on their economic interlinkages and of commuting patterns. This classification specifies 258 labor market regions with an average of 16 regions in each of the 16 federal states.[5] Information on employment density at the level of regional labor markets is gathered from administrative data sources (i.e. the INKAR database) and merged to the individual place of residence in the GSOEP data. [6]

I employ two measures for qualification mismatch: vertical mismatch (i.e. overqualification) and horizontal mismatch. Overqualification is measured based on workers' self–assessment about the educational requirements of the job. More precisely, the following question is asked in the GSOEP questionnaire: "What type of education or training is usually necessary for this type of work?" I consider an individual to be overqualified if he reports that his job requires a lower degree than the he possesses. One drawback of this measure, which is widespread in the literature on overeducation, is the reliance on the subjective individual self–assessment. For instance, according to Hartog (2000), respondents might have a tendency to upgrade the status of their position. However, since I employ this measure as an outcome variable, subjectivity would be an issue only if workers in small and large labor markets systematically differ in the way they answer such a question. Several authors have claimed that overqualification measures based on self-assessments are preferable to measures based on the distribution of educational qualifications within occupations – i.e. "realized matches" (Leuven and Oosterbeek, 2011).

[4] Similar results are found though when using population density or dummy variables for urban areas.

[5] Results using a different classification of 150 labor market regions show baseline estimates that are of similar magnitude, as shown in Table 3.A.4.

[6] Ideally, I would consider the workplace location, because agglomeration economies are expected to arise where the production process takes place. Unfortunately, this information is not available in the GSOEP. Nevertheless, I do not expect this to affect much the results, since few individuals commute outside of regional labor markets. Moreover, the GSOEP data includes information on commuting distances, so that I can test whether the results are robust to excluding long-distance commuters.

Measurement error might be more severe for measures based on realized matches, because they ignore any variation in required education within occupations. Moreover, realized matches do not reflect only job requirements but are already the outcome of the interplay of supply and demand (Hartog, 2000). I also rely on workers' self–assessment to compute the horizontal mismatch measure similarly to previous studies (see, e.g., Robst, 2007). The question asked in the GSOEP is: "Is this position the same as the profession for which you were educated or trained?". Since the only possible answers are yes or no, I construct a dummy that is equal to 1 if individuals answer negatively to this question.

Hourly wages are measured through the self-reported monthly gross income divided by monthly working hours. I calculate real wages based on the CPI deflator using 2010 as the base year. In order to ensure that outliers are not driving the main results I trim wages excluding the 1st and the 99th percentile (individuals receiving a hourly wage lower than EUR 4 or higher than EUR 75) and I employ the standard logarithmic form for the wage regressions. The richness of the data allows me to include in the analysis an extensive set of control variables. I consider demographic characteristics, educational and parental background information, job characteristics and some geographic characteristics, such as previous regional mobility. Further information on school grades, personality traits and risk preferences is available only for certain waves or for given individuals depending on the time of their first participation to the survey. I thus add this characteristics only in a separate analysis. In Section 3.4 I also add further regional information at the local labor market level which is gathered from the INKAR database.

3.2.2 Descriptive Results

Table 3.1 presents the mean and standard deviation for the variables included in the analysis. The overqualification incidence is about 15% in the sample, while the incidence of horizontal mismatch amounts to 30%. Employment density ranges from 16 employed individuals per square km in Salzwedel (Sachsen-Anhalt) to 1,889 in Berlin.[7] Most individuals have a vocational degree as their highest qualification (68%), while the rest of the sample has a tertiary degree either from a standard university or a university of applied science (*Fachhochschule, FH*). I further include information on the school leaving

[7] Figure 3.A.2 shows the differences in employment density across the 258 German labor market regions in 2010 (darker colors depict a higher employment density).

Table 3.1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.
Dependent variables and other main variables				
Overqualified	0.19	0.39	0	1
Horizontal mismatch	0.34	0.47	0	1
Hourly wage (log)	2.76	0.44	1.59	3.91
Employment density (log)	5.03	1.02	2.75	7.56
Main control variables				
University degree	0.21	0.41	0	1
FH degree	0.11	0.31	0	1
Vocational degree	0.68	0.47	0	1
Migration background	0.08	0.27	0	1
Married or living with partner	0.82	0.38	0	1
Actual work experience	21.1	10.5	0	48
Has children	0.40	0.49	0	1
School leaving qualification				
University access (Abitur)	0.27	0.44	0	1
FH access (Fachhochschulreife)	0.08	0.27	0	1
Realschulabschluss	0.35	0.48	0	1
Hauptschule or no degree	0.30	0.46	0	1
Parental background				
Father: higher educ.	0.14	0.34	0	1
Mother: higher educ.	0.06	0.25	0	1
Mother non-employed (age 15)	0.24	0.43	0	1
Geographic characteristics				
Lives in city of childhood	0.61	0.49	0	1
Macro-region				
Centre	0.33	0.47	0	1
North	0.13	0.34	0	1
South	0.27	0.44	0	1
East	0.27	0.44	0	1
Job characteristics				
Public sector	0.25	0.43	0	1
Firm tenure	12.8	10.5	0	47
Industry				
Agriculture	0.01	0.12	0	1
Energy	0.02	0.13	0	1
Mining	0.01	0.07	0	1
Manufacturing	0.23	0.42	0	1
Construction	0.19	0.39	0	1
Trade	0.10	0.30	0	1
Transport	0.07	0.25	0	1
Bank & Insurance	0.05	0.23	0	1
Services	0.32	0.47	0	1

Note: The summary statistics are based on the baseline sample of 35,363 observations (5,625 individuals). Main control variables include year fixed effects, as well as a squared term for work experience. Job characteristics also include firm tenure squared.

qualification, which is a further important control variable in Germany because of the tracking system of secondary education. Individuals have on average 21 years of work experience, most of which (13 years on average) gained with their current employer. Most of the individuals in the sample (61%) never left the city where they grew up.

Figure 3.1: Employment Density and Qualification Mismatch

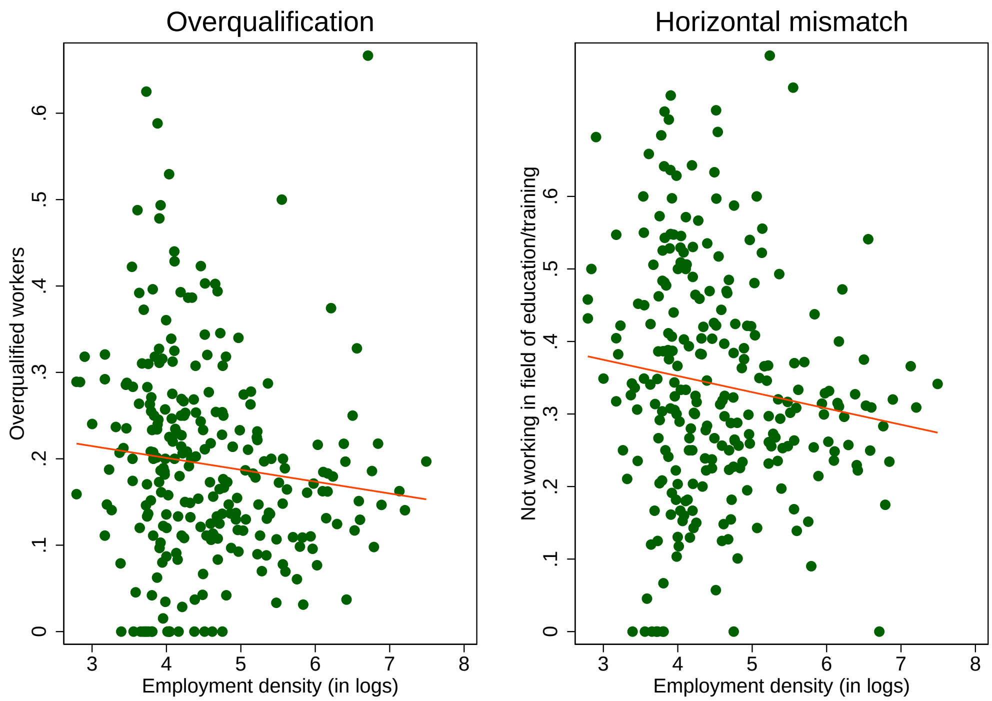

Figure 3.1 shows the existence of a negative relationship between employment density and qualification mismatch as measured through the subjective assessment of the qualification level required by the job (vertical mismatch or overqualification) and the relatedness of the job to the worker's field of education or training (horizontal mismatch). The unit of observation in both graphs is the labor market region, meaning that the information on the individual match is aggregated at the regional level. The slope of the fitted regression line is -0.014 for vertical mismatch and -0.022 for horizontal mismatch and the coefficients are statistically significant at standard levels for both regressions.

3.3 Impact of Agglomeration on Qualification Mismatch

3.3.1 Baseline Regressions

Having seen that there is a negative relationship between employment density and qualification mismatch, I first test whether the results change when I include an extensive set of control variables. I thus estimate a the following simple linear probability model[8]:

$$Pr(mismatch_{ijt} = 1) = \alpha + \beta \ empdensity_{jt} + \gamma \mathbf{X_{ijt}} + \epsilon_{ijt} \tag{3.1}$$

where *mismatch* is a dummy variable that takes value 1 in case of a qualification mismatch for individual i in year t, *empdensity* denotes the employment density of the region of residence j in year t and $\mathbf{X_{ijt}}$ is a vector of covariates that differs across specifications. Panel A of Table 3.2 shows the results for the overqualification dummy, and Panel B those for horizontal mismatch. Column (1) reports results for a regression with the inclusion of the main control variables only (i.e. highest educational qualification, migration background, marital status, having children in household, actual experience, experience squared, year dummies). The remaining five columns show results by gradually including dummies for the school leaving qualification, parental background characteristics (i.e. father and mother education, whether the mother was employed when the individual was 15 years old), geographic characteristics (macro-region dummies and whether individual still lives in place of childhood), job characteristics (i.e. tenure, public sector, industry dummies) and occupation fixed effects in column (6).

Column (1) in Panel A shows the existence of a negative relationship between regional employment density and the probability of being overqualified for the job when standard control variables are included. The coefficient is equal to -0.031 and is significant at the 1% significance level. A 10% increase in employment density would imply a decrease of about 0.3 percentage points in the overqualification probability, which is equal to a decrease of about 1.6% (given that the overqualification rate in our sample is 19%). The employment density coefficient decreases to -0.018 when school degree dummies, parental background

[8] Average marginal effects estimates of a probit model lead to results that are very similar to the linear probability model estimates.

Table 3.2: Impact of Employment Density on Qualification Mismatch

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Overqualification			
Empl. density (log.)	-0.031***	-0.026***	-0.025***	-0.017***	-0.019***	-0.012***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
School degree	No	Yes	Yes	Yes	Yes	Yes
Parental background	No	No	Yes	Yes	Yes	Yes
Geographic charact.	No	No	No	Yes	Yes	Yes
Job charact.	No	No	No	No	Yes	Yes
Occupation FE	No	No	No	No	No	Yes
Observations	35,363	35,363	35,363	35,363	35,363	35,363
R-squared	0.021	0.049	0.051	0.060	0.090	0.207
			Panel B: Horizontal mismatch			
Empl. density (log.)	-0.030***	-0.027***	-0.025***	-0.012*	-0.015**	-0.012**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
School degree	No	Yes	Yes	Yes	Yes	Yes
Parental background	No	No	Yes	Yes	Yes	Yes
Geographic charact.	No	No	No	Yes	Yes	Yes
Job charact.	No	No	No	No	Yes	Yes
Occupation FE	No	No	No	No	No	Yes
Observations	35,363	35,363	35,363	35,363	35,363	35,363
R-squared	0.041	0.055	0.057	0.069	0.099	0.184

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variables. Standard errors are clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

information, geographic characteristics and job characteristics are included. The inclusion of occupation fixed effects (ISCO 1-digit) in column (6) leads to a smaller coefficient (-0.012), but is still statistically significant. While the ISCO classification at the 1-digit level is relatively broad, its inclusion together with the information about educational qualifications might partially capture vertical qualification mismatch. Therefore, in the overqualification regressions it is better not to control for occupation fixed effects, since these might be bad control variables.

Panel B shows that regional employment density appears to have a negative impact on horizontal mismatch as well, i.e. whether one works in the same field of one's education or training. The coefficient in column (1) is equal to -0.030 and is statistically significant. A 10% increase in employment density would imply a decrease of about 1% in horizontal mismatch (since the incidence of horizontal mismatch is about 30%). The coefficient decreases to -0.016 when school degree dummies, parental background information, geographic controls and job characteristics are included. In particular, a large part of the correlation between density and horizontal mismatch can be explained by differences between West and East Germany, as East Germany is characterized on average by both a lower employment density and a higher incidence of horizontal mismatch. When occupation fixed effects are included, the coefficient drops (in absolute value) further to 0.012 but is still significant at the 5% significance level. Since no information on the field or orientation of the highest qualification obtained is included, there are less arguments against the inclusion of occupation fixed effects in the case of horizontal mismatch. Workers in denser regions appear thus to have a better job match in terms of their actual qualifications.

3.3.2 Controlling for School Grades, Personality Traits and Risk Preferences

The GSOEP data contains further individual information which might be important to control for when analyzing the effect of employment density on skill mismatch. First, high-school grades might proxy individual ability and motivation and thus reduce potential biases from the sorting of talented individuals into larger cities. Second, personality traits and risk preferences might differ on average across regional areas and are likely to affect the job match, as well as the individual assessment of the match. Since these characteristics are available only for a relative small sample of individuals, I exclude these from the baseline regressions and present separate results for a sub-sample of 2,141 individuals, for whom I have information on all relevant characteristics.

Table 3.A.1 presents results of a linear probability model of qualification mismatch by gradually adding mathematics and German grades from workers' final school reports, standard measures of the big five personality traits (extraversion, consciousness, agreeableness, neuroticism and openness to experience) and a subjective measure of risk preference.[9]

[9] Mathematics and German are the only compulsory courses for the high school diploma in most federal states in Germany. The grades are measured using the 6 points scale typical for the German system, where 1 is the best grade and 6 the worst. The big 5 personality traits are indexes in the range of 1 to 21, which are computed basing on a larger set of personality items contained in the survey following Gerlitz and Schupp (2005). The measure of risk preference is a index ranging from 1 to 10 based on an individual statement. Since I have information for both the big five and risk preference only for specific years, I compute the individual average of all observed values.

Columns (1) and (5) of the table present the results of the same model of column (5) in Table 3.2, where all baseline control variables are included except for occupation fixed effects. The employment density coefficient is slightly larger in absolute terms for overqualification compared to the baseline sample and is statistically significant. Conversely, the estimate is not significant for horizontal mismatch, because of the smaller sample size. For both variables, however, the estimates remain very similar when school grades, personality traits and risk preference are included. While some characteristics matter for the qualification mismatch measures, they appear to be almost irrelevant for the impact of employment density on the match.[10]

3.3.3 Addressing the Omitted Ability Bias

Two empirical strategies are employed to address the potential overestimation of the results due to omitted ability bias stemming from the sorting of talented individuals into larger cities. Similarly to Boualam (2014) I first investigate whether the results are different for the sub-samples of individuals ever moving from a district to another (movers) and the ones who stay in the place where they grew up (non-movers). Focusing on non-movers helps me to avoid biases from the direct migration of more talented workers to cities. Column (1) of Table 3.3 reports the results for the same linear regression estimated in the last column of Table 3.2 (with the inclusion of all control variables apart from occupation fixed effects). The same model is then estimated on the sub-sample of individuals never moving from the place of childhood who represent about 61% of the sample.[11] In the case of overqualification, the coefficient for the sub-sample of non-movers is very similar to the one for all individuals. In the case of horizontal mismatch, the coefficient is even slightly larger.

The results for the sub-sample of non-movers might still be biased if talented individuals are more likely to grow up in cities because of inherited abilities by parents and grandparents who moved to large agglomerations (Bosquet and Overman, 2016; Glaeser and Mare, 2001). Thus, in a second step, I exploit the panel structure of the data in order to control for individual fixed effects. On the one hand, this enables me to address the problem that

[10] In particular, it appears that individuals with better math and German school grades have a lower likelihood of mismatch. Among the big 5 personality traits neuroticism in particular appears to be positively associated with both mismatch variables.

[11] This question is constructed based on a survey question asking if the individual still lives in the city or regional area where most of the childhood was spent.

Table 3.3: Impact of Employment Density on Qualification Mismatch: Addressing Spatial Sorting

	Pooled OLS		Fixed effects			
	all	non-movers	all		restricted sample	
	(1)	(2)	(3)	(4)	(5)	(6)
			Overqualification			
Empl. density (log.)	-0.018***	-0.017***	-0.017*		-0.020*	
	(0.004)	(0.006)	(0.009)		(0.011)	
Ave. empl. density (log)				-0.014		-0.017*
				(0.009)		(0.010)
			Horizontal mismatch			
Empl. density (log.)	-0.016***	-0.019**	-0.005		-0.005	
	(0.006)	(0.008)	(0.009)		(0.010)	
Ave. empl. density (log)				-0.002		-0.002
				(0.009)		(0.009)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No	No	No
Observations	35,363	21,532	35,363	35,363	35,137	35,137

Note: Standard errors are clustered at the individual level in the cross-sectional regressions and at the individual level in the panel regressions; *** p<0.01, ** p<0.05, * p<0.1; Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies). [a] Only control variables that change over time are included.

unobserved individual ability might lead to an overestimation of the results. On the other hand, the identification will be achieved through individuals migrating from one district to another and individuals moving to a different region are likely to do so because they find a better job match (Gould, 2007). Therefore, the identification strategy will rely on the assumption that the reason to change region will not differ for the same individual whether he moves to a larger or a smaller region. Only 486 individuals in our sample change their labor market region of residence. 260 of these change both region and job. Since the identification will hinge upon those changing their region of residence and only job switchers can change the match status, I also estimate a regression excluding the spells in which individuals change region but not the job.

For simplicity, I estimate the following linear fixed effects model that gets rid of the time-constant unobserved individual heterogeneity:

$$Pr(\ddot{overqual}_{ijt} = 1) = \beta \ \ddot{empdensity}_{jt} + \gamma \ddot{\mathbf{X}}_{\mathbf{1,ijt}} + \ddot{\epsilon}_{ijt} \tag{3.2}$$

where the "double dot" denotes that the variables are time demeaned, *overqual* is a dummy variable denoting if individual i in year t is overqualified for the job, *empdensity* denotes the employment density of the region of residence and the vector $\mathbf{X_{1,ijt}}$ includes all time variant control variables, excluding occupation fixed effects. These are part of the demographic characteristics, job and geographic characteristics. In a separate regression I also use the average regional employment density over the period considered (2000-2011), so that change in density across years are not taken into account. I do this because, unlike wages, the mismatch measures are dummies that are typically constant if the worker does not change job and it is unlikely that they respond quickly to small changes in the size of the labor market. Column (4) of Table 3.3 presents the results of the fixed effects model. The coefficient for overqualification (-0.017) turns out to be similar to the estimate of the baseline model, but it is less precise and statistically significant only at the 90% confidence level. In column (5) I not allow regional employment density to vary over time and the coefficient turns out to be smaller (in absolute value) and not anymore statistically significant. When I exclude the spells of those changing region but not job, the size of the effect increases slightly and both the time-varying and the average employment density coefficients become statistically significant at the 90% confidence level. Differently from overqualification, the impact of employment density on horizontal mismatch appears to vanish in the fixed effects model.

Thus the fixed effects estimate turns out to be similar to the baseline LPM regression for overqualification, but almost zero for horizontal mismatch. As said, while the fixed effects estimation gets rid of the omitted ability bias, it relies on the assumption that the individual reasons to change region do not differ systematically depending on the move to a bigger or smaller region. If the same individual moving to a larger city because of a better job match then returns to his place of childhood at the cost of a worse match (e.g. to take care of the parents), this will of course affect the results.

3.3.4 Addressing Potential Reverse Causality

Another potential bias arising in the estimation of agglomeration economies is reverse causality. In fact, higher local wages or areas with better matching could attract workers and increase local labor supply and thus employment density (Combes and Gobillon, 2015). Following several studies in the literature I use historical population values as an instrument for current density (Andini et al., 2013; Ciccone and Hall, 1996; Combes et al., 2008). Specifically, I employ population data from the 1880 census of all cities and towns with more than 10,000 inhabitants.[12] The cities and towns from 1880 are then matched to actual regional labor markets. In regional labor markets, for which no city is listed in the 1880 census, a population of 10,000 is imputed. As it will be shown, the instruments are relevant because of inertia of the local population. However, because of the profound changes that affected Germany over the past century, I am confident that the instrument is exogenous to the current match. The country experienced a structural shift from agriculture to manufacturing and services, dramatic political and administrative changes, two world wars that strongly reshaped its borders, as well as the separation and reunification of East and West Germany.

Table 3.4: Impact of Employment Density on Qualification Mismatch: IV Regressions

	Overqualification		Horizontal mismatch	
	2SLS (1)	FE IV (2)	2SLS (3)	FE IV (4)
Empl. density (log.)	-0.028***	-0.018*	-0.023***	0.006
	(0.006)	(0.009)	(0.008)	(0.010)
	First-stage equation			
1880 Population (log.)	0.679***	0.710***	0.679***	0.710***
	(0.008)	(0.003)	(0.008)	(0.003)
Control variables	Yes	Yes[a]	Yes	Yes[a]
Occupation FE	No	No	No	No
Observations	35,363	35,363	35,363	35,363

Note: Standard errors are clustered at the individual level in the cross-sectional regressions and at the individual level in the panel regressions; *** p<0.01, ** p<0.05, * p<0.1; Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies). [a] Only control variables that change over time are included.

Column (1) of Table 3.4 shows the results of a two-stage least square regression on overqualification, where the log of employment density is instrumented by the log of the (city) population in 1880. The coefficient of the 1880 population in the first-stage equation is large and significant and the F-statistics on the excluded instruments is equal to

[12]The data was collected from the Statistical Yearbooks of the German Empire (Statistisches Jahrbuch für das Deutsche Reich).

6648. The coefficient of employment density in the overqualification regression is negative, statistically significant and even larger in absolute value than in the the baseline regression. Column (3) shows the results of the same model for horizontal mismatch. Also in this case the coefficient is negative, statistically significant and larger than the baseline estimate. Column (2) and (4) report the results for fixed-effects models, where employment density is instrumented with historic population. Again the results are similar to the fixed-effects equation. The employment density is negative and significant (at the 90% confidence level) in the overqualification regression and not significantly different from zero in the horizontal mismatch regression (similar to the baseline model).

3.3.5 Further Robustness Checks

I carry out several robustness checks to ensure that the main results are not driven by outliers and specific regions. Given that regions in East Germany have on average a higher mismatch and a lower employment density, it is important to test if the results are driven by differences between East and West Germany. Estimating the baseline regression only for West Germany gives very similar estimates for overqualification, while the horizontal mismatch estimate is slightly smaller (in absolute values) and not statistically significant (see Table 3.A.2). Very similar results are also found when leaving out Berlin, which is the largest agglomeration. I then test if the results are comparable for younger and older workers and split the sample into those younger and older than 45. The results do not change for overqualification, while the density estimate for individuals younger than 45 in the horizontal mismatch regression is similar but not statistically significant. This shows again the robustness of the results for overqualification, while the outcomes for horizontal mismatch are much less precise and not always statistically significant.

While one would ideally use the density of the place of work, in this paper only the place of residence is observed. I am confident that the vast majority of individuals live and work in the same labor market region, as the definition of the regional level that I employ is based on commuting patterns. However, not observing the place of work can lead to a certain degree of measurement error. This is likely to lead to an attenuation bias, as for any type of measurement error in the explanatory variable. In the present setting, there are also reasons to believe that the employment density in larger cities is underestimated, since individuals are more likely to commute to denser regions. The attenuation bias could thus get even larger. While there is no information on the place of work, there is information on commuting distances for a sub-sample of the population. To minimize the error, I drop those individuals who commute long distances and are thus likely to work in a different region from the one of residence. However, one needs to bear in mind that individuals in less dense areas are more likely to commute, even within the regional labor market, and commuters are more likely to have a better match. So, if commuters within the regional labor market are dropped, one could actually induce a further attenuation bias. When excluding individuals commuting more than 50 km we get results that are about 10% larger in the overqualification equation and very similar in the horizontal mismatch equation (see Table 3.A.3). When dropping all individuals commuting more than 25 km the employment density coefficient gets smaller than the baseline both for horizontal mismatch and overqualification, suggesting that the attenuation bias gets larger when restricting the sample to individuals commuting short distances. These results suggest that the main estimates suffer from a certain degree of attenuation bias. Nevertheless, it is very difficult to estimate the bias precisely with the data at hand.

In this paper I use employment density at the regional labor market level (258 regions) to measure agglomeration. However, the main results are very similar when employing population density as an agglomeration measure (see Table 3.A.4). The baseline results are also qualitatively very similar if I employ a broader definition of local labor markets (150 regions) or the finer administrative definition of 402 districts. Finally, even a binary variable distinguishing urban from rural districts leads to negative and significant results.[13] The likelihood to be overqualified is about 20% lower in urban regions, while horizontal mismatch is about 11% lower.

3.3.6 Heterogeneous Effects by Education Level

So far I have estimated the impact of employment density on qualification mismatch without distinguishing among individuals with a different highest qualification. However, denser regions have typically a higher share of high-skilled individuals and if the qualification mismatch measures differ across individuals with different levels of education this is likely to lead to biased results. Moreover, it would be interesting to analyze whether the effect of agglomeration on better matches differs between tertiary graduates and individuals

[13]The urban variable is based on the definition of the Federal Office for Building and Regional Planning.

with a vocational degree. To analyze heterogeneous effects by qualification level I first add an interaction term to the baseline regression and then estimate separate regressions for individuals with a tertiary education degree and for those who have a vocational degree as their highest qualification.

Better educated individuals are more likely to be overqualified in our sample (see Figure 3.A.1 for more details). Conversely, the incidence of horizontal mismatch differs substantially across qualifications and is much higher for individuals with a vocational degree compared to university graduates. Even if I controlled for the highest degree obtained in the baseline specifications, it is very important to make sure that the results obtained are not biased from the different composition of qualified labor across regions.

To better address these compositional issues I first add an interaction term between employment density and the highest degree obtained in the baseline model for the whole sample considered. Columns (1) and (2) of Table 3.5 show the results of this estimation without and with the inclusion of occupation fixed effects. For simplicity, I include only the interaction between employment density and vocational qualification, so that I can interpret the employment density coefficient as the effect for tertiary graduates. The regressions for overqualification (panel A) show estimates for tertiary graduates that are similar but slightly higher than the baseline estimates for the full sample. Conversely, the estimations for horizontal mismatch show a zero effect of density on horizontal mismatch for tertiary graduates. Thus, if there is a significant impact of agglomeration on horizontal mismatch, this seems to be only present for individuals with a vocational degree.

The results of the separate regressions on the sub-sample of tertiary graduates and on that of individuals with a vocational degree as their highest qualification are fairly similar to the model with interaction terms. However, as regards overqualification, the estimates are now very similar for the two education groups. As regards horizontal mismatch, the coefficient for individuals with a vocational degree is larger than the baseline estimates and statistically significant at standard confidence levels. For tertiary graduates, again there does not seem to be any difference in the horizontal mismatch between smaller and larger cities. This could also point to the fact that this type of measure is not always a "real" job mismatch for university graduates. In fact, even if graduates that are horizontally mismatched earn on average less than better matched graduates, some individuals in highly remunerated jobs also report being mismatched with respect to their field of study.

Table 3.5: Impact of Employment Density on Qualification Mismatch by Qualification Level

	All degrees		Vocational degree		Tertiary degree	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Overqualification					
Empl. density (log.)	-0.025***	-0.025***	-0.020***	-0.012***	-0.020**	-0.014*
	(0.010)	(0.009)	(0.005)	(0.005)	(0.010)	(0.008)
Vocational degree	-0.291***	-0.466***				
	(0.061)	(0.055)				
Vocational degree ×	0.008	0.019*				
Empl. density	(0.011)	(0.010)				
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes	No	Yes
Observations	35,363	35,363	24,277	24,277	11,290	11,290
	Panel B: Horizontal mismatch					
Empl. density (log.)	-0.012	-0.011	-0.019**	-0.015**	-0.011	-0.009
	(0.010)	(0.009)	(0.008)	(0.007)	(0.010)	(0.010)
Vocational degree	0.119*	0.073				
	(0.066)	(0.065)				
Vocational degree ×	-0.006	-0.003				
Empl. density	(0.012)	(0.012)				
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes	No	Yes
Observations	35,363	35,363	24,277	24,140	11,223	11,223

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variables. Standard errors are clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

3.4 Determinants of the Qualification Mismatch Differential across Regions

So far I have established that thick labor markets reduce the probability of workers being overqualified for their job. In this section I investigate the channels that contribute to the mismatch differential across cities. More precisely, I am interested in highlighting those characteristics of denser regions (apart from a pure market size effect) that contribute to better average job matches. I did not include those characteristics in the previous chapters, because I consider these to be outcomes or intrinsic characteristics of larger cities. However, from a theoretical perspective it is very important to try to disentangle agglomeration economies and localization economies, as well as to separate the agglomeration economies due to better matches from those due to knowledge spillovers.

Denser regions typically have a higher proportion of high-skilled individuals. On the one hand, one would like to exclude the effects of skills from agglomeration economies, as far as this represents a pure composition effect (Combes and Gobillon, 2015). High-skilled individuals might be over-represented in cities, because they value city amenities more or because of historical migration of high-skilled individuals (with their skills being partly transmitted to their children). On the other hand, people could become more skilled through living in cities, thanks to stronger learning effects in denser regions. Faster learning and knowledge diffusion is indeed one of the main mechanisms of agglomeration economies (De La Roca and Puga, 2017). In our setting, it is interesting to analyze the qualification mismatch differential across regions while keeping the regional skill composition fixed. Column (2) in Table 3.6 shows the results of our baseline regression augmented with the regional share of tertiary educated individuals in the workforce. This variable has often been used in the literature to account for knowledge spillovers (Moretti, 2004a). A higher share of high-skilled workers is associated with a lower risk of overqualification (but the coefficient is not statistically significant), probably because of a greater availability of high-skilled jobs. Controlling for skill composition, the employment density coefficient drops (in absolute value) to -0.012 but remains statistically significant at the 90% confidence level. Conversely, the high-skill share coefficient is positive and not statistically significant in the horizontal mismatch equation, so that the employment density estimate increases slightly in absolute value.

Previous studies have often included an index of industrial concentration or diversity in order to isolate the effect of agglomeration economies from urban specialization (localization economies) (see for instance D'Costa and Overman, 2014). I thus include an index of industrial concentration of the region (Herfindahl-Hirschman-Index, HHI) based on the regional share of employment in 7 major industries in column (3) of Table 3.6. The coefficient of the industrial concentration turns out to be negative in both equations but is not statistically significant. The effect of employment density on qualification mismatch appears thus to be robust to the inclusion of this variable.

Firm size has been found to be an important determinant of the urban wage gap in Germany (Lehmer and Möller, 2010). In column (4) I also add two dummies for firm

Table 3.6: Determinants of the Qualification Mismatch Differential

	(1)	(2)	(3)	(4)	(5)
		Panel A: Overqualification			
Empl. density (log.)	-0.019***	-0.012*	-0.018***	-0.019***	-0.011*
	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)
High-skilled share		-0.005*			-0.005**
		(0.002)			(0.002)
HHI industry			-0.324		-0.386
			(0.268)		(0.270)
Large firm (>200 empl.)				0.005	0.006
				(0.010)	(0.010)
Small firm (<=20 empl.)				-0.008	-0.007
				(0.012)	(0.012)
		Panel B: Horizontal mismatch			
Empl. density (log.)	-0.015**	-0.018**	-0.014**	-0.018***	-0.019**
	(0.006)	(0.008)	(0.006)	(0.006)	(0.008)
High-skilled share		0.002			0.001
		(0.003)			(0.003)
HHI industry			-0.439		-0.442
			(0.353)		(0.353)
Large firm (>200 empl.)				0.046***	0.046***
				(0.012)	(0.012)
Small firm (<=20 empl.)				-0.086***	-0.086***
				(0.016)	(0.016)
Control variables	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No	No
Observations	35,363	35,363	35,363	35,363	35,363

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variables. Standard errors are clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

size to see if the lower mismatch incidence in denser regions can be partly explained by the presence of larger firms. The firm size coefficients turn out not to be significant in the overqualification equation and the main results are not affected. On the contrary, horizontal mismatch turns out to be more common in larger firms. Since firms are on average larger in thicker labor markets, the employment density coefficient increases (in absolute value) in the horizontal mismatch equation.

Finally, column (5) presents the results of a regression in which all discussed determinants are included. The coefficient of the variables included do not lose their magnitude suggesting that they affect the two mismatch variables through different channels. The coefficient of horizontal mismatch does not change substantially compared to the baseline equation, suggesting that it can be interpreted as a pure labor market size effect. Conversely, the overqualification effect drops by almost one half when the regional high-skilled share is included. Part of the agglomeration effect found in the baseline is thus associated with a different skill composition in denser regions.

3.5 Qualification Mismatch and the Urban Wage Premium

I will now investigate the importance of qualification mismatch as a mechanism of agglomeration economies. More precisely, I analyze what portion of the effect of regional employment density on earnings can be explained by better job matches with respect to workers' qualifications. To do so, I first estimate an OLS regression with hourly wages as the dependent variable, the regional employment density as the variable of interest and the full set of control variables presented in the previous sections. I then add to this regression our measures of qualification mismatch and look at how the coefficient for employment density is affected. Since I found a relatively large effect of employment density on overqualification and the overeducation literature documents a strong negative relationship between overqualification and wages, I expect that a large part of the effect of regional employment density on wages can be explained by a lower probability of being overqualified.

Table 3.7 shows the results of these regressions, where both employment density and hourly wages are expressed in logarithmic form. The coefficient of employment density in column (1) is equal to 0.049 and is significant at the 1% significance level. Doubling the number of employed workers per square kilometer is associated with an increase in wages of about 5%. This result appears to be in line with previous studies which estimate the magnitude of urban agglomeration economies as ranging from 0.02 to 0.07 (Combes and Gobillon, 2015). In column (2) and (3) I add our measure of overqualification and horizontal mismatch separately. Consistent with the literature, vertical and horizontal mismatch are both associated with lower wages. As expected the coefficient of employment density decreases in both specifications. However, the decrease is relatively small. According to the estimations, overqualification accounts for about 6% of the urban wage premium while horizontal mismatch accounts for less then 2%. In column (4) both mismatch measures are added and it becomes clear that they are positively correlated, since their coefficients decrease significantly. Their impact on the wage premium does not appear to add up, suggesting that they explain slightly more than 6% of the effect of employment density on wages. In column (5) I also add an interaction term between employment density and the mismatch variables. The results show that the regional difference in wages are significantly smaller for overqualified workers. Doubling employment density is associated with about 3% higher wages for the overqualified compared to 5% higher wages for those that are well-matched according to their qualifications.

Table 3.7: Impact of Employment Density and Mismatch on Log Hourly Wages

	(1)	(2)	(3)	(4)	(5)
Empl. density (log.)	0.049***	0.046***	0.048***	0.046***	0.049***
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
Overqualification		-0.156***		-0.154***	-0.062
		(0.009)		(0.009)	(0.048)
Horizontal mis.			-0.053***	-0.004	-0.002
			(0.008)	(0.008)	(0.040)
Overqualification ×					-0.019*
Empl. density					(0.010)
Horizontal mis. ×					-0.000
Empl. density					(0.008)
Control variables	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No	No
Observations	34,204	34,204	34,204	34,204	34,204
R-squared	0.512	0.530	0.515	0.530	0.530

Note: Standard errors are clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

The wage results are consistent with previous studies using different measures of skill mismatch, which found that it only accounts for about 5-8% (Abel and Deitz, 2015) of the urban wage premium or has almost no contribution at all (Boualam, 2014). Overqualification seems to be the most important channel here, while horizontal mismatch does not seem to add much to this. Furthermore, knowing that less talented individuals are more likely to be overqualified (Leuven and Oosterbeek, 2011), part of this explained effect might actually denote unobserved ability. Indeed, due to spatial sorting, controlling for ability is expected to lead to a decrease in the coefficient of the urban wage premium and the overqualification dummy might to some extent proxy unobserved ability.

3.6 Conclusion

The aim of this paper is to measure the effect of local labor market size on vertical and horizontal qualification mismatch. Estimating a linear probability model with an extensive set of control variables, I find that German male workers who live in denser regions are less likely to be overqualified and to work in a different field than that of their education or training. The impact on overqualification is robust to the inclusion of an extensive set of control variables (including school grades, personality traits and risk preference) and is relatively large. An increase of 10% in the regional employment density is associated with a decrease of 1-1.5% in the overqualification incidence. The impact of horizontal mismatch is slightly smaller and less precise (not statistically significant in some specifications). I then follow three empirical strategies to deal with the fact that talented workers might sort into larger cities. First, by restricting the sample to individuals that remain in the area they grew up in, I get a smaller but still sizable estimate of the effect of employment density on overqualification and a similar estimate for horizontal mismatch. Second, by exploiting the panel structure of the data and accounting for individual fixed effects, I get a coefficient for overqualification that is less precise, but very similar in size to the baseline regressions. Conversely, the estimate in the horizontal mismatch regression is not statistically different from zero. Third, I address potential reverse causality by instrumenting current employment density with historical population data. The IV regression estimates are very similar to the baseline and fixed effects regression results.

While differences in the regional skill composition account for a large portion of the match differential across regions, most of the impact found seems to be attributable to a pure labor market size effect. Finally, I investigate the extent to which lower qualification mismatch in large agglomerations contributes to the urban wage premium. I find that overqualification explains about 6% of the impact of regional employment density on hourly wages, while the contribution of horizontal mismatch appears to be insignificant.

3.A Further Tables and Figures

Figure 3.A.1: Qualification Mismatch by Highest Degree

Figure 3.A.2: Employment Density in German Labor Market Regions (in 2010)

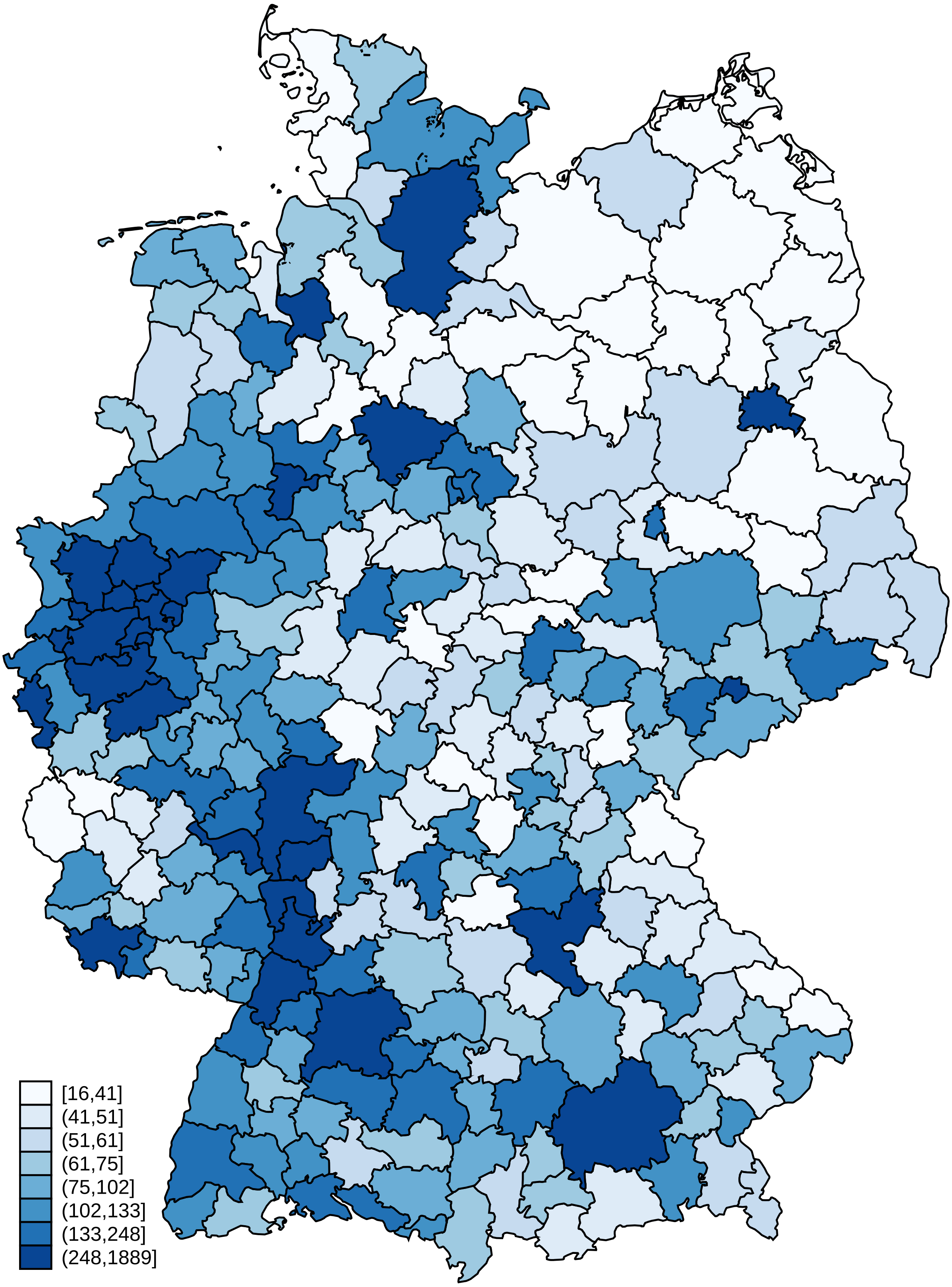

Table 3.A.1: Impact of Employment Density on Overqualification: Further Controls

	Overqualification				Horizontal mismatch			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Empl. density (log.)	-0.020***	-0.022***	-0.020**	-0.020**	-0.012	-0.013	-0.013	-0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
School grade: Math		-0.013*	-0.013*	-0.013*		-0.006	-0.004	-0.004
		(0.008)	(0.008)	(0.008)		(0.010)	(0.010)	(0.010)
School grade: German		-0.019**	-0.017*	-0.017*		-0.018	-0.019	-0.018
		(0.010)	(0.009)	(0.009)		(0.012)	(0.012)	(0.012)
Extraversion			0.002	0.002			0.003	0.003
			(0.002)	(0.003)			(0.003)	(0.003)
Consciousness			0.003	0.003			-0.003	-0.003
			(0.003)	(0.003)			(0.004)	(0.004)
Agreableness			0.003	0.003			0.007*	0.007**
			(0.003)	(0.003)			(0.004)	(0.004)
Neuroticism			0.005**	0.005**			0.008***	0.008***
			(0.002)	(0.002)			(0.003)	(0.003)
Openness to experience			-0.005*	-0.005*			-0.001	-0.001
			(0.002)	(0.003)			(0.003)	(0.003)
Risk aversion				-0.000				0.005
				(0.005)				(0.006)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No	No	No	No	No
Observations	14,814	14,814	14,814	14,814	14,814	14,814	14,814	14,814
R-squared	0.092	0.095	0.098	0.098	0.094	0.095	0.099	0.99

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variables. Standard errors are clustered at regional labor market level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

Table 3.A.2: Excluding Specific Regions or Age Groups

	Excluding regions		Excluding age groups	
	only west (1)	excl. Berlin (2)	45 or younger (3)	over 45 (4)
	Overqualification			
Empl. density (log.)	-0.019**	-0.018***	-0.018***	-0.020***
	(-0.007)	(0.006)	(0.006)	(0.007)
	Horizontal mismatch			
Empl. density (log.)	-0.013	-0.011	-0.013*	-0.016*
	(0.008)	(0.008)	(0.008)	(0.009)
Control variables	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No
Observations	25,780	34,215	18,449	16,914

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variable. Standard errors are clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

Table 3.A.3: Excluding Long-Distance Commuters

	Commuters excluded			
	None (1)	>50 km (2)	>30 km (3)	> 20 km (4)
	Overqualification			
Empl. density (log.)	-0.020***	-0.022***	-0.021***	-0.018***
	(0.005)	(0.005)	(0.006)	(0.006)
	Horizontal mismatch			
Empl. density (log.)	-0.016**	-0.017**	-0.014**	-0.012*
	(0.006)	(0.007)	(0.007)	(0.007)
Control variables	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No
Observations	33,313	30,406	26,947	22,688

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variables. Standard errors are clustered at the individual level; *** p<0.01, ** p<0.05, * p<0.1. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

Table 3.A.4: Other Agglomeration Measures

	258 regions	150 regions	402 districts	
	(1)	(2)	(3)	(4)
	Overqualification			
Pop. Density (log.)	-0.019***			
	(0.005)			
Emp. Density (log.)		-0.020***	-0.014***	
		(0.006)	(0.003)	
Urban region				-0.040***
				(0.010)
	Horizontal mismatch			
Pop. Density (log.)	-0.016**			
	(0.007)			
Emp. Density (log.)		-0.023***	-0.015***	
		(0.008)	(0.008)	
Urban region				-0.034***
				(0.013)
Control variables	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No
Observations	35,363	35,363	35,363	35,363

Note: The table shows the estimates of a linear probability model with skill mismatch measures as dependent variables. Standard errors are clustered at the individual level; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables included are the main control variables (highest degree, migration background, marital status and children, experience, experience squared and year fixed effects), school degree, parental background (higher education of mother/father and working status of mother), geographic characteristics (macro-regions and whether living in city of childhood) and job characteristics (tenure, tenure squared, industry and public sector dummies).

Chapter 4

College Openings and Local Economic Development

4.1 Introduction

Income and unemployment rates differ substantially across cities and regions in most countries. In the United States, for example, wages in the highest and lowest paying metropolitan areas differ by a factor of three (Moretti, 2011). Similar discrepancies in income per capita are observed between regions in the European Union (OECD, 2009). The variation in unemployment rates is even larger. In Germany, for example, local unemployment rates across metropolitan areas vary by a factor of five or even six (BMWi, 2013).

In response to these large disparities, many governments promote policies aimed at reducing regional inequalities. These place-based policies could be direct subsidies to firms located in or planning to move into a disadvantaged area. Alternatively, they can take the form of investments in infrastructure by local, state or federal governments in order to increase the economic attractiveness of a region. One such local policy is the opening of educational institutions like universities or colleges in a region. The opening of new universities and colleges could be a powerful tool for regional development for at least three reasons: first, by improving the human capital base in the region, they may attract new

This chapter is joint work with Christina Gathmann and Verena Lauber. We thank Susanna Bolz and Patrick Colli for research assistance, as well as Henry Overman, Sascha Becker, Pat Kline and participants at the CESifo Summer Institute, Verein für Socialpolitik, EALE and the ZEW for very helpful comments and suggestions. We are responsible for all remaining errors.

investments or plants to the region. Second, staff and students as well as the universities and colleges themselves may stimulate the demand for local goods and services. Finally, universities and colleges might generate positive spillover effects on local firms through research collaborations or knowledge spillovers, for instance.

Yet, do colleges really promote regional economic development in disadvantaged regions? And if so, what are the channels: is it the direct consequence of the educational institution itself? Or, does an educational institution create spillover effects by raising labor demand and by encouraging innovation in private firms in the region?

Our study sheds light on these important questions. An important challenge in this endeavor is that the location of colleges is not random. As numerous universities and colleges were originally established many decades ago, it is difficult to isolate the impact of a university from other cumulative developments in the local economy. We solve this identification problem in two steps: first, we use the establishment of new colleges in Germany during the 1980s and early 1990s to study the short- and medium-run impact on regional development. Using college openings, we can trace the evolution of skilled labor and other adjustments in the local labor market and economy more broadly. Nevertheless, regions that obtain a new college might differ from other regions without a college opening in terms of prior employment, wages and innovative capacity. In a second step, we therefore combine matching with a time-varying difference-in-differences approach. Our empirical approach allows us to compare flexibly employment and wages in regions with a college opening to employment and wages in suitable control regions that did not obtain a new college.

We have three main findings. First, the opening of a technical college results in large, persistent growth in the regional student population relative to the control regions. In line with the notion that college openings change the composition of the workforce, we find that high-skilled employment in the region increases by 12% eight to nine years after the opening. Second, we find evidence that new colleges also raise the employment of workers without a college degree suggesting complementarities in the local production function. Third, we find that wages of high-skilled workers do not decrease in the medium run, indicating a shift in local demand. The employment and wage effects remain if we exclude all employees working in the education sector indicating that a college opening has an impact on the local economy beyond creating additional jobs in teaching and research. The large increase in employment and the lack of a drop in wages instead point to sizable adjustments on the labor demand side. We find no employment effect in new establishments, suggesting that most of the adjustments happen in incumbent firms either through changes in the output mix or through changes in technology.

Our article is related to several strands of the literature. A sizable amount of literature has documented the correlation between the location of universities and patenting activity, innovation and business start-ups (Andersson et al., 2009; Audretsch and Feldman, 1996; Bania et al., 1993; Cohen et al., 2002; Jaffe, 1989; Woodward et al., 2006). Most studies focus on the importance of academic research for the development of specific local industries, such as pharmaceuticals or electronic equipment.[1] Closer to our analysis are two studies specifically on universities. Beeson and Montgomery (1993) study the link between the quality of a university and local employment growth. Kantor and Whalley (2014a) use shocks to a university's financial endowment to identify wage effects outside the education sector.[2] The analysis in this paper uses a different, plausibly exogenous variation to identify the link between universities, local employment and innovation: the opening of new technical colleges. A second way in which this paper differs from most of the literature is that we investigate the impact of colleges focused on applied research but concentrate on their impact on the local economy as a whole - rather than just on specific, technology-driven industries.

Furthermore, our analysis contributes to a sizable literature on the local effects of labor supply shocks. A large number of studies in labor economics analyze how inflows of immigrants into a region affect local wages (Borjas, 2003; Card, 2001; Glitz, 2012; Manacorda et al., 2012; Ottaviano and Peri, 2012). Most studies using the local area approach suggest that immigrants have a small effect on the local wages of natives (but see Dustmann et al., 2017, who show that employment effects on natives may be sizable in remote areas). Traditional open economy models instead emphasize adjustments to labor supply shocks through changes in the output mix produced by the local economy. Regions with a relative growth in low-skilled labor, for instance, experience shifts to products and sectors that make intensive use of low-skilled labor. A third adjustment mechanism is that firms adjust their production technology either through changes in the actual technology

[1] Valero and Van Reenen (2016) use a global dataset to explore the link between research universities and economic growth.

[2] Kantor and Whalley (2014b) take a long-term view tracing the role of experiments in agricultural technology for the evolution of agricultural activity over more than a century.

of production (Acemoglu, 1998) or by redirecting research efforts (see, e.g. Beaudry and Green, 2003, 2005; Caselli and Coleman, 2006). Evidence on technology adoptions suggests that automation machinery does indeed expand more slowly in areas with high growth rates in the relative supply of low-skilled labor (Lewis, 2011) and that skill abundance leads to a faster adoption of new technologies (Beaudry et al., 2010).

As immigrants in most countries are on average less skilled than the native population, these studies focus on adjustments to an increase in low-skilled labor (but see Ciccone and Peri, 2011, which analyzes the growth in the number of medium-skilled workers following compulsory schooling laws). Our study contributes to this literature by exploiting the opening of new colleges as a plausibly exogenous shock to the high-skilled workforce in the local labor market (see Carneiro et al., 2015). The adjustments to a low-skill supply shock are likely to differ from the response to a shock to high-skilled labor if there are human capital externalities or other types of knowledge spillovers. If high-skilled workers raise the productivity or innovative capacity of other workers in the same firm or other firms in the region, for instance, the effects on the local economy might be much larger than a growth in the low-skilled workforce (Ciccone and Peri, 2006; Moretti, 2004b).

Another reason why college openings may foster economic growth in the region are local multiplier effects: an increase in local employment may raise the demand for local goods and services. The local multiplier effect could be especially strong in the case of a college opening because much of the additional employment is concentrated among high-skilled workers with more disposable income. Recent research suggests that local multiplier effects may be sizable (see, for example, Moretti, 2010, for the US; Moretti and Thulin, 2013, for Sweden; and Faggio and Overman, 2014, for the UK). Our study relies on a different, plausibly exogenous source of identification to investigate how the creation of new college-related jobs generates additional benefits in the non-tradable sector.

Finally, knowing whether an increase in high-skilled labor improves the economic conditions of other workers in that region and the regional economy overall has important policy implications. If there are indeed positive externalities from college openings on the local economy, this could be one argument for public subsidies for tertiary education.[3] In addition, our results may also be important for the design of regional policies. National

[3] We focus on the pecuniary benefits of an increase in the human capital stock of a region; there might be other spillover effects, such as reduced criminal activity or increased political participation, for instance, which we do not consider here (see e.g. Valero and Van Reenen, 2016).

and state governments often use regional policies to support areas with high unemployment and low economic growth. Prominent examples include region-specific subsidies to firms or local governments, such as the Federal Empowerment Zones in the US (Busso et al., 2013), regional subsidy programs in France (Gobillon et al., 2012), Italy (Bronzini and Blasio, 2012), the UK (Criscuolo et al., 2012) or Germany (von Ehrlich and Seidel, 2015); or the European Structural Funds (Becker et al., 2013; Becker et al., 2010).[4] Our research can shed new light on the question of whether public investments like the opening a new college can improve employment prospects and local development in a region and thus contribute to a decline in regional disparities.

The paper proceeds as follows. The next section introduces the empirical setting of the college openings we analyze. Section 4.3 presents our data sources and discusses the empirical strategy we use to identify the effects of a college opening on the local economy. Section 4.4 presents the results. Finally, Section 4.5 discusses the implications of our findings and concludes.

4.2 College Openings in West Germany

In 1968, the federal government of Germany decided to expand the tertiary education sector in order to improve the competitiveness of Germany's industry by increasing the human capital base.

The resulting reform focused on the nationwide establishment of technical colleges (*Fachhochschulen*) to complement tertiary education at regular universities.[5] As student capacity at existing universities and colleges was exhausted, policy-makers turned to the establishment of new institutions in the face of rising demand for post-secondary education. Technical colleges were considered particularly suitable for this purpose because of their close ties to the local economy. Unlike universities, technical colleges focus on teaching. Degree programs at technical colleges last three to four years and combine academic study with periods of practical training allowing students to gain work experience. It was only in the 1990s that technical colleges became more actively engaged in research activities.

[4] Earlier work has focused on the relationship between city (or local industry) size or density and productivity more generally (see, for example, Ciccone and Hall, 1996, for a seminal contribution and Rosenthal and Strange, 2004, for a survey).

[5] see "Abkommen der Länder in der Bundesrepublik Deutschland zur Vereinheitlichung auf dem Gebiet des Fachhochschulwesens"(see also Wissenschaftsrat, 1991).

Teaching and also research are more practice-oriented than at universities and are often conducted in cooperation with local companies. In 2003, 31% of third-party funding in technical colleges came from private companies, while it is only 20% in universities (Haug and Hetmeier, 2003).

Technical colleges rank below universities in terms of pay and status. However, the teaching staff have PhDs and, in addition, unlike at universities, several years of practical work experience. Unlike universities, technical colleges also cover a more narrow set of academic subjects. In 2001, 42% of all students of technical colleges were enrolled in economics, social sciences or law, while around 30% were studying engineering (Haug and Hetmeier, 2003).[6]

While the costs for establishing and running the new colleges were divided between the federal and the respective state governments (see the law of 1969, *Hochschulbauförderungsgesetz*), the individual states were responsible for setting up the new colleges. Municipalities and local governments had only limited influence on the decision where a new college would be located. A common goal in all states was to distribute post-secondary educational institutions more evenly across space in order to reduce costs and facilitate access to tertiary education. Our data show that new colleges were indeed predominantly opened in rural and semi-rural districts with no university or technical college (see Table 4.1 and Figure 4.A.1).

Local economic performance also played a role in the decision-making process. The perceived risk of structural problems was often taken as an explicit criterion for the selection of suitable regions. It was hoped that colleges would attract new businesses and prevent out-migration of potential students and other workers (Landtag, 1991; Schindler et al., 1991; Wissenschaftsrat, 1995).[7] Technical colleges were also seen as a tool to meet the demand for high-skilled workers.[8]

Yet, targeted areas also had to fulfill certain criteria that would not apply to the least developed regions. A prerequisite was that the demand for the degree programs offered

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[6] Admission criteria for a technical college is a high school degree or a more specialized secondary school degree, which are both obtained after 12 or 13 years of schooling. Given their more practice-oriented nature, technical colleges cannot award doctoral degrees.

[7] In North Rhine-Westphalia, for example, policymakers hoped to speed up the structural transformation from coal and steel to other industries and services (Holuscha, 2012). In a similar fashion, policymakers in Rhineland-Palatinate hoped to counterbalance structural changes (Wissenschaftsrat, 1995).

[8] The employers' federation were typically in favor of opening new colleges (see e.g. Landtag, 1991; Wissenschaftsrat, 1994).

was deemed adequate and that a sufficient number of students and teaching staff could be attracted in the catchment area. Local industry was required to cooperate with the college and to offer jobs to graduates. Whether or not this was plausible was assessed on the basis of measures of economic viability, infrastructure like transportation and overall attractiveness of the region (see, e.g. Landtag, 1991; Schindler et al., 1991). In some cases, technical colleges mainly served as a political bargaining tool: technical colleges were used to compensate the Bonn area when the federal government moved to Berlin, for instance (Wissenschaftsrat, 1996).

Based on the discussion above, we expect that the average economic performance of regions with a new college would not differ much from the performance of other districts, which is clearly supported by our data (see Table 4.1). However, technical colleges are more likely to be established outside of metropolitan areas. Hence, regions with a college opening are unlikely to be similar to the average region in West Germany in terms of wages, employment and industry structure, for instance. In Section 4.3.2, we use a matching procedure to select suitable control regions that have a similar industry and employment structure in the years before the opening to that of districts that eventually get a new technical college.

For our empirical analysis, we focus on college openings in the 1980s and 1990s. Earlier openings often happened to be transformations of former vocational schools or similar institutions for secondary education. We expect those adjustments to have a small and relatively smooth effect on the regional supply of high-skilled workers (see Kulicke and Stahlecker, 2004; Wissenschaftsrat, 1991). Furthermore, we cannot trace openings prior to 1980 as the social security data start in 1975 and we require up to four years before the opening for our estimation approach. Our analysis is further restricted to publicly funded colleges which were created following the federal law of college expansion. Private colleges make up only a small fraction of tertiary education in Germany. These institutions are often very small and cover only a narrow range of subjects. As a result, their founding does not generate a sizable shift in local labor supply.[9]

In 1984, there were 58 public technical colleges with in total 97 campuses in West Germany. By 2004, the number of campuses had increased to 123 located in 110 West German districts. Of the 26 openings of a new campus, we analyze 23 events in 21 districts.

[9] Finally, the founding of private institutions is often financed (or co-financed) by local companies and hence, much more likely to be correlated with labor demand.

Two college openings took place in the same district and year which we combine into a single event.[10] In two other districts, our matching procedure was unable to identify suitable control regions (see section 4.3.2 for further details).[11] Finally, we drop one opening of a small campus because the post-treatment period overlaps with the opening of a larger campus in the same district.[12] Table 4.A.1 provides a list of the new colleges, the district in which they were founded and the year of the opening. Our sample of colleges is relatively small as are most technical colleges. Five years after the opening, the new technical colleges have 98 employees as teaching and support staff on average. The total number of students and employees of technical colleges amounts to just about 2.8% of local employment eight to nine years after the college opening.

4.3 Data Sources and Empirical Strategy

4.3.1 Data Sources

To analyze the consequences of college openings empirically, we use German Social Security Records over more than three decades. More specifically, we draw on employment data from the German Establishment History Panel (BHP), a 50% random sample of all establishments with at least one employee covered by the social security system in Germany (see Schmucker et al., 2016, for more details). Civil servants, military personnel and the self-employed are not included.[13] The data have been available annually since 1975 which enables us to compare districts with a new college opening to suitable control regions without an opening several years before the opening. Another advantage of our dataset is that we observe the location of each plant. We can thus identify whether an establishment is potentially affected by a college opening in the same district. We further observe the detailed industry of an establishment which allows us to analyze whether the effects of a new college affect the local economy as a whole or specific industries. To study whether labor demand changes in new or incumbent plants, we rely on the procedure developed by Hethey and Schmieder (2010) to distinguish plant openings from a simple change in

[10] These are the district of Göppingen in 1988 and the district of Rhein-Sieg in 1995.

[11] These are the district of Salzgitter and the district of Emsland, where new campuses for existing technical colleges were opened in 1993 and 1995 respectively.

[12] This is the campus in Idar-Oberstein that was opened in 1986 in the district of Birkenfeld.

[13] The social security data cover around 80% of the German labor force.

the establishment identifier as a consequence of spin-offs of existing establishments, for instance.

Furthermore, we have detailed information on the workforce in each establishment, especially the number of workers by skill, occupation and age. We distinguish three skill groups based on the highest qualification obtained. High-skilled workers are workers who have graduated from a college or university. Medium-skilled workers have completed a vocational training program or obtained the university entrance certificate after high school (*Abitur*). Low-skilled workers have lower qualifications or no qualifications at all. In the raw data, the education variable is missing for about 9% to 37% of the observations depending on the year. The BHP provides adjusted education variables based on standard imputation procedures reducing missing education to less than 1% (see Fitzenberger et al., 2006). We further distinguish three age groups (20-34, 35-49 and 50-64). Moreover, we proxy the R&D intensity of a plant as the number of employees in engineering and natural sciences.

Establishments also report for each employee the daily wage of the employment spell which contains the reference date (June 30 of each year). As is common in social security records, wages are right-censored at the highest level of earnings that are subject to social security contributions. We use imputed wages based on the imputation procedure of Card et al. (2013). All wages are deflated using the consumer price index with 1995 as the base year.

We aggregate establishment data to the district level which is our main unit of analysis. Overall, we observe 325 districts in West Germany with an average population of 196,000 and around 93,000 employees. Local employment refers to full-time employment on June 30 of each year; it excludes apprentices, workers in marginal employment or partial retirement. We further compute regional employment shares and levels by skill level, age and industry. We complement this data with regional information on population, total employment and gross value added which is based on the European regional Database of Cambridge Econometrics.

4.3.2 Matching Procedure

Regions that experience a new college opening are likely to differ from regions that did not get a new college. The anecdotal evidence discussed in Section 4.2 suggests that local governments had little influence on the location decision of a new college. In most cases, the location was chosen by the federal or state government which were also responsible for financing the new institution. Nevertheless, politicians at the state and national level might have chosen a location in order to foster economic development in the region. In that case, the region with a new college might have had a less favorable economic development prior to the opening.

A comparison of treatment regions (i.e. regions with a college opening) to the average district in West Germany shows that event regions differ in some important characteristics (see the first columns of Table 4.1). Specifically, regions with a college opening have a lower employment share in agriculture and fishing, a higher employment share in public administration and lower total employment (even if the difference is not statistically significant). They were also less likely to have a university or technical college in their district.

To find suitable controls for the event regions, we use a matching procedure. To each region with a new college, we match a region without a college opening that is similar to the event region in terms of its age structure (3 age groups 20-34, 35-49, 50-64), industry structure (17 broad industries), skill structure (high-, medium- and low-skilled) and population in the five years before the opening.[14] In addition, we match on an indicator whether a district had a university or college in the year prior to the college opening. Including this indicator ensures that event and control regions have similar numbers of students prior to the college opening.

To find a match for each event region, we use the Mahalanobis matching algorithm which minimizes the standardized Euclidean distance of all matching variables between treatment and control regions.[15] We then use nearest-neighbor matching with replacement, i.e. we select for each event region the control region with the smallest sum of normalized squared distances. This matching method works best if the number of matching variables is not too large (see Stuart and Rubin, 2008).

[14] We include the age and skill shares in $\tau - 1, \tau - 3$ and $\tau - 5$ where τ is the year of the college opening. Industry shares and population are measured in $\tau - 1$ and $\tau - 5$.

[15] The distances between each treatment and each potential control region are normalized by the variance-covariance matrix of the pooled sample of event and possible control regions. Normalizing by the variance-covariance matrix in the control group only does not alter the results.

Table 4.1: Treatment Versus Control Districts in the Pre-Event Period

	Treated Districts	Other West German Districts	Control Districts	Difference Treated versus Other Districts		Difference Treated versus Control Districts	
				Coeff.	S.E.	Coeff.	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Matched Characteristics							
Age:							
Age: 25-34	0.439	0.442	0.448	-0.002	(0.008)	-0.009	(0.009)
Age: 35-49	0.347	0.346	0.342	0.000	(0.007)	0.005	(0.007)
Age: 50-64	0.193	0.193	0.188	0.000	(0.005)	0.005	(0.006)
Education:							
High-Skill Share	0.055	0.065	0.053	-0.010	(0.007)	0.002	(0.005)
Medium-Skill Share	0.792	0.790	0.800	0.003	(0.009)	-0.008	(0.014)
Low-Skill Share	0.148	0.139	0.142	0.008	(0.009)	0.006	(0.016)
Industry:							
Agriculture and Fishing	0.009	0.012	0.010	-0.004*	(0.002)	-0.002	(0.002)
Energy and Mining	0.020	0.024	0.022	-0.004	(0.009)	-0.002	(0.009)
Food	0.031	0.040	0.031	-0.009	(0.006)	-0.000	(0.005)
Consumer Goods	0.084	0.066	0.081	0.018	(0.011)	0.003	(0.019)
Producer Goods	0.086	0.104	0.102	-0.018	(0.019)	-0.016	(0.019)
Investment Goods	0.179	0.175	0.175	0.004	(0.024)	0.004	(0.035)
Construction	0.098	0.105	0.110	-0.006	(0.008)	-0.012	(0.009)
Retail Trade	0.160	0.145	0.138	0.015	(0.009)	0.021*	(0.012)
Transport and Communications	0.044	0.041	0.038	0.002	(0.005)	0.006	(0.006)
Finance and Insurance	0.028	0.032	0.027	-0.004	(0.005)	0.001	(0.004)
Hotel and Restaurant Industry	0.020	0.026	0.025	-0.006	(0.005)	-0.005	(0.004)
Educational Services	0.013	0.015	0.013	-0.002	(0.002)	-0.000	(0.002)
Health and Social Services	0.083	0.078	0.096	0.005	(0.008)	-0.013	(0.012)
Corporate Services	0.050	0.053	0.042	-0.003	(0.007)	0.007	(0.008)
Other Services	0.016	0.019	0.016	-0.003	(0.003)	-0.001	(0.002)
Non-Profit Organizations	0.007	0.009	0.006	-0.002	(0.003)	0.001	(0.002)
Public Administration	0.075	0.057	0.066	0.018***	(0.007)	0.008	(0.013)
Other:							
Population (in Thousands)	162.8	195.5	167.6	-32.7	(37.3)	-4.9	(41.7)
Technical College in Region	0.095	0.303	0.190	-0.208**	(0.101)	-0.143	(0.100)
University in Region	0.143	0.331	0.095	-0.188*	(0.103)	0.048	(0.102)
Panel B: Characteristics Not Matched							
Population Per Square km	469.2	565.1	373.2	-95.9	(156.8)	96.0	(124.5)
Employment (in Thousands)	74.70	93.38	73.4	-18.68	(22.99)	1.31	(15.25)
Employment Growth (past 3 years)	0.024	0.027	0.027	-0.004	(0.015)	-0.004	(0.015)
Average Daily Wage	84.07	86.32	82.05	-2.25	(2.08)	2.02	(1.66)
Wage Growth (past 3 years)	0.051	0.048	0.050	0.002	(0.008)	0.001	(0.011)
Gross Value Added	3241	4482	3147	-1240	(1369)	95	(742)

Note: Mahalanobis matching: all matched variables for t=-1; industry shares and population also for t=-5; age and skill shares also for t=-3 and t=-5. The main data source is the BHP. Population, total employment and GVA data come from the Cambridge Econometrics' European regional Database.

Note that we do not match on outcome variables prior to the event. Instead we work with district fixed effects using a difference-in-differences approach.[16]

To rule out other confounding factors, we impose two additional restrictions. We first exclude all control districts which share a border with an event district in order to eliminate concerns that the event spills over across district boundaries to control districts. Moreover, we drop matches with the highest difference in total employment growth using a 5% trimming margin. This results in excluding two treatment districts from our analysis.

Figure 4.A.1 shows the geographic location of treatment districts and control districts in West Germany. Most college openings during our sample period occurred in Southern Germany, especially in the states of Baden-Württemberg and Bavaria. The figure also reveals that most districts with a college opening are located in more remote areas; the same is true for most of the control districts.

Overall, the matching procedure successfully eliminates differences in observable characteristics between the event and chosen control districts. The right-hand side of Table 4.1 reveals that the selected control districts are very similar to the treated districts along observable characteristics.

4.3.3 Empirical Model

Using our matched sample of regions, we then compare labor market outcomes in the event regions to those in the control regions before and after a college opening. In particular, we estimate variants of the following time-varying difference-in-differences model:

$$Y_{r\tau t} = \sum_{\tau=-5}^{-2} \beta_\tau I_\tau \cdot Treat_r + \sum_{\tau=0}^{9} \gamma_\tau I_\tau \cdot Treat_r + \theta_\tau + \delta_t + \alpha_r + \epsilon_{r\tau t} \tag{4.1}$$

where $Y_{r\tau t}$ is the labor market outcome of interest in region r in a given calendar year t for the event period τ. τ denotes the period relative to the year of the college opening, which occurs in period $\tau = 0$. We consider a period of five years before and nine years after the college opening (i.e. $-5 <= \tau <= +9$). $Treat_r$ is a binary variable equal to 1 if there is a college opening in region r and 0 for all other regions. I_τ is an indicator function equal to one in the year τ before or after the college opening and zero otherwise.

[16]The difference-in-differences method allows for time-invariant unobservable confounders but requires common trends. Matching in turn on pre-treatment outcomes does not require common trends but assumes that conditional on pre-treatment outcomes unobservable confounders are mean independent of the outcome (Lechner, 2011).

Note that t and τ differ as college openings happened in different regions in different years. Below, we report estimates for two-year increments of the event time τ.

Our parameters of interest are the γ_τ which trace the evolution of the outcome of interest in the event region between τ years after the college opening and the pre-event period ($\tau = -1$) relative to the development in the control region over the same span of years. The empirical model in (4.1) further controls for event fixed effects (θ_τ) which are measured relative to the year before the opening (i.e. $\tau = -1$). Furthermore, we include year fixed effects (δ_t) and region fixed effects α_r. Standard errors are clustered at the district level to account for the level of aggregation in the treatment variable.

The key identifying assumption is that labor market outcomes would have evolved similarly in the event and control district in the absence of a college opening. The region fixed effect allows for differences in the levels of the outcome variables. We only require that *trends* in outcome variables are comparable between event and control districts conditional on our control variables. The evolution of outcomes in the pre-event period sheds light on the plausibility of this assumption. If the identifying assumption is valid, the parameters β_τ should be close to zero and statistically insignificant. We next discuss our empirical results.

4.4 Empirical Results

4.4.1 Student Population and High-Skilled Employment

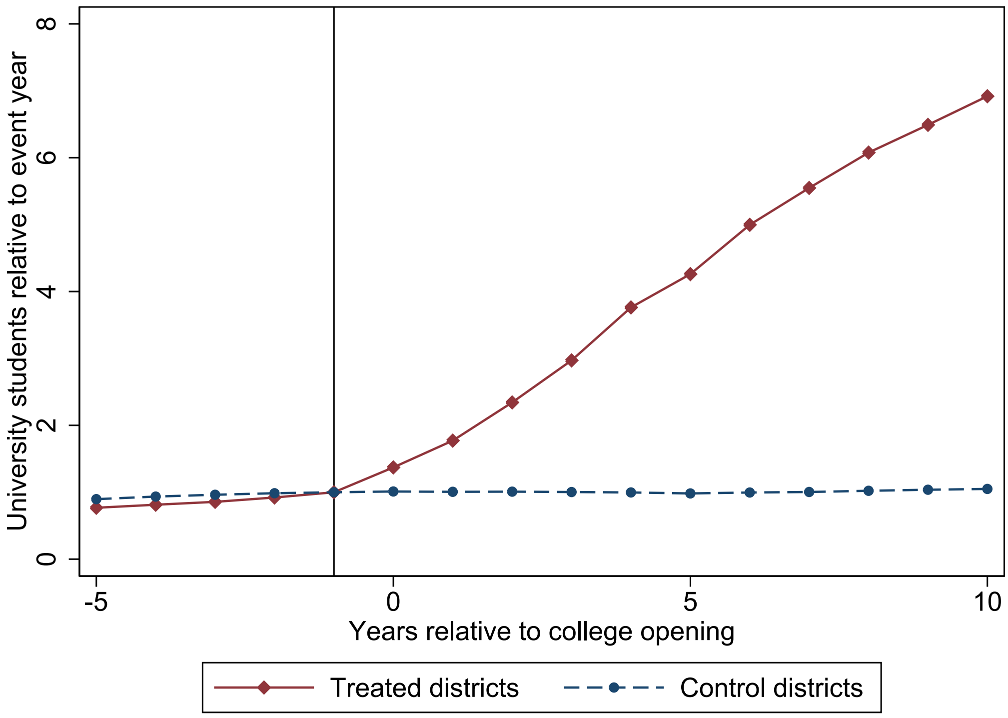
We start out by demonstrating that a college opening indeed increased the number of college students in the region. A rise in the local student population is an important indicator of the exact timing of the treatment. A rise in the student population is also a prerequisite for a positive supply shock of high-skilled labor in the region.

Figure 4.1 traces the average number of college students in treated and control districts. Prior to the college openings, there are on average about 300 university students in treatment districts and about 2,000 students in control districts.[17] We see a substantial increase in the number of students in the event region starting from the year of the college opening. Ten years after the college opening, the student population in the treatment

[17]Four districts already had another college or university before the new opening which explains the positive student population prior to the treatment we analyze.

regions has increased by about 1,600. In control regions in contrast, the student population remains roughly constant over this period.

Figure 4.1: Number of Students in Treatment and Control Districts

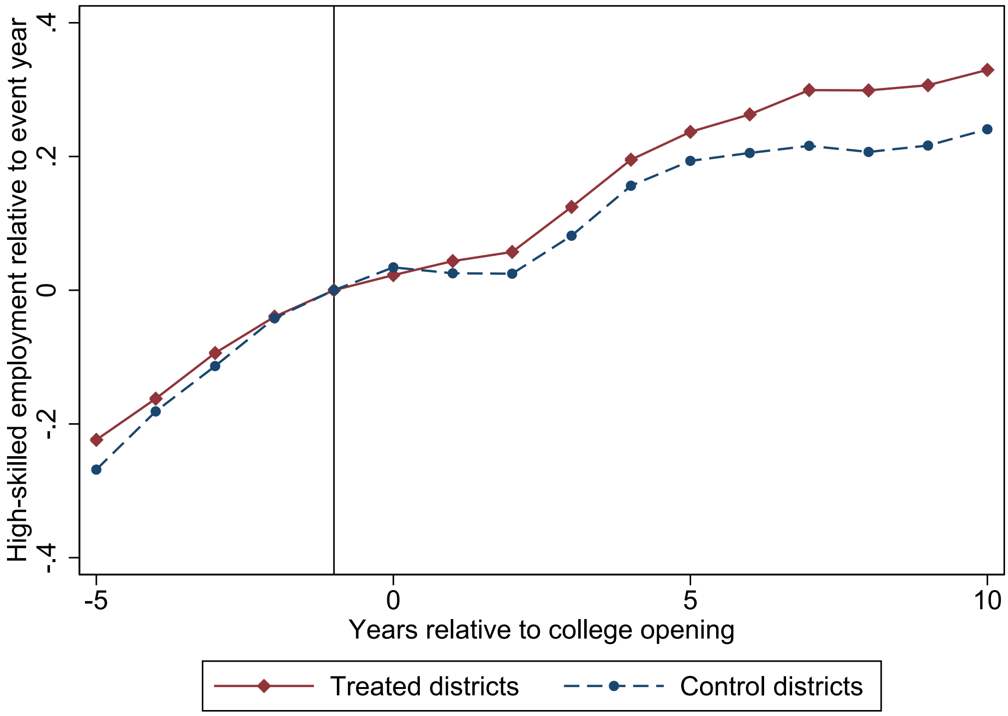


Note: Average number of full-time students enrolled in universities of treatment and control districts. The year before the university opening is normalized to 1.

We also estimate the difference-in-differences model as in equation (4.1) using the number of students relative to the number of employees in the region as the outcome variable. Column (1) of Table 4.2 shows that new technical colleges lead to a large and significant increase in the share of students: Five years after the college opening the student population increases by 1.6% of total district employment, which is about 24% of all employees with a college degree. The effect further accumulates over time. Eight to nine years after the college opening the share has increased to 2.6% of local employment, an increase in the share of high-skilled workers in the region of about 34%.

In a next step we examine whether the college opening actually induces a positive shock in high-skilled labor. If most students leave the region after they finish their college degree to work and live elsewhere, a college opening would not affect the skill structure of the local workforce considerably. In that case, we would not expect to find any sizable impact of a college opening on the local economy.

Figure 4.2: High-Skilled Employment in Treatment and Control Districts



Note: Average number of college graduates employed full-time in treatment and control districts. The year before the university opening is normalized to 0.

Figure 4.2 traces the evolution of full-time employment of high-skilled workers in event and control regions relative to the year prior to the college opening ($\tau = -1$). High-skilled employment increases slightly in treatment districts one to two years after the college opening compared to control districts. Four to five years after the college opening, when the first cohort of college graduates enters the labor market, the number of high-skilled workers grows much faster in the event regions compared to control regions. Note that there is a slight upward trend in the number of high-skilled workers in both treatment and control regions prior to the opening which reflects the substantial growth in college graduates over this period.[18] Column (2) of Table 4.2 shows that eight to nine years after the college opening local high-skilled employment has increased by about 12%.

Overall, the evidence on the student population and high-skilled employment highlight that the opening of a new college generates a sizable positive shock to the supply of high-skilled labor to the local economy. The timing of the shock to high-skilled employment in the local economy supports our identification strategy. High-skilled employment exhibits

[18]Between the mid-1980s and the 2000s, the number of students at technical colleges increased by more than 25% and hence, more than the student population in regular universities (see Beck and Wilhelm, 2003).

Table 4.2: Effects on Student Population and Employment Levels

	Share Student/Employed (1)	High-skilled Employment (2)	Total FT Employment (3)	Employment Education (4)	Employment w/o Education (5)
Period (τ=-4/-5)	0.002	0.015	0.010	0.010	0.010
	(0.001)	(0.022)	(0.013)	(0.038)	(0.013)
Period (τ=-2/-3)	0.001	0.005	0.000	-0.012	0.001
	(0.001)	(0.012)	(0.006)	(0.026)	(0.006)
Period (τ=-1)					
Period (event year, τ=+1)	0.003***	-0.003	0.006	-0.013	0.007
	(0.001)	(0.016)	(0.008)	(0.028)	(0.008)
Period (τ=+2/+3)	0.009***	0.028	0.017	-0.024	0.018
	(0.002)	(0.029)	(0.014)	(0.047)	(0.014)
Period (τ=+4/+5)	0.016***	0.031	0.023	0.057	0.023
	(0.003)	(0.028)	(0.019)	(0.070)	(0.019)
Period (τ=+6/+7)	0.021***	0.078*	0.031	0.064	0.031
	(0.004)	(0.040)	(0.026)	(0.083)	(0.026)
Period (τ=+8/+9)	0.026***	0.115**	0.041	0.120	0.040
	(0.005)	(0.054)	(0.031)	(0.085)	(0.032)
Event Period Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	714	714	714	714	714

Note: The table reports the estimates of the regression described in equation 4.1. The dependent variables are the ratio of students over total district employment (column 1), the logarithm of the number of full-time workers with a university degree (column 2), and the logarithm of full-time employment (total and by sector, columns 3-5). The unit of observation is district-year. Standard errors are clustered at the district level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

strong growth about four years after the opening when the first cohort of students graduates and enters the local labor market. If high-skilled employment had increased earlier in the region, this would have been an indication of local demand shocks affecting both high-skilled employment and the decision to open a new college in the region. Alternatively, this would raise doubts whether the observed increase might just reflect the number of high-skilled jobs created in the college with no further effect on the local economy. We now turn to the question how the college opening affected local labor markets.

4.4.2 Regional Employment

To trace the effects of a college opening on the local economy, we estimate variants of the regression model in equation (4.1) where the dependent variables are the logarithm of full-time employment in the district, log employment of medium- and low-skilled workers and the share of employment in a given skill group over total district employment. All variables refer to employment of full-time workers and are measured relative to the calendar year before the college opening in the event and control districts.

The effects on total district employment are shown in column (3) of Table 4.2. The estimates are noisy but the coefficients are positive and picture an increase in overall employment after the college opening. Eight to nine years after the reform the estimate hints at an increase of employment of 4% in treatment districts. However, the coefficient is imprecise and not statistically significant. Some of the additional employment might be jobs created by the college itself. In order to assess the effect on the local economy net of the new college jobs, we estimate equation (4.1) for the education sector and all other sectors separately.

The estimates for full-time employment in the education sector are indeed larger than for the other economic sectors but again not statistically significant (see columns (4) and (5) of Table 4.2). The absence of an effect on employment in the education sector is not too surprising given that the new technical colleges are relatively small compared to the total educational sector (including schools at all levels) in the district, especially in the first years after their opening. Moreover, given that employment by the technical colleges represents only about 0.1% of total district employment, the results on total employment do not change if we exclude the educational sector.

Table 4.3 presents the results separately by skill groups. Columns (1) and (2) suggest that the college opening does not harm employment of low- and medium-skilled workers. On the contrary, eight to nine years after the college opening employment of low- and medium-skilled workers in the region has increased by 3-4% though the estimates fail to reach statistical significance at conventional levels. The right-hand side of the table shows that the college opening encourages a skill upgrading in the local economy where the share of college graduates in total employment increases by 0.7 percentage points from a base level of 5.5% (see columns (4) to (6) of Table 4.3).

We next ask whether the additional high-skilled employment occurs in incumbent plants or in new plants. Table 4.4 shows that the share of employment in newly established plants does not increase significantly more in treatment districts than in control districts after a college opening. However, the share of high-skilled labor in new establishments relative to total high-skilled employment in the district increases significantly in the long-run. Eight to nine years after the college opening, the share of high-skilled employment in new

Table 4.3: Effects on Employment by Skill Group

	Log Employment			Employment Share		
	Low-skilled (1)	Medium-skilled (2)	High-skilled (3)	Low-skilled (4)	Medium-skilled (5)	High-skilled (6)
Period (τ=-4/-5)	0.018	0.006	0.015	0.002	-0.002	-0.000
	(0.019)	(0.016)	(0.022)	(0.003)	(0.004)	(0.001)
Period (τ=-2/-3)	-0.002	-0.001	0.005	0.000	-0.000	-0.000
	(0.013)	(0.007)	(0.012)	(0.002)	(0.002)	(0.001)
Period (τ=-1)						
Period (event year, τ=+1)	0.002	0.006	-0.003	-0.000	0.000	-0.000
	(0.015)	(0.007)	(0.016)	(0.002)	(0.002)	(0.001)
Period (τ=+2/+3)	0.029	0.016	0.028	0.001	-0.002	0.000
	(0.021)	(0.013)	(0.029)	(0.002)	(0.003)	(0.001)
Period (τ=+4/+5)	0.030	0.021	0.031	0.000	-0.002	0.002
	(0.029)	(0.018)	(0.028)	(0.003)	(0.004)	(0.002)
Period (τ=+6/+7)	0.025	0.029	0.078*	-0.002	-0.003	0.005*
	(0.039)	(0.025)	(0.040)	(0.005)	(0.006)	(0.002)
Period (τ=+8/+9)	0.034	0.037	0.115**	-0.003	-0.004	0.007**
	(0.043)	(0.031)	(0.054)	(0.006)	(0.007)	(0.003)
Event Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	714	714	714	714	714	714

Note: The table reports the estimates of the regression described in equation 4.1. The dependent variables are the logarithm of full-time employment by skill group (columns 1-3) and the ratio of full-time workers in given skill group over total full-time employment (columns 4-6). The unit of observation is district-year. Standard errors are clustered at the district level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

plants has increased by 0.5% (see column (2)). Newly established plants thus contribute to the educational upgrading of the labor force in treatment districts, possibly because new firms locating in the treatment region are more innovative firms. Yet, we find no evidence that the number of employees engaged in R&D increased in new establishments. Overall, however, most of the additional employment in the treated regions is created in incumbent plants.[19]

Taken together, the evidence on local employment indicates that a college opening facilitates skill upgrading without negative consequences for the employment of less skilled workers. Hence, the additional supply of high-skilled workers does not just replace less-skilled workers with no effect on total employment. The very moderate effects on the employment of workers without a college degree suggest modest local multiplier effects. Given the relatively small size of the new technical colleges, the absence of a sizable boost

[19]The employment effects when new establishments are excluded are very similar to the overall effects. For instance, the estimate for log high-skilled employment in a regression on incumbent establishments is equal to 0.111 compared to 0.115 from Table 4.2.

Table 4.4: Effects on Employment in New Establishments

	Employment Relative to District		Employment Structure	
	FT Employment (1)	High-skilled (2)	High-skilled Share (3)	R&D staff (4)
Period (τ=-4/-5)	0.000	0.002	0.006	-0.001
	(0.001)	(0.003)	(0.008)	(0.002)
Period (τ=-2/-3)	-0.001	0.000	0.008	0.003
	(0.002)	(0.004)	(0.010)	(0.003)
Period (τ=-1)				
Period (event year, τ=+1)	0.002	0.001	0.001	0.002
	(0.002)	(0.003)	(0.009)	(0.002)
Period (τ=+2/+3)	0.002	0.005	0.017	-0.000
	(0.002)	(0.003)	(0.013)	(0.002)
Period (τ=+4/+5)	-0.001	0.000	0.003	-0.000
	(0.001)	(0.003)	(0.013)	(0.003)
Period (τ=+6/+7)	-0.001	-0.001	0.000	-0.004
	(0.002)	(0.004)	(0.009)	(0.004)
Period (τ=+8/+9)	0.002	0.005**	0.018**	0.001
	(0.002)	(0.002)	(0.007)	(0.003)
Event Period Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes
Observations	714	714	714	714

Note: The table reports the estimates of the regression described in equation 4.1. The dependent variables are the ratio of employment in new establishments over total full-time employment (for all workers or high-skilled workers only, columns 1-2) and the ratio of full-time high-skilled workers or full-time workers in R&D occupations in new establishments over total full-time employment in new establishments (columns 3-4). The unit of observation is district-year. Standard errors are clustered at the district level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

for local goods and services is not surprising. We next ask whether local wages by skill group decrease or not in response to the positive labor supply shock to the local economy.

4.4.3 Average Wages in the Region

To investigate the effect of a college opening on local wages, we re-estimate the regression model in equation (4.1) where the outcome variable is now the log average wages of full-time workers. Table 4.5 shows that average wages in the district do not change much in response to a college opening. In particular, we do not observe any negative effect on wages of university graduates despite the increase in employment for this skill group (see column (4)). The coefficient even turns positive eight to nine years after the college opening, when treated districts experience the largest increase in high-skilled labor. Unfortunately, the data available to us do not currently allow us to distinguish the effects on high-skilled wages for different age groups. Wages typically increase with labor market experience and firm seniority. Since we expect a larger inflow of young high-skilled workers with little or no labor market experience in the district, our results could mask some positive effects on the wages of high-skilled workers in different age and experience groups. Column (2) and (3) of Table 4.5 show that the wages of low- and medium-skilled workers were largely unaffected by the college opening relative to the control region.

Table 4.5: Effects on Log Average Wages by Skill Group

	All Skill Groups (1)	Low-skilled (2)	Medium-skilled (3)	High-skilled (4)
Period (τ=-4/-5)	0.000	0.003	0.001	-0.010
	(0.004)	(0.006)	(0.004)	(0.011)
Period (τ=-2/-3)	0.000	-0.001	0.001	-0.006
	(0.003)	(0.004)	(0.002)	(0.008)
Period (τ=-1)				
Period (event year, τ=+1)	-0.002	-0.004	-0.001	-0.004
	(0.003)	(0.005)	(0.002)	(0.009)
Period (τ=+2/+3)	-0.001	-0.000	-0.001	0.001
	(0.004)	(0.007)	(0.004)	(0.012)
Period (τ=+4/+5)	0.002	0.001	-0.000	0.003
	(0.006)	(0.010)	(0.006)	(0.011)
Period (τ=+6/+7)	0.003	-0.018	-0.001	0.000
	(0.008)	(0.013)	(0.007)	(0.013)
Period (τ=+8/+9)	0.012	-0.005	0.005	0.014
	(0.010)	(0.015)	(0.009)	(0.016)
Event Period Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes
Observations	714	714	714	714

Note: The table reports the estimates of the regression described in equation 4.1. The dependent variable is the logarithm of average gross daily wages of full-time workers. The unit of observation is district-year. Standard errors are clustered at the district level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the evidence suggests that an increase in the supply of high-skilled workers does not reduce wages, which points to a positive adjustment on the labor demand side in response to college openings. We also do not find evidence for spillovers on medium-skilled and low-skilled workers. The skill upgrading without negative wage effects in the local economy indicates labor demand adjustments like changes to the output mix or to technology used in production in response to a college opening. To investigate this further and separate these two channels, we need to analyze the data at the industry- or plant-level.

4.5 Discussion and Conclusion

Exploiting the opening of new technical colleges in Germany during the 1980s and early 1990s, this paper shows that universities have a significant impact in the local economy. Our empirical strategy combines a matching procedure with a time-varying difference-in-differences approach to find suitable control regions to regions that had a college opening. We have three main findings. First, the opening of a technical college substantially increases the regional student population. This results in a large positive shock of high-skilled labor to the district when the first cohorts enter the local labor market. We find that high-skilled employment in the region increases by 12% eight to nine years after the opening. Second, we find some evidence of a positive, albeit imprecisely estimated, effect on the employment of workers without a college degree. Third, we find that wages of high-skilled workers do not decrease in the medium-run indicating a shift in local demand, especially for high-skilled workers. The employment and wage effects remain if we exclude all employees working in education, suggesting that a college opening has an impact on the local economy beyond additional jobs in teaching and research. The large increase in employment and the lack of a drop in wages point to sizable adjustments on the labor demand side. We find no large increase in employment at new establishments, suggesting that most of the adjustments happen in incumbent firms either through changes in the output mix or through changes in the technology used in the production process.

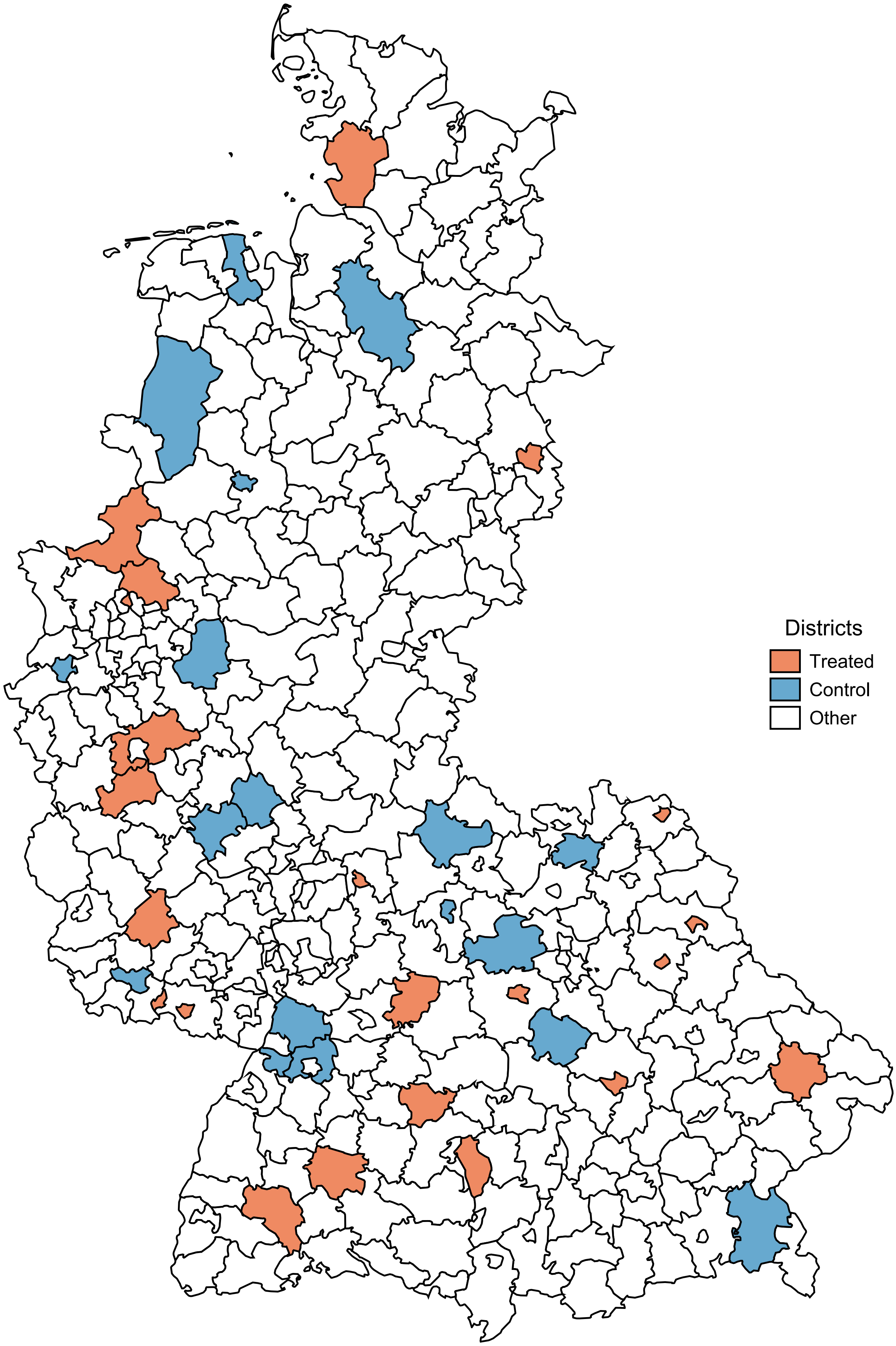
While our analysis focuses on the short- to medium-run effects of a college opening, the insights gained have important implications for regional policy. First, opening a new college is indeed an effective strategy to increase the skill level of the regional workforce. Second, we do not find evidence of adjustment costs in terms of lower wages or employment of such a large shock in high-skilled labor.

4.A Further Tables and Figures

Table 4.A.1: List of Treatment Colleges

College	City	District	Opening Year
FH Braunschweig-Wolfenbüttel	Wolfsburg	Wolfsburg	1988
Hochschule Esslingen, Hochschule Nürtingen-Geislingen	Göppingen, Gesilingen	Göppingen	1988
Hochschule Heilbronn	Künzelsau	Hohenlohekreis	1988
FH Furtwangen	Villingen-Schwenningen	Schwarzwald-Baar-Kreis	1988
Hochschule Albstadt-Sigmaringen	Albstadt-Ebingen	Zollernalbkreis	1988
Westfälische Hochschule Gelsenkirchen	Bocholt	Borken	1992
FH Westküste	Heide	Dithmarschen	1993
Hochschule Kaiserslautern	Zweibrücken	Zweibrücken	1994
Technische Hochschule Ingolstadt	Ingolstadt	Ingolstadt	1994
Technische Hochschule Deggendorf	Deggendorf	Deggendorf	1994
Hochschule Hof	Hof	Hof	1994
FH Neu-Ulm	Neu-Ulm	Neu-Ulm	1994
Hochschule Bonn-Rhein-Sieg	Sankt Augustin	Rhein-Sieg-Kreis	1995
Westfälische Hochschule	Recklinghausen	Recklinghausen	1995
Ostbayerische Technische Hochschule Amberg-Weiden	Amberg	Amberg	1995
Ostbayerische Technische Hochschule Amberg-Weiden	Weiden	Weiden	1995
FH Aschaffenburg	Aschaffenburg	Aschaffenburg	1995
Hochschule Trier	Birkenfeld	Birkenfeld	1996
Hochschule für angewandte Wissenschaften Ansbach	Ansbach	Ansbach	1996
Hochschule Koblenz	Remagen	Ahrweiler	1998
Hochschule Kaiserslautern	Pirmasens	Pirmasens	1988

Figure 4.A.1: Geographic Location of Treatment and Control Districts



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